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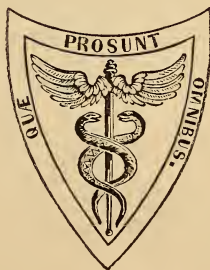
EDITED BY

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FELLOW OF THE PHILADELPHIA COLLEGE OF PHYSICIANS; MEMBER OF THE
AMERICAN MEDICAL ASSOCIATION; OF THE AMERICAN PHILOSOPHICAL SOCIETY; OF THE
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TO READERS AND CORRESPONDENTS.

The following works have been received :—

Medico-Chirurgical Transactions. Published by the Royal Medical and Chirurgical Society of London. Vol. XLIII. London, 1860. (From the Society.)

The Reparative Process in Human Tendons after subcutaneous division for the cure of Deformities; with an account of the appearances presented in fifteen post-mortem examinations in the human subject; also a series of experiments on rabbits, and a résumé of the English and Foreign Literature of the subject. Illustrated by seven lithograph plates and a series of wood-cuts. By WILLIAM ADAMS, F. R. C. S., Surgeon to the Royal Orthopædic and Great Northern Hospitals, &c. &c. London: John Churchill, 1860. (From the Author.)

On Diseases Peculiar to Women, including Displacements of the Uterus. By HUGH L. HODGE, M. D., Prof. Obstetrics and Diseases of Women and Children in the University of Pennsylvania. With original illustrations. Philadelphia: Blanchard & Lea, 1860. (From the Publishers.)

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An Address delivered before the American Medical Association at its thirteenth annual meeting, held in the city of New Haven, Conn., June, 1860. By HENRY MILLER, M. D., President.

Introductory Discourse on the Speculative and Inductive Medicine, delivered in the Medical Department of Pennsylvania College, Oct. 3, 1860. By HENRY HARTSHORNE, M. D., Prof. Theory and of Pract. Med. Philadelphia, 1860.

Report of the Medical Superintendent of the Provincial Lunatic Asylum, Toronto, for the year 1859. Toronto, 1860.

Observations upon the form of the Occiput in the Various Races of Men. By J. AITKEN MEIGS, M. D., Prof. Inst. Med. in Medical Department of Pennsylvania College. (From the Author.)

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The following Journals have been received in exchange:—

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Journal de Médecine de Bordeaux. Rédacteur en chef, M. COSTES. September, October, 1860.

Le Moniteur des Sciences Médicales et Pharmaceutiques. Rédacteur en chef, M. H. de CASTELNAU. September, October, November, 1860.

Journal de la Physiologie de l'Homme et des Animaux. Publié sous la direction du Docteur E. BROWN-SÉQUARD. July, 1860.

- Annales Médico-Psychologiques. Redégé par MM. les Docteurs BAILLARGER, CERISE, et MOREAU (de Tours). October, 1860.
- Medical Times and Gazette. August, September, October, November, 1860.
- Dublin Medical Press. August, September, October, November, 1860.
- British Medical Journal. Edited by ANDREW WYNTER, M. D. September, October, November, December, 1860.
- Edinburgh Medical Journal. September, October, November, December, 1860.
- The Glasgow Medical Journal. July, October, 1860.
- The London Medical Review. September, October, November, December, 1860.
- Guy's Hospital Reports. Edited by SAMUEL WILKS, M. D., and ALFRED POLAND. Third series, vol. vi.
- British and Foreign Medico-Chirurgical Review. October, 1860.
- The Journal of Psychological Medicine. Edited by FORBES WINSLOW, M. D., D. C. S. October, 1860.
- The Dublin Quarterly Journal of Medical Science. May, August, November, 1860.
- The British American Journal. Edited by ARCHIBALD HALL, M. D. October, November, December, 1860.
- The Jamaica Quarterly Journal of Medicine, Science, and Arts. Edited by H. CROSKERY, L. R. C. S. J. July, 1860.
- The Boston Medical and Surgical Journal. Edited by F. E. OLIVER, M. D., and CALVIN ELLIS, M. D. October, November, December, 1860.
- American Medical Times. October, November, December, 1860.
- The Maryland and Virginia Medical Journal. Edited by J. B. McCaw, M. D., and W. C. VAN BIBBER, M. D. October, November, December, 1860.
- The Southern Medical and Surgical Journal. Edited by HENRY F. CAMPBELL, M. D., and ROBERT CAMPBELL, M. D. October, November, December, 1860.
- The Proceedings of the Academy of Natural Sciences of Philadelphia. September, October, 1860.
- The North American Medico-Chirurgical Review. Edited by S. D. GROSS, M. D., T. G. RICHARDSON, M. D., and S. W. GROSS, M. D. November, 1860.
- Charleston Medical Journal and Review. Edited by J. DICKSON BRUNS, M. D. November, 1860.
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- The American Journal of Insanity. Edited by the Medical Officers of the New York State Lunatic Asylum. October, 1860.
- The Cincinnati Lancet and Observer. Edited by Drs. E. B. STEVENS, J. A. MURPHY, and G. C. E. WEBER. October, November, December, 1860.
- The Medical and Surgical Reporter. Edited by S. W. BUTLER, M. D., and R. J. LEVIS, M. D. October, November, December, 1860.
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- The Pacific Medical and Surgical Journal. Edited by DAVID WOOSTER, M. D. August, 1860.
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- The Medical Journal of North Carolina. Edited by EDWARD WARREN, M. D. August, October, 1860.
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- The Chicago Medical Examiner. Edited by N. S. DAVIS, M. D., and E. A. STEELE, M. D. November, December, 1860.
- St. Louis Medical and Surgical Journal. Edited by M. L. LINTON, M. D., and W. M. MCPHEETERS, M. D. November, 1860.
- The American Journal of Science and the Arts. Edited by Profs. B. SILLIMAN, B. SILLIMAN, JR., and JAMES D. DANA. November, 1860.

Ohio Medical and Surgical Journal. Edited by JOHN DAWSON, M. D., and J. W. HAMILTON, M. D. September, November, 1860.

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Cleveland Medical Gazette. Edited by Drs. C. E. WEBER, E. B. STEVENS, and J. A. MURPHY. October, November, December, 1860.

New Orleans Medical News and Hospital Gazette. Edited by D. W. BRICKELL, M. D., and E. D. FENNER, M. D. October, November, December, 1860.

Nashville Journal of Medicine and Surgery. Edited by W. K. BOWLING, M. D. October, November, December, 1860.

Atlanta Medical and Surgical Journal. Edited by J. G. WESTMORELAND, M. D. October, November, December, 1860.

The Journal of Materia Medica. Edited by Jos. BATES, M. D., and H. A. TILDEN. October, November, 1860.

Oglethorpe Medical and Surgical Journal. Edited by Drs. H. L. BYRD, and W. HAUSER. October, 1860.

The Cincinnati Medical and Surgical News. Edited by A. H. BAKER, M. D. September, October, November, 1860.

American Medical Monthly. Edited by J. H. DOUGLAS, M. D., and AUSTIN FLINT, Jr., M. D. October, November, December, 1860.

The Georgia Medical and Surgical Encyclopædia. Edited by H. N. HOLLI-FIELD, M. D., and T. W. NEWSOME, M. D. September, October, 1860.

The Saint Joseph Medical and Surgical Journal. Edited by G. C. CATLETT, M. D., and J. B. SNELSON, M. D. November, 1860.

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The American Journal of Pharmacy. Published by authority of the Philadelphia College of Pharmacy. Edited by WM. PROCTER, Jr. November, 1860.

American Druggists' Circular and Chemical Gazette. October, November, December, 1860.

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
The New York Dental Journal. Edited by FRANK H. NORTON. Oct., 1860.

The American Journal of Dental Science. Edited by CHAPIN A. HARRIS, M. D., and A. SNOWDEN PIGGOT, M. D. October, 1860.

Communications intended for publication, and Books for Review, should be sent, *free of expense*, directed to ISAAC HAYS, M. D., Editor of the American Journal of the Medical Sciences, care of Messrs. Blanchard & Lea, Philadelphia. Parcels directed as above, and (carriage paid) under cover, to John Miller, Henrietta Street, Covent Garden, *London*; or M. Hector Bossange, Lib. quai Voltaire, No. 11, *Paris*, will reach us safely and without delay. We particularly request the attention of our foreign correspondents to the above, as we are often subjected to unnecessary expense for postage and carriage.

Private communications to the Editor, may be addressed to his residence, 1525 Locust Street.

ALL REMITTANCES OF MONEY, and letters on the *business* of the Journal, should be addressed *exclusively* to the publishers, Messrs. Blanchard & Lea.

 The advertisement-sheet belongs to the business department of the Journal and all communications for it should be made to the publishers.

To secure insertion, all advertisements should be received by the 20th of the previous month.

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- XI. On Diseases peculiar to Women, including Displacements of the Uterus. By Hugh L. Hodge, M. D., Professor of Obstetrics and Diseases of Women and Children in the University of Pennsylvania. "Nullius addictus jurare in verba magistri. With original illustrations. 8vo. pp. 442. Philadelphia: Blanchard & Lea, 1860. 175

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- XII. An Elementary Treatise on Human Anatomy. By Joseph Leidy, M. D., Professor of Anatomy in the University of Pennsylvania, &c. &c. With three hundred and ninety-two illustrations, 8vo. pp. 663. Philadelphia : J. B. Lippincott & Co., 1861. 197
- XIII. De la Nature et du Traitement du Croup et des Angines couenneuses, étude clinique et microscopique, démontrant : 1. Que les concrétions, source de tous les accidents, sont des produits d'origine parasitaire ou moisissures. 2. Que la base du traitement repose sur l'application de topiques parasitiques, médication aussi rationnelle qu'heureuse en pratique. Par le Dr. N. Jodin, Médecin du 9^e Bureau de Bienfaisance de Paris, Chevalier de la Légion d'Honneur. Paris, 1859, 8vo. pp. 39. (Adrien Delahaye, Libraire-éditeur.)
- A Clinical and Microscopical Study of the Nature and Treatment of Croup and of Pseudo-Membranous Angina, showing : 1. That the concretions, the source of all the symptoms, are products of parasitic or vegetable origin. 2. The basis of Treatment consists in the application of topical parasitides, a medication as rational as it is happy in practice. By Dr. N. Jodin, &c. Paris, 1859. 8vo. pp. 39. 200

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1. Of the New Hampshire Asylum, for the fiscal year 1859–60.
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XVII. Memoranda Medica, or Note Book of Medical Principles. Being a Concise Syllabus of Etiology, Semeiology, General Pathology, Nosology, and General Therapeutics ; with a Glossary for the Use of Students. By Henry Hartshorne, A. M., M. D., Professor of Theory and Practice of Medicine in the Medical Department of Pennsylvania College, etc. etc. *Ὠφελειν ἢ, μὴ βλάπτειν*, "To heal, or, not to harm." 12mo. pp. 190. Philadelphia, 1860, J. B. Lippincott & Co. 227

XVIII. A Treatise on Medical Jurisprudence. By Francis Wharton and Moreton Stillé, M. D. The medical part revised and corrected with numerous additions. By Alfred Stillé, M. D. Second and revised edition. Philadelphia : Kay & Brother, 1860. 8vo. pp. 1031. 228

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ART. I.—*Clinical Report on Pneumonia, based on an analysis of one hundred and thirty-three Cases.* By AUSTIN FLINT, M. D., Professor of Clinical Medicine and Pathology in the New Orleans School of Medicine, and in the Long Island College Hospital.

MY clinical records, for the last twelve years, embrace one hundred and thirty-three cases of pneumonia, exclusive of cases in which the patients were under five years of age. I propose to analyze this collection of cases with reference to certain points of inquiry. I shall not undertake to examine and compare the histories, as regards the symptomatic phenomena which belong to the disease. The researches of Grisolle have rendered this labour, in a measure, uncalled for; at all events, the same amount of labour might be more profitably bestowed upon other diseases, which, as yet, have been very imperfectly studied by means of the analytical method. Nor shall I direct my inquiries to the physical signs of this disease. The physical signs, in a large proportion of the cases, were recorded with much minuteness; and with special reference to these, a considerable number of the histories have been already analyzed, in the preparation of a work on physical exploration.¹ Moreover, the analysis of so large a collection of cases, with reference to the symptoms and signs, would extend this paper beyond the limits to which it must be restricted. My plan is to interrogate the histories, with reference to the laws of the disease, as regards the ages, occupations, and habits of the patients, the circumstances connected with its causation, the situation and extent of the local affection, the com-

¹ Physical Exploration, and the Diagnosis of Diseases affecting the Respiratory System. Blanchard & Lea, 1856.

plications of the disease, its intrinsic tendency to death or recovery, etc. ; and to endeavour to develop information bearing on the indications for treatment. A large number of the cases were observed in a northern city, viz. Buffalo, N. Y. ; and a large number were observed in a southern city, viz. New Orleans. A few of the cases were observed at an intermediate point, viz. Louisville, Ky. Does the disease present marked variations, as regards its important laws, in these widely separated situations ? Are the indications for treatment similar in New Orleans, Buffalo, and Louisville, or does the treatment require to be modified in places which represent the northern, southern, and middle portions of our country ? These are interesting questions, and important also in a practical point of view. I shall be glad, if the results of my analysis should prove of any value, as data upon which the answers are to be based. It is plain, that the questions can be answered only by comparing the histories of cases recorded in the different situations. Mere speculation, or general impressions, are of little value, and are as likely to lead to error as to truth. Irrespective of modifications which may be derived from climate, the treatment of pneumonia has of late been the subject of much discussion, and is invested with an importance greater than belongs to the disease itself, inasmuch as it involves the principles which are to govern the treatment of acute inflammations in general.

Preparatory to the analysis, I have made abstracts of the histories of all the cases. These abstracts are before me, and I shall proceed to interrogate them. They are arranged in four classes, viz : 1st. *Forty-nine* cases observed in the Buffalo Hospital of the Sisters of Charity, from August, 1848, to June, 1859 ; 2d. *Fifty-three* cases observed in the New Orleans Charity Hospital, during the winters of 1858-9 and 1859-60 ; 3d. *Eleven* cases observed in the Louisville Marine Hospital, during the winters of 1853-4 and 1855-6 ; 4th. *Twenty* cases observed in private practice, from January, 1851, to May, 1860. Nearly all the cases included in class 4, were observed at Buffalo. With respect to certain points of inquiry, I shall institute comparisons between the cases belonging to these four classes.

The propriety of at least two subdivisions is obvious. Cases of simple pneumonia are to be separated from the cases in which this disease is complicated with other serious affections. With respect to some points of inquiry, these two classes of cases are to be interrogated separately. It is plain, that a collection of cases, in which the disease is uncomplicated, can alone furnish the data for determining the natural history of the disease, its intrinsic tendencies, the principles of treatment which belong to it *per se*, etc. An analysis of a collection, in which complicated and uncomplicated cases were mingled together, would furnish results leading to erroneous practical conclusions. The propriety of studying fatal and non-fatal cases separately, and making comparisons with regard to certain points, is apparent. This will be the ground of another subdivision.

The points of inquiry, to which attention will be first directed, relate to age, sex, occupation, habits, other circumstances connected with the causation of the disease, and the comparative frequency of the disease in the different situations in which the cases were observed.

Analysis with reference to Age, Sex, Occupation, Habits, Causation, and the comparative frequency of Cases of the Disease, in New Orleans, Louisville, and Buffalo.

Age.—The ages were noted in 121 cases. In three cases, the ages were under 10 and over 5 years. Two of these three cases were in private practice. Few patients were received into hospital under the age of ten years, and as all but twenty were hospital cases, it is evident, that in this collection, the proportion of cases in which the disease occurs under ten years of age is not fairly represented. The same remark is measurably applicable to the cases in which the ages were between 10 and 20 years. The proportion of hospital patients, between these ages, is much less than of those over 20 ; so that the liability to the disease, during that period of life, is not fairly represented by the number of patients over 10 and under 20, in this collection. The number is 13. The number of cases in which the ages were between 20 and 30 years is the largest, viz. 44. The number between the ages of 30 and 40 years is 37. The number between 40 and 50 years, 17 ; and between 50 and 60 years, 7. There was not a single instance of a patient over 60. Making due allowance for the periods of life, in which persons are most likely to resort to hospitals, and to the relative number of living persons of different ages, the inference to be drawn from these statistics, as to the existence of a predisposition to pneumonia, peculiar to any of the foregoing decades, is negative. The liability to the disease, in childhood, adolescence, early manhood, middle life, and old age, is probably not very far from uniform ; at all events, the number of cases occurring at different ages is sufficient to show, that no period of life is exempt from it. The comparatively small number of persons over 60 living, and the few persons, so old as this, received into hospitals, will account for there being no patients over that age. The statistics of others show that pneumonia occurs after 60.¹ I do not embrace in my collection cases under five years of age. This is not because I have not the records of such cases. Lobar pneumonia occurs in infancy, as well as the so-called lobular pneumonia ; the propriety of considering the latter as a variety of pneumonia having been rendered more than doubtful by late researches, I have excluded cases in which the ages were under five years, in order to avoid discussion of a mooted question in pathology.

Sex.—My service at the Charity Hospital, in New Orleans, embraces male wards only ; but in the hospitals at Buffalo and Louisville I had charge of both male and female wards. Of the 80 cases observed in the

¹ *Traité pratique de la Pneumonie, etc.* Par A. Grisolle. Paris, 1841, p. 100.

latter hospitals and in private practice, 10 were females. This illustrates the much greater frequency of pneumonia in the male than in the female sex.

Occupation.—The occupation was noted in 87 cases, exclusive of the female patients. In 52 of these cases, the patients were labourers. In the remaining 35 cases, a variety of occupations were represented. It is important to enumerate them with reference to a general conclusion: Boatmen or seamen, 5; blacksmiths, 3; medical students, 3; gardeners, 9; clerks, 2; machinists, 3. Seventeen different callings had but a single representative, as follows: Sign painter, common painter, barkeeper, steamboat pilot, baker, steward, last-maker, draper, peddler, joiner, moulder, storekeeper, male nurse, milkman, farmer, plasterer, and merchant.

The calling of a labourer includes a great variety of out-door work. Muscular exertion and exposure to the weather are its distinctive characteristics. A large proportion of male patients received into hospitals with all diseases, are labourers; but if these were in any measure rendered exempt from pneumonia by their calling, they would not constitute so large a proportion of the patients affected with the disease. In fact, the large proportion (50 out of 87) must be considered as showing that, as a class, they are particularly prone to the disease. The occupations in the remainder of the cases are generally of a kind to involve, more or less, exposure to the weather. Various sedentary occupations, and those which confine within doors, such as shoemaking, tailoring, etc., are not represented. This would not be so, if such occupations favoured a predisposition to the disease. An analysis of these cases leads to the conclusion that persons are most liable to pneumonia who are exposed to the vicissitudes of the weather, and, *per contra*, that in-door occupations afford, in a measure, security against the disease.

Habits.—The histories of many of the cases are defective as regards intemperance, and nearly all are deficient in an account of habits in other respects. In 37 cases, it is noted that the patients were either temperate or intemperate, or addicted to moderate drinking. Of these 37 cases, in only 6 were the patients temperate; in 25 they confessed that they were intemperate; and in 6 they were moderate drinkers. In 10 cases the attack of pneumonia followed excessive drinking, or a debauch.

Allowance is to be made for the fact, that a large majority of male patients with all diseases in the hospitals where my cases were chiefly collected, are either intemperate or moderate drinkers, and much oftener the former than the latter. But we are warranted, in concluding from the very small numbers of patients with pneumonia, who asserted that they were temperate, that the abuse of alcohol leads to the development of the disease. This conclusion is strengthened by the fact, that, in nearly one-quarter of the cases analyzed with reference to this point, the disease was preceded by excessive indulgence. It is a question, however, whether the

abuse of alcohol acts directly as a cause, or whether the development of the disease may not be due, in a great measure, to circumstances incidental to intemperance, such as exposure to cold, &c.

Causation.—The previous points of inquiry, relating to age, sex, habits, and occupation, in fact, have reference to the causation of the disease. Under the present caption I wish to include other circumstances connected with the development of the disease, which may be supposed to stand in a causative relation to it. I shall, therefore, interrogate the histories with regard to any affections which may have preceded or accompanied the attack of pneumonia, and with regard to any unusual exposure or injury to which the disease may be attributable. But, with respect to particular circumstances bearing on the causation, many of the histories are defective. This arose often from an indisposition to push interrogatories into much detail in consequence of the suffering and weakness of the patients; in other cases, from the difficulty of obtaining clear and reliable statements from hospital patients, and, finally, from want of time and patience on the part of the recorder.

The connection of pneumonia with periodical fever is the first point of inquiry which suggests itself. In each of the hospitals in which the greater part of the cases in this collection were observed, a large proportion of the patients, received with different diseases, had been exposed to malaria, and cases of intermittent fever were abundant. In 13 cases the histories state that the patients with pneumonia had been more or less affected previously with intermittent fever. I presume that the number was considerably larger than this, pains not being taken to note the fact in all the histories. Of these 13 cases, in 3 it is simply stated that the patients were subject to intermittent; in 1 case the patient had suffered much of the time with intermittent fever in the preceding ten months; in 1 case the patient had recovered from an attack three weeks before, and in 1 case a week before; in 1 case the interval was some months; in 5 cases the development of the pneumonia was immediately preceded by the intermittent; but in 1 case only was the patient actually suffering from intermittent fever, when the pneumonia became developed. In no case did a relapse of intermittent occur while the pneumonia was in progress.

These facts certainly do not go to show any pathological connection between pneumonia and periodical fever. It may fairly be doubted, whether persons subject to intermittent fever, are thereby more prone to pneumonia. It is even probable, that a person, during the continuance of the paroxysms of an intermittent fever, is less likely to be attacked by pneumonia than after the paroxysms are interrupted, since, in several instances, the pneumonia followed closely on the heels of an intermittent fever, and in only one case was the pneumonia developed while the intermittent fever continued. Nor does this analysis present grounds for supposing that pneumonia tends to reproduce an attack of intermittent fever in persons subject

to relapses of the latter disease, since in not a single instance in the present collection of cases, did this occur.

That intermittent fever and pneumonia may coexist is certain. I have observed these two affections combined. We can understand that, at certain times and places, both diseases being prevalent, the combination may not unfrequently occur, and the most rational view is, when this is observed, that neither predisposes to the other, but that the coexistence is due merely to the coincidence of the causes proper to each.

Do other pulmonary affections involve a predisposition to pneumonia? In 13 cases the pneumonia was preceded by some other pulmonary affection. I presume that these cases embrace nearly all in which this was the fact. In a few of these cases (or, to be explicit, in 4) the attack was preceded simply by cough and other symptoms denoting only bronchitis. In 3 cases a previous attack of pneumonia had occurred within a short period. In one of these 3 cases the patient had recovered from pneumonia, and been discharged, thirteen days prior to his returning with the second attack; and the inflammation in the second attack was seated in a different lobe (the upper) of the lung opposite to that affected in the first attack; this case proved fatal. In another case the upper lobe was affected in the second attack, and the lower lobe of the same lung in the first attack; the interval between the two attacks is not precisely noted, but it was short. In the remaining case the patient had pneumonia two months before the second attack, the whole right lung having been affected, and in the second attack the lower lobe of the same lung was affected; this patient recovered. In 7 cases the patients were affected with pulmonary tuberculosis. The pneumonia in one of these cases was seated in the lower lobe of the left lung, while the deposit of tubercle was chiefly at the summit of the right lung. In one case the pneumonia was seated in the upper lobe in which the deposit of tubercle was seated. In the other cases, save one, the existence of tubercle is predicated on long continued cough, hæmoptysis, loss of weight, etc., the physical signs not been noted in the histories. In one case the lungs after death were found crammed with miliary tubercles. In all the cases, save this one, the patients recovered from the pneumonia.

These are all the facts relating to antecedent pulmonary disease. They show that bronchitis does not tend to eventuate in pneumonia. They show that after a patient has fairly recovered from an attack of pneumonia, there is not much liability to a speedy recurrence of the disease. They show that patients affected with pulmonary tuberculosis are not particularly prone to an attack of pneumonia; and that when it does supervene, it may or may not affect the upper lobe in which the deposit of tubercle is most abundant, and that the coexistence of tubercle and pneumonia does not render the disease fatal. Finally, the absence of other pulmonary affections, such as emphysema, chronic pleurisy, asthma, shows that these affections do not

involve a predisposition to pneumonia, but, on the other hand, it is probable they may afford protection against the disease.

Delirium tremens preceded the disease in 3 cases. In one case the patient had been discharged ten days before being readmitted with pneumonia, and in another case but two days. In the remaining case the patient entered with delirium tremens, and the pneumonia was subsequently developed. As will be seen in another connection, delirium tremens was a complication of the pneumonia in a number of instances.

In two cases the patients were affected with organic disease of the heart. In one of these cases death occurred during the first stage of the pneumonia; the other patient recovered.

The affections which preceded the pneumonia in other cases were, conjunctivitis in 3 cases; dysentery in 1 case; diarrhœa in 1 case; ecthyma and subacute laryngitis in 1 case.

In 3 cases the pneumonia was traumatic, following, in 2 cases, a severe injury to the chest, the ribs being fractured in 1 case; and in the remaining case, following the passage of a pistol ball through the chest.

The analysis of these cases thus affords no evidence of the dependence of pneumonia on any antecedent affections seated in the lungs or elsewhere. It is to be borne in mind, however, that cases are not included in this collection in which the pneumonia was developed in the course of continued or other fevers.¹ It is well known that it occurs not infrequently as a complication of typhus and typhoid fever. The cases which I have recorded of these diseases would furnish numerous examples. It is to be remarked that in none of the cases in this collection do the histories afford evidence of the existence of degenerative disease of the kidney or Bright's disease. In one case the patient had had acute albuminuria two years before, from which he had recovered; and in another case albuminuria was developed several months afterward. These are the only instances in which there appears any ground for supposing that there may have been a connection between disease of the kidney and the development of the pneumonia. And during the period that these cases were observed, forty or fifty cases, at least, of Bright's disease were recorded. So far as my experience goes, therefore, pneumonia is not to be reckoned among the consecutive affections dependent on disease of the kidney.

In 9 cases the attack was attributed, by the patients, to some unusual exposure, such as working in the cold and wet, and sleeping out of doors at night. But in the majority of cases it could not be traced to any obvious exciting cause. The attack in most of the cases was sudden, the first event being a chill, which was soon followed by pain in the side, febrile

¹ Inadvertently, two cases at New Orleans are included in which the disease was developed in the course of typhoid fever; with this exception the statement holds good that such cases were excluded.

movement, etc. These symptoms usually marked the date of the attack, but sometimes it was not easy to determine precisely when the disease commenced. This collection of cases, also, furnishes a number of examples of pneumonia latent, as regards the symptoms which generally characterize the disease, so that the diagnosis would not have been practicable without the aid of physical signs.

Comparative frequency of Cases of the Disease at New Orleans, Louisville and Buffalo.—As bearing on this point of inquiry, the number of cases of the disease in the three places named, are to be considered relatively to the period in which they were collected, and the number of patients with all diseases under observation. My period of service at the Buffalo Hospital commenced in August, 1848, and continued uninterruptedly for a year and a half. The hospital was then small, the number of medical cases under my care averaging from thirty to forty. Afterwards, for three consecutive years, my service was for six months, commencing in October and ending with March. The hospital had then increased so that my cases averaged from fifty to sixty. My next service was from April to October, in 1855. Next, from April, 1856, to October, 1858. Finally, from April to October, 1858, and from April to June, 1859. The aggregate number of months is 68, or five years and eight months; the average number of patients during the whole of this period being, at least, forty. The number of cases of pneumonia during this period was forty-nine, exclusive of the cases in which the disease was developed in the course of continued fever. It is thus evident, that in the climate of Buffalo, pneumonia is not a disease of very frequent occurrence.

At Louisville, I had charge of the medical wards from October to March, in 1853-4, and in 1855-6, making in all, eight months. During this period, eleven cases of pneumonia came under observation. Without having the data to be precise, the number of medical patients in the hospital during this period averaged about forty. Thus it is evident that pneumonia occurs more frequently at Louisville than at Buffalo, since in less than one-eighth of the period of my connection with the hospital at the latter place, the number of cases collected were less in the proportion only of between one-fourth and one-fifth. Moreover, my service at Louisville did not extend over the months when probably cases of pneumonia are most likely to occur, viz., in March and April.

At New Orleans, my service in 1858-9 was for three months, commencing in the middle of November, and ending in the middle of February. In 1859-60, it was nearly five months, from November 1st, to the latter part of March; in all, a little under eight months. During this period, I collected fifty-three cases. The number of patients under my charge in this hospital averaged about forty. It is thus seen that pneumonia is a disease of much more frequent occurrence in New Orleans than in either Buffalo or Louisville, the number of cases in a little less than eight months exceeding

the number observed in the former of these two places during a period of service extending over five years and eight months; the average number of hospital patients in the three cities during the whole time of service being about the same.

Season.—My hospital cases at Buffalo alone admit of analysis with respect to the occurrence of cases in all the different months of the year, since at Louisville and New Orleans my service was in the winter months only. The forty-nine cases at the Buffalo hospital were distributed among the different months, as follows: January, 7; February, 2; March, 13; April, 9; May, 3; June, 3; July, 3; August, *none*; September, 2; October, 1; November, 1; December, 5.

Fifty-two cases at the New Orleans Charity Hospital occurring between November and March, these two months included, were distributed as follows: January, 11; February, 5; March, 6; November, 17; December, 53. This disparity may be in a measure owing to my service having included only half of November, half of February, and no part of March on one of the two years.

The eleven cases at the Louisville Hospital, occurring in the months between October and March of the years, were thus distributed: January, 8; February, *none*; December, *none*; November, 1; October, 2.

The conclusions to be drawn from the results of an analysis with reference to the points just considered, may be summed up as follows:—

Pneumonia is a disease which has no predilections for any particular periods of life. It affects the male much oftener than the female sex. It is not produced directly by any particular occupation, but occurs chiefly among persons whose callings involve muscular exertion and exposure to the weather. Habits of intemperance render a person more liable to the disease, perhaps in consequence of exposure and other circumstances incident to these habits. It has no pathological connection with periodical fever. Other pulmonary affections do not predispose to it, and some, *e. g.*, asthma, emphysema, and chronic pleurisy, appear to afford a protection against it. It occurs, but not frequently, in persons affected with organic disease of heart. It may be produced traumatically, by external injury, or the passage of a pistol ball through the chest. It is not one of the secondary affections referable to degenerative disease of the kidney. It may not infrequently be traced to some exciting cause, such as unusual exposure to cold or wet, but it oftener occurs when no exciting cause is apparent. It is oftener developed abruptly than in a gradual manner. In respect of frequency, cases are not as numerous at Buffalo as at Louisville, and they are by far more frequent at New Orleans than in the other two places. At Buffalo cases occur in the months of January, March, and April, much more frequently than at other parts of the year; at New Orleans cases were more numerous in November and December than in any other of the

winter months, and of the comparatively few cases observed at Louisville, three-fourths occurred in January.

In view of the much more frequent occurrence of the disease in some parts of the country than in other parts, its greater prevalence at certain periods of the year than at other seasons, and the fact that it attacks especially those who are exposed to the weather, it is reasonable to conclude that the morbid agency, or agencies, determining pneumonia are exterior to the body. Another fact leading to this conclusion, is the much greater prevalence of the disease in some years than in others. This is a matter of common observation. I was led to observe in the New Orleans Charity Hospital, that cases of the disease came in, as it were, in successive crops, that is, cases would be received for several days in succession, and then would follow an interval of several days when few or no cases were admitted. The dates of the admissions of cases into my ward show this on a small scale, but the dates of all the admissions into the hospital, were they at hand, would, I doubt not, exhibit this fact in a striking manner. It would be interesting to see if these irruptions of pneumonia are connected with any uniform meteorological changes, but the data for such a comparison are not at present available. Still another fact leading to the conclusion that pneumonia is determined by external causes, is its occasional prevalence as an epidemic in the southern and southwestern portions of our country, especially among the negro population. This fact is well known, and it is a common belief among practitioners at the south, that epidemic pneumonia in the negro is more apt to be latent as regards its local symptoms, than in the white population, and that it is a much graver affection in consequence of a greater tendency to death by asthenia. It is certain that it destroys many lives, and is the scourge of plantations. Of this form of the disease, I cannot speak from personal observation.

The next points of inquiry will relate to the seat of the disease and the extent of lung affected. With reference to these points I shall interrogate the cases observed at New Orleans, Louisville, and Buffalo, separately, in order to see if the disease in these different places manifests different laws as regards its situation and diffusion.

Analysis with reference to the Seat of the Disease and the extent of Lung affected.

The pneumonia was seated in the *lower lobe of the right lung*, and limited to this lobe in 17 of fifty-seven cases observed at Buffalo, including hospital cases and the cases in private practice. It was thus seated and limited in 11 of fifty-three cases observed at the New Orleans Charity Hospital, and in 1 case of eleven observed at the hospital in Louisville. The proportion relatively to the whole number of cases is thus seen to be somewhat larger in Buffalo than in New Orleans, and considerably larger than in Louisville.

The pneumonia extended over *the whole of the right lung* in 13 of fifty-

seven cases at Buffalo, in 18 of fifty-three cases at New Orleans, and in 6 of eleven cases at Louisville. Here the proportion relatively to the whole number of cases is seen to be precisely the reverse of that in the preceding paragraph, being the largest in Louisville, and larger in New Orleans than in Buffalo. The disparity between New Orleans and Buffalo as regards these two points of inquiry, viz., the proportionate number of cases in which the disease was limited to the lower lobe of the right lung, and the proportionate number of cases in which the disease extended over the whole of this lung, will be found to be not far from compensatory; in other words, the disease extended over the whole of the right lung in a greater number of the cases in New Orleans so as nearly to account for the lesser number of cases in which it was limited to the lower lobe. In the cases in Louisville the number of cases in which the disease extended over the whole of the right lung will more than compensate for deficiency of cases in which it was limited to the lower lobe. From the results thus far given, then, we may draw the following conclusion: In Buffalo, pneumonia, when seated in the right lung, had not so much tendency to extend over the whole of that lung as in Louisville and New Orleans, but was more likely to be limited to the lower lobe. So far as this conclusion goes, a corollary is that pneumonia in Louisville and New Orleans is apt to be a graver affection than in Buffalo, since the gravity of the disease is undoubtedly greater when it extends over an entire lung than when it is limited to a single lobe.

The pneumonia was seated in *the lower lobe of the left lung* in 10 of fifty-seven cases in Buffalo, in 12 of fifty-three cases in New Orleans, and in 3 of eleven cases in Louisville. The proportion to the whole number of cases in the three places is thus seen to be not far from equal.

The pneumonia *extended over the whole left lung* in 5 of fifty-seven cases in Buffalo, in 3 of fifty-three cases in New Orleans, and in 1 of eleven cases in Louisville. Here, too, there is not a notable variation.

The pneumonia was seated in *the upper lobe of the right lung*, and limited to this lobe in only 1 of fifty-seven cases in Buffalo, in 6 of fifty-three cases in New Orleans, and in not one of the cases in Louisville.

It was seated in *the upper lobe of the left lung* in 2 cases in Buffalo, in 1 case in New Orleans, and in not one of the cases in Louisville.

The pneumonia was seated in *the lower lobes of both lungs* in 6 of fifty-seven cases in Buffalo, in 1 of fifty-three cases in New Orleans, and in 1 of eleven cases in Louisville. The preponderance of the number of cases in Buffalo over the number in New Orleans is to be noted.

Four cases only are not included in the foregoing enumeration. These cases are as follows: In Buffalo the lower and middle lobes of the right lung and the upper lobe of the left lung were affected in 1 case; the whole of the right lung and the lower lobe of the left lung were affected in 1 case, and the lower lobe of the left with a portion of the upper lobe was affected

in 1 case. In New Orleans, it is noted in one case that the upper and middle lobes of the right lung were affected in one case.

If the cases at New Orleans and Louisville, in which the right lung was affected, the disease being either limited to one lobe or extending over the whole lung, be added and compared, we have in the fifty-seven cases at Buffalo, 30, and in the fifty-three cases at New Orleans, 36, showing a greater number by six cases at New Orleans than at Buffalo. It so happens that the cases at Buffalo, in which the disease was limited to the lower lobe of the right lung, exceeds the cases in which it extended over the whole of that lung by precisely the same number, viz., 6. There would seem, therefore, to be a somewhat greater tendency of the disease to attack the right lung at New Orleans than at Buffalo; while, as already stated, there is a greater liability to an extension of the disease over the whole of that lung at New Orleans and also at Louisville.

In addition to these inferences from the results of the analysis, there appears to be a greater tendency at New Orleans than at Buffalo in the disease to attack, and to limit itself to the upper lobe of the right lung. On the other² hand, the disease seems to attack the lower lobes of both lungs oftener at Buffalo than at New Orleans. As regards the lower lobe of the left lung, and extension over the whole of that lung, the liability to be affected is not far from equal in the three cities, being much less in all than the tendency of the disease to the right lung.

Directing inquiry now to the cases collectively in the three cities, the whole number of cases analyzed is 121. Of these cases the lower lobe of the right lung was alone affected in 29, and the disease extended over the whole of the right lung in 37. The lower lobe of the left lung was alone affected in 25, and the whole of the left lung in 9 cases. The lower lobes of both lungs were affected in 7 cases. The upper lobe of the right lung was alone affected in 8,¹ and the upper lobe of the left lung in 3 cases. The whole of the right lung and the lower lobe of the left lung were affected in 1 case, the lower lobe and part of the upper lobe of the left lung in 1 case, and the lower and middle lobes of the right with the upper lobe of the left lung in 1 case.

These results show the almost invariable extension of the inflammation over at least a lobe of the lungs. They show that the most frequent variety of lobar pneumonia is that in which the inflammation extends over the whole of the right lung. This would not have been expected, and would not be the fact were it not for the cases observed at New Orleans and Louisville, the tendency of the disease to extend over the whole of the right lung being greater in these cities than in Buffalo. The results show that next to the variety, in which the whole of the right lung is involved, that in which the lower lobe of the right lung is alone affected is the most frequent. And at

¹ Including the case in which the upper and middle lobes were affected.

Buffalo the latter variety is the most frequent variety of pneumonia. These results, however, show that the cases in which the disease is limited to the lower lobe of the left lung are nearly as numerous as those in which the lower lobe of the right lung is alone affected. This would not have been expected, the common belief being that the lower lobe of the right lung is much oftener the seat of pneumonia than the lower lobe of the left lung. They show that inflammation seated primarily in the lower lobe of the left lung rarely extends over the whole of that lung. They show that the lower lobes of both lungs (double pneumonia) are rarely affected. They show that pneumonia very rarely attacks primarily the upper lobes, and oftener the upper lobe of the right than the upper lobe of the left lung. They show, finally, that when the whole of either the right or left lung is affected the liability of an extension of the disease to the opposite lung is excessively small.

The next points of inquiry will relate to the complications of pneumonia, and the occurrence of gangrene and abscess. With reference to these I shall interrogate the cases at New Orleans, Louisville, and Buffalo, separately, and compare the results.

Analysis with reference to the Complications of Pneumonia, and the occurrence of Gangrene and Abscess.

Two important complications were oftenest observed, viz: Pericarditis and delirium tremens. The latter affection occurs, of course, only in persons addicted to the abuse of alcohol, and has no special pathological connection with the pneumonia. It is simply a superadded affection, generally attributable in part, or entirely, to the discontinuance or diminished use of alcohol after the attack of pneumonia. We will consider these complications under distinct heads. It is needless to remark that under the name of complications are included those affections only which are developed during the progress of the pneumonia.

Pericarditis.—In the whole number of cases (133) pericarditis was ascertained to exist in 8 cases. It is possible that this complication may have been overlooked in some cases which recovered, and in some of the fatal cases not examined after death. In most, if not all, the cases, physical exploration was directed to the heart; but when patients do not come under observation until the first stage of pericarditis is passed, and still more, if the liquid effusions have been absorbed and the pericardial surfaces agglutinated, the diagnosis is not easy, and may not be practicable.

Of these 8 cases, 5 were among fifty-three cases observed at New Orleans; 2 were among sixty-three cases observed at Buffalo; and 1 case was among the eleven cases observed at Louisville. These results show that this complication occurs oftener at New Orleans and Louisville than at Buffalo.

Six of the 8 cases were fatal. In one of the fatal cases gangrene of the lung took place. In one of the two cases ending in recovery, there occurred

abscess of the lung. These results show the gravity of pneumonia complicated with pericarditis, and also that recovery may take place even when, in addition, the inflammation of the lung eventuates in abscess.

Delirium Tremens.—This complication existed in precisely the same number of cases as pericarditis, viz., in 8 of the 133 cases. It was associated with pericarditis in one of these 8 cases. Three of these 8 cases were fatal, one being the case in which pericarditis also existed as a complication.

Of these 8 cases, 4 were among fifty-three cases at New Orleans; 2 among sixty-three cases at Buffalo; and 1 case was among the eleven cases at Louisville. The proportion of the cases in which this complication existed was larger at New Orleans and Louisville than at Buffalo; but this, of course, only goes to show that a larger number of the patients at the two former places were addicted to intemperance. It does not show any difference as respects the laws or tendencies of pneumonia in the different places.

Other complications were extremely rare. The following are all that were noted in the histories: Parotiditis; yellow fever; phlebitis, with obstruction of the femoral vein; ophthalmia; and intercostal neuralgia; of each a single example only. This shows that, exclusive of pericarditis and delirium tremens, complications of pneumonia are by no means common, and that this is true equally of the disease at New Orleans, Louisville, and Buffalo.

Abscess.—In 4 cases the existence of abscess was ascertained. It may have existed in some fatal cases in which its existence was not known, the bodies not being examined after death. Two of the 4 cases were at New Orleans, one at Louisville, and one at Buffalo. Two of the 4 cases were fatal, and in two recovery took place. The existence of abscess in the cases which recovered was determined by the sudden occurrence of copious purulent expectoration, and by physical signs. In one of the cases which recovered, the pneumonia was complicated with pericarditis.

Gangrene.—The pneumonia eventuated in gangrene of the lungs in one case only. This case was observed at New Orleans, and proved fatal. The pneumonia in this case was complicated with pericarditis.

The number of complicated cases in the whole number of cases analyzed is 21. Of these 21 cases, 13 were among fifty-three cases at New Orleans; 5 were among sixty-three cases at Buffalo; and 3 were among eleven cases at Louisville. This comparison shows a greater frequency of complications at New Orleans and Louisville than at Buffalo, and consequently greater gravity of the disease. In this enumeration of complicated cases, those with abscess and gangrene are not included, and cases are only so considered in which affections become developed during the course of the pneumonia.

Analysis with reference to Fatality and the Duration of the Disease.

Fatality.—Points of inquiry falling under this head relate to the whole number of deaths in all the cases; to a comparison of the ratio of fatality

in the three places in which the cases were observed ; to the fatality in uncomplicated cases as compared with the cases in which important complications or concomitant affections existed ; to the fatality in cases in which the inflammation was limited to a single lobe, as compared with the cases in which two or more lobes were involved, and to the influence of age and habits of intemperance on the fatality.

In the 133 cases there were 35 deaths, making the fatality a fraction over 26 per cent.

The ratio of fatality in the three places was not the same. In 64 cases at Buffalo, there were 11 deaths. In 53 cases at New Orleans, there were 17 deaths. In 11 cases at Louisville, there were 7 deaths. The proportion of fatal cases is thus seen to be considerably larger at New Orleans and Louisville than at Buffalo. At Buffalo the ratio of fatality was a fraction over 17 per cent. ; at New Orleans it was a fraction over 32 per cent., and at Louisville it was a fraction over 63 per cent.

Of the 35 fatal cases, 16 were either complicated or associated with other important affections. Pericarditis was a complication in 6 of these cases ; delirium tremens in 5 cases ; yellow fever in 1 case ; parotiditis, some symptoms of typhoid fever, in 1 case. Meningitis was associated in 1 case ; miliary tubercles in 1 case, and great enlargement of the heart in 1 case.

Deducting from the fatal cases the number in which the pneumonia was complicated or associated with other important affections, the balance is 19. The number of cases in the whole collection in which the pneumonia was complicated, has been seen to be 21 ; deducting this number, the balance is 112. This makes the proportion of the fatality in the uncomplicated cases 19 in 112, or a fraction under 17 per cent.

In only two of the fatal cases not complicated nor associated with other important affections, was the inflammation limited to a single lobe. In one of these cases the inflammation was seated in the upper lobe, and eventuated in abscess. In the other case the inflammation was limited to the lower lobe, and no complication was ascertained during life. The body was not examined after death.

The pneumonia was double in 8 of the fatal cases. The whole of the right lung was affected in 10 cases, and the whole of the left lung in 1 case. In 2 of these cases the inflammation eventuated in abscess. Moreover, in most of the cases complicated or associated with other affections, more than a single lobe was involved. In 1 case pericarditis was associated with delirium tremens and gangrene of the lung.¹

These facts show that pneumonia is more likely to prove fatal at New Orleans and Louisville than at Buffalo, a conclusion which accords with

¹ I have enumerated among the cases of double pneumonia the case of a patient who had recovered from an attack in which the disease was seated in a lower lobe, sufficiently to be discharged, and who soon returned with another attack in which the upper lobe of the opposite lung was affected.

previous facts showing a greater liability to complications, and to an extension of the inflammation over two or more lobes in the two former places.

Another important conclusion is, that pneumonia uncomplicated and not associated with any other important affection, if it be limited to one lobe, is not a disease dangerous to life, since only two examples are contained among the 133 cases now analyzed.

The following questions arise in the present connection : Of the complicated cases, what proportion were fatal? and in how large a proportion of the cases in which the inflammation extended over more than one lobe did death take place? The first of these questions has been in part answered already. It has been seen that of 8 cases complicated with pericarditis, death took place in 6; and of 8 cases in which delirium tremens coexisted, 3 were fatal, pericarditis also existing in one of these fatal cases. Of two cases in which there existed organic disease of the heart, in 1 the recovery took place, and the other ended fatally. In the case which recovered, there existed aortic and mitral lesions with considerable enlargement. In the other case there was considerable enlargement without valvular lesions, and the pneumonia ended fatally in the first stage of the disease. Of the whole number of cases in which some other affections became developed in the course of the pneumonia (21), 11 were fatal. Of 7 cases in which pulmonary tubercles coexisted, recovery took place in all save one, and in the excepted case the lungs were crammed with miliary tubercles.

Directing attention to the second question, of 11 cases in which the pneumonia was double, the number of deaths was 8; of the 37 cases in which the inflammation extended over the whole of the right lung, the number of deaths was 10, and of 9 cases in which the whole of the left lung was involved, the number of deaths was 1 only. Thus, out of 57 patients in whom the pneumonia extended over two or more lobes, 19 died, the proportion being exactly one-third. The fatality is thus seen to be greatest in double pneumonia. Of the 11 cases falling under this head, in 3 the whole of one lung and a lobe of the other lung were affected; but deducting these from the whole number and from the fatal cases, the rate of fatality still takes the lead. Next in the ratio of fatality are the cases in which the whole of the right lung is affected. The fatality is strikingly less when the whole of the left lung is affected, being only as one to nine.

Does age exert any influence on the fatality of this disease? Of 35 fatal cases in which the ages were noted, in none was the age under 10; in 2 cases only were the ages under 20; in 6 cases the ages were between 20 and 30; in 14 cases the ages were between 30 and 40; in 10 cases the ages were between 40 and 50, and in 3 cases the ages were over 50. Comparing these results with those developed by the analysis already made with reference to age, it will be seen that in the largest number of cases in this collection the ages were between 20 and 30, while in the largest number of fatal cases, the ages were between 30 and 40. Of 3

cases under 10, none were fatal. Of 13 cases between 10 and 20, 2 were fatal. Of 44 cases between 20 and 30, 6 were fatal. Of 37 cases between 30 and 40, 14 were fatal. Of 17 cases between 40 and 50, 10 were fatal. Of 7 cases between 50 and 60, 3 were fatal. This comparison shows conclusively that age does exert an influence on the fatality from pneumonia, the liability to a fatal result being greater in proportion as patients approach the age of sixty, this collection of cases containing no examples of a greater age than sixty years.

Does intemperance exert an influence on the fatality? Of the fatal cases the habits were noted in 19. And of these 19 cases, intemperance was acknowledged in 13; 3 of the patients said they were moderate drinkers, and 3 were temperate. This shows a large proportion of intemperate patients; but on reference to the analysis of all the cases with reference to habits, it will be seen that in only 6 of 37 cases was it noticed that the habits were temperate. It is probably true that habits of drinking were noticed in a larger proportion of the cases in which they existed, than the absence of these habits in the cases in which the patients were temperate. Hence, these results do not fairly exhibit the influence of intemperance.

Duration.—The duration of the disease, from the date of the attack to the time when the patient could be pronounced convalescent, appears in the histories of 30 cases. Cases in which the pneumonia was complicated or associated with other important affections, are not included. The shortest duration was 5 days, and the longest duration 23 days. The duration was between 7 and 10 days in 8 cases; between 10 and 15 days in 17 cases; between 15 and 20 days in 8 cases; and between 20 and 25 days in one case. The mean duration in these cases was a fraction over 12 days.

The duration from the date of the attack to the time of death appears in 14 of the fatal cases. The shortest duration was 3 days, and the longest duration 20 days. The duration was between 3 and 5 days in 2 cases; between 5 and 10 days in 7 cases; between 10 and 15 days in 1 case; between 15 and 20 days in 3 cases; and it was 20 days in 1 case. The mean duration, in these cases, was a fraction over 10 days.

The duration in hospital cases, from the date of admission to the time of discharge, varied from 4 to 70 days. Extrinsic circumstances so often affect the discharge of patients, that the length of stay in hospital, in a series of cases, cannot be considered as showing the length of time that the patient was necessarily detained by the disease.

Analysis with respect to Treatment.

With reference to treatment, various points of inquiry arise. The first point which suggests itself, is the proportionate fatality, in cases in which different therapeutical measures were employed. This, however, may not afford evidence of the influence of different measures, in cases of uncomplicated pneumonia, the inflammation being limited to one lobe; for, our

analysis does not furnish more than two examples of death under these circumstances. We can, therefore, expect to bring the fatal result to bear on the investigation of the influence of therapeutical measures, in those cases only in which the disease was associated with some other important affection, or in which the inflammation extended over two or more lobes ; that is, assuming the therapeutical measures not to be in themselves destructive of life.

The duration of the disease, in cases differently treated, is another point of inquiry ; for, next to conducting an affection to a favourable termination, it is an object with the physician to render its continuance as short as possible. The data for determining the duration with precision, however, exist in the histories of only a portion of the cases, as has been already seen ; and I do not expect much from an interrogation with regard to this point.

The immediate apparent effects of remedies constitute an important point of inquiry. Palliation of important or distressing symptoms is an end of therapeutics, ranking next to the abridgment of the duration of a disease. An interrogation with regard to this point may lead to some important conclusions. In studying cases, in this point of view, we are constantly to bear in mind, a liability to error in imputing changes which take place naturally, in the course of a disease, to the influence of remedies.

Above all these objects of therapeutics is the arrest of a disease. These cases are not open to an inquiry on this point ; for in all the pneumonia progressed, and passed through its career. The practicability of arresting pneumonia is a mooted question. My experience furnishes no examples, under any plan of treatment. I am not prepared to say that it is never arrested ; but instances, if they occur, must be extremely rare. In the great majority of the cases which I have collected, the disease had already existed for several days, before coming under my observation ; so that there was little opportunity to employ measures with a view to an arrest. The previous histories, in some cases, will afford information bearing on the question of the efficacy of certain measures employed for that purpose.

To prevent extension of inflammation over more than a single lobe, and to avert certain complications, especially pericarditis and delirium tremens, are important objects in the treatment of pneumonia. We have seen that it is the development of these complications, and the extension over two or more lobes, which render the disease dangerous to life. I should be glad, were the analysis of my cases to develop any information bearing on this point.

As a point of departure for the study of therapeutical agencies in any disease, its intrinsic tendency to death or recovery is to be considered. A very important question preliminary to the consideration of the treatment of pneumonia, therefore, is, How is the disease observed to terminate when left to itself, *i. e.* without treatment ? In answer to this question, I could

cite important facts, which have been contributed by different observers within late years ; but I propose here, as with reference to other points, to limit my attention to the results of the analysis of my own cases. I shall proceed to see if my collection of cases furnishes any examples of pneumonia passing through its career without therapeutical interference. Afterwards I shall direct attention to the different measures which were employed with a view to effect a cure, abridge the duration, and palliate the symptoms of the disease.

Examples of Pneumonia without Treatment.—Three cases only, strictly fall under this head, all of which were observed at Buffalo. I shall add a fourth case, illustrating the progress of the disease under most unfavourable hygienic influences, and with scarcely any medical treatment.

CASE 1. The patient, aged 23, was attacked six days before his admission into hospital. He had no remedies prior to his admission. The lower lobe of the left lung was affected. He was convalescent on the fifth day after admission, and left the hospital on the sixth day. No remedies were given in hospital, exclusive of half an ounce of brandy, three times daily.

CASE 2. The patient, aged 30, was attacked eleven days before his admission. He had no treatment prior to his admission. The lower lobe of the left lung was affected. He was convalescent when admitted, and no remedies were prescribed. The date of his discharge is not noted.

CASE 3. The patient, aged 49, was attacked two weeks before his admission. He had no treatment prior to his admission. The lower lobe of the right lung was affected, and undergoing resolution when he was admitted. No remedies were prescribed, and convalescence progressed rapidly.

CASE 4. This patient was attacked when working alone in a shanty, there being at the time two inches of water on the ground. He was unable to leave his bed, for any purpose, for a week, and during this time he was entirely alone. He had a quart of brandy, which he drank during the week. His habits, as he stated, were temperate. At the end of a week he was visited by some one (not a physician), who gave him thirty grains of calomel. After this he remained alone for ten days. A friend at length came to him, gave him some doses of quinia, and removed him first to his own house, and afterwards to the New Orleans Charity Hospital. The physical signs on his admission into the hospital showed pneumonia affecting the whole of the right lung, and resolution progressing. He remained in hospital six days, convalescence going on rapidly. The only remedy given in hospital was a little syrup of morphia, with milk punch.

In several cases, in which patients entered hospital after the disease had existed for some time, it is stated there had been no treatment prior to admission, and, in some instances, there had been great fatigue and exposure after the commencement of the disease. I shall cite these cases, in order to show that pneumonia may progress favourably without treatment in the early stage, and under circumstances which would, *a priori*, be considered as extremely inauspicious.

CASE 1. The patient had been confined to bed for eight days before admission; had had no medical treatment, and had drank spirits freely during that time. Delirium tremens became developed after his admission, but he was discharged well in ten days. The lower lobe of the left lung was affected.

CASE 2. The patient had been confined to the bed for a week before his admission, and had had no medical treatment. His habits were intemperate. He was discharged well in twenty days. The lower lobe of the lung was affected.

CASE 3. The patient was attacked four days before his admission, and had had no medical treatment. His habits were intemperate. He was discharged well in nineteen days. The entire left lung was affected.

CASE 4. The patient was affected with acute lancinating pain in the side a week before his admission. After the attack he walked nearly a hundred miles to reach a boat for New Orleans. He had had no medical treatment. He was convalescent in ten days. The lower lobe of the left lung was affected.

CASE 5. The patient was attacked a week before his admission, and took at once to the bed. He had had no medical treatment. His habits were intemperate. The whole of the right lung was affected. Pericarditis co-existed on his admission. The inflammation of the upper lobe of the right lung eventuated in abscess. He has discharged well in seventy-two days.

CASE 6. This patient was attacked a week before his admission, and took at once to the bed. The whole of the right lung was affected. The day before his admission, in the month of January, in Louisville, he walked a mile to the hospital, and, not having a permit, he was not admitted. He walked back, and the next day, having obtained a permit, he again walked to the hospital. Death took place on the day after his admission.

CASE 7. This patient, a female aged 16, had been confined to the bed three days before admission, and had no medical treatment. The lower lobe of the right lung was affected. Discharged well in fifteen days.

CASE 8. This patient was attacked five days before his admission. After keeping the bed for three days, he walked seven miles to the hospital in Buffalo, in the month of December. The lower lobe of the right lung was affected. He was discharged well in twenty-eight days.

CASE 9. This patient was attacked three days before his admission, and had had no treatment, except that he took some cathartic pills once. The lower lobe of the left lung was affected. He was discharged well in thirty-one days.

CASE 10. This patient was attacked eleven days before his admission, and had kept the bed most of the time. He had had no medical treatment. The lower lobe of the left lung was affected. The patient convalesced immediately, but the date of his discharge was not noted.

CASE 11. This patient was attacked a week before his admission, and kept the bed. He had had no medical treatment. The lower lobe of the left lung was affected. He was well enough to become an assistant to the ward nurse in twenty days.

CASE 12. This patient was attacked five days before his admission, and kept the bed. He had had no treatment, except that he had of his own accord applied sinapisms to the chest. The lower lobe of the left lung was affected. He was transferred to the surgical ward to be treated for spermatorrhœa in thirty-six days.

Before proceeding to direct attention to the remedial measures employed in these cases, I will give a brief exposition of the general principles which governed the treatment while the cases were under my observation.

Bloodletting, general or local, was employed by me in only one of the cases, which, it will be observed, extend over a period of twelve years. This did not proceed from a determination not to resort to this measure in any case of pneumonia, but because I have, for the period just mentioned, looked upon it as only a palliative, not a curative measure, and have believed that in most instances other measures, less liable to do harm, will accomplish all the good to be effected by bloodletting. Moreover, in the majority of cases, the patients did not come under observation at the commencement of the disease. The previous history, however, in several cases, shows that bloodletting had been employed before the cases came under my care, so that an analysis will develop some facts relating to this remedy.

Tartar emetic was employed to some extent in the early part of the period during which these cases were observed; but, of late years, I have rarely employed this remedy, deeming it unnecessary in most of the cases in which it is not positively contraindicated.

Cathartics and laxatives were prescribed in some cases; but these remedies, for several years past, I have generally omitted, deeming them called for only when there is evidence of undue accumulation of fecal matter.

Blisters, and other severe methods of counter irritation, I have not employed, feeling assured that they may do harm, and not feeling an assurance that they influence favourably the progress of the disease. The pain, which they frequently occasion, is not unimportant; and an additional objection is, they stand in the way of physical examinations, by means of which alone the extent and progress of the local affection can be determined.

Mercurialization in pneumonia I abandoned ten years ago, having been led to the conclusion that the evidence of its efficacy in limiting the amount of exudation and promoting resolution, was not enough to warrant incurring the risk of salivation and other evils.

My treatment of the disease during the greater part of the period, during which these cases were observed, has been *expectant*, using this term, not in the sense in which it is often used, viz., to denote the absence of all active treatment, but in its proper sense as denoting the application of remedies, not to the disease *per se*, but to symptoms and circumstances incidental to the disease in individual cases. I have not employed measures with a view to cut short the disease, nor with direct reference to abridging

its duration. I have not expected, by any particular plan of treatment, to limit the inflammation to a single lobe, nor to prevent the development of pericarditis, believing that, as yet, we have not evidence of the adequateness of any remedies to fulfil these important objects. But the objects of treatment have been the palliation of symptoms, placing the system in a condition of tolerance as regards the disturbance caused by the local affection, and sustaining the vital powers so as to obviate a tendency to death by asthenia or exhaustion, and to promote a speedy and complete recovery. In fulfilling these objects, the indications differ widely in different cases, and the treatment in some cases, so far from being inefficient, may be called heroic with as much propriety as the treatment which consists of excessive bloodletting and other depressing agencies.

As a palliative remedy and as rendering the system tolerant of the disease, opium has seemed to me to be a most valuable remedy in the treatment of pneumonia. For the last five or six years, especially, I have been led to attach great value to the use of this remedy in pretty large doses, and I have, therefore, employed it more freely than previously.

The sulphate of quinia I have prescribed frequently in pretty full doses, but chiefly in cases in which the patients have been subject to intermittent fever, or, as has often occurred, when the latter has shortly preceded the attack of pneumonia, the object being to prevent the concurrence of the two affections.

Alcoholic stimulants and concentrated nourishment, in other words, sustaining measures, have entered frequently and often largely into my treatment of pneumonia during the last five or six years. My experience has led me to think that, judiciously regulated, they are useful in a large proportion of cases, and that heroically employed they not infrequently save life. The stage of the disease, the extent of lung involved, and the existence of complications, have not seemed to me to affect the propriety of sustaining measures, whenever there is danger of death from asthenia, and the extent to which their use is to be carried, as it appears to me, is to be measured by the amount of this danger.

From the foregoing exposition it will be seen that the range of remedies employed in these cases is limited. I shall proceed to interrogate them with regard to bloodletting, antimony, the sulphate of quinia, opium, and alcoholic stimulants.

Bloodletting.—Bloodletting had been employed before the patients came under my observation in 11 cases, and was employed by me in 1 case. Four of these 12 cases proved fatal. Directing attention to the fatal cases, in one case it is stated simply that the patient was bled and the chest blistered on the second or third day after the attack. The day following he was violently delirious; delirium tremens became developed, and death took place the day after his admission.

In another of the fatal cases the patient was admitted into hospital

12 days after the attack. The history simply states that he had been bled and extensively blistered. The treatment after his admission consisted of opium, quinine, and brandy. The whole of the right lung was affected. He became delirious and died on the seventh day after admission.

In the third fatal case the patient was bled on the sixth day, the pneumonia not having been previously ascertained. Twelve ounces of blood were taken away; delirium and death occurred on the following day; the lower lobes of both lungs were affected.

In the fourth fatal case the patient was bled to the amount of twenty ounces, and had wet cups applied on the second day; he stated that he felt relief from the bleeding. On the third day he was again bled to twenty ounces; six hours after the second bleeding there was marked aggravation of pain and other symptoms; four grains of the sulphate of morphia were given between midnight and ten o'clock A. M.; the pain was relieved, the respirations were less frequent, and the frequency of the pulse was diminished. He remained comfortable during the next day; during the night delirium tremens became developed, and death took place; the whole of the right lung was affected.

It would be unfair to impute the fatal result to the bleedings in these cases, since the whole of the right lung was affected in two cases, the lower lobes of both lungs in one case, and in the remaining case delirium tremens coexisted. That the remedy was probably injurious must be admitted, death taking place by asthenia in all the cases.

In the 8 cases in which recovery took place, bloodletting was employed as follows: On the first day in 2 cases; on the second day in 3 cases; on the fourth day in 1 case; on the sixth day in 1 case; and in 1 case the history only states that the patient had been twice bled before his admission on the fifth day. In none of the cases, except the last, had bloodletting been employed more than once; in one case wet cups had been employed; in three of the cases a blister had been applied, and one of the patients was salivated on his admission.

In four of these cases the inflammation was limited to one lower lobe; in three cases the upper and lower lobes were affected; and in one the extent of the inflammation does not appear in the history. In one case the pneumonia became complicated with pericarditis after the bleeding.

The immediate apparent effects of the remedy are noted in only two cases. In one of them, employed on the sixth day, it appeared to afford distinct relief, the pulse and respirations being diminished in frequency, and in the other case the patient experienced so much relief as to work at his calling (plasterer) a portion of the following day.

The longest duration of the disease in these cases, from the date of the attack to convalescence, was 30 days, and the next longest 15 days. The shortest duration was 9 days, and the next shortest 11 days; the mean duration was a fraction under 15 days. The mean duration of the stay

in hospital was a fraction under 25 days. In making a comparison, as regards duration, with the results of an analysis of the cases of uncomplicated pneumonia already given, it is proper to exclude the case in which the pneumonia was complicated with pericarditis. The duration in this case was 30 days. Excluding that case, the average duration, from the attack to convalescence, is exactly the same as in the aggregate of the cases in which the duration appears in the histories, viz., a fraction over 12 days.

So far as any conclusions are admissible from these facts, they go to show that the bleeding did not prevent the extension of the disease to the entire lung, which occurred in 5 of the 12 cases; nor the affection of the lower lobes of both lungs, which occurred in 1 case; nor the development of *délirium tremens*, which occurred in 1 case; nor the occurrence of pericarditis in 1 case. It did not diminish the fatality, which was at the rate of 36 per cent., being considerably larger than the rate in all the cases (26 per cent.); much larger than the rate at Buffalo (17 per cent.); somewhat larger than at New Orleans (32 per cent.);¹ but falling below the rate at Louisville (63 per cent.). It did not, however, affect unfavourably the duration of the disease in the cases which recovered, nor the length of stay in hospital. And it appeared to afford relief in the two cases in which the immediate apparent effects were noted.

Tartar Emetic.—This remedy was prescribed in one only of the cases observed at New Orleans, and in one case only at Louisville. It was prescribed in 10 of the cases observed at Buffalo. In all of these 12 cases it was given in small doses (gr. $\frac{1}{16}$ or $\frac{1}{8}$), and in nearly all cases given in conjunction with opium or the sulphate of morphia in small doses. It was always suspended if it occasioned vomiting or much nausea. It was given in those cases only in which there was high febrile movement with heat of skin, and never when supporting measures were indicated. As thus employed, this remedy could not be expected to affect very materially the progress of the disease. Palliation of certain symptoms was alone looked for through its sedative influence on the circulation and nervous system. It will suffice, therefore, to examine the cases with reference to its immediate apparent effects.

CASE 1. Given during one day, the third day after admission into the hospital, it occasioned vomiting, and was then discontinued. The symptoms were not relieved, but marked relief followed the administration of the sulphate of morphia, which was given after the antimony was discontinued.

CASE 2. The patient was 5 years of age. The remedy was given in doses of gr. $\frac{1}{8}$ hourly, for several nights in succession. Frequently it produced vomiting, and it was then suspended. This constituted chiefly

¹ None of the cases in which bloodletting had been employed were observed at New Orleans.

the treatment in this case. Convalescence was distinct on the seventh day, and the patient discharged on the twelfth day.

CASE 3. Given in doses of gr. $\frac{1}{4}$, with the same quantity of the sulphate of morphia, every four hours. On the seventh day of the disease, there was a marked improvement in the symptoms. On the following day, neither vomiting nor nausea having been produced, the antimony was discontinued. The case progressed favourably.

CASE 4. The remedy was prescribed on the day of admission into hospital, third day of the disease, in doses of gr. $\frac{1}{16}$, hourly. The first dose vomited, and the remedy was suspended. The day following there was marked improvement, the pulse falling from 130 to 110, and the respirations from 36 to 30. It is hardly probable that this improvement was due to the single dose of the remedy; and as no other remedy was given, it must be attributed to a favourable change occurring naturally in the progress of the disease.

CASE 5. The remedy was given on the day of admission into the hospital, the fifth day of the disease, in doses of gr. $\frac{1}{16}$, hourly, and at night the sulphate of morphia, gr. $\frac{1}{8}$, was prescribed. This treatment was continued for five days. There was no improvement till the fifth day, and on this day it was marked, the pulse falling from 124 to 88, and the respirations from 36 to 28. Convalescence speedily followed.

CASE 6. The remedy was given on the fourth day of the disease, in doses of gr. $\frac{1}{16}$, with the sulphate of morphia gr. $\frac{1}{8}$, every four hours. This treatment was continued for two days. The symptoms denoted improvement, the pulse falling from 120 to 96, the respirations, however, remaining the same, viz., 24. The antimony was then discontinued, and the morphia given alone. The case progressed favourably.

CASE 7. The remedy was given on the day of admission into the hospital, the third day of the disease, in doses of gr. $\frac{1}{8}$, every two hours, and at night the sulphate of morphia gr. $\frac{1}{8}$. It occasioned nausea and vomiting, and was suspended at the end of twenty-four hours. There was a marked improvement on the following day, the pulse falling from 100 to 92, and the respirations from 32 to 20. The case progressed favourably.

CASE 8. The remedy was given on the second day after admission into the hospital, the 5th day of the disease, in doses of gr. $\frac{1}{8}$ hourly, with the sulphate of morphia gr. $\frac{1}{8}$ every four hours. This treatment was continued for several days, and in the mean time bloodletting was employed once. This case progressed favourably.

CASE 9. The remedy was given on the day of admission into the hospital, the third day of the disease, in small doses (the quantity not noted), in conjunction with small doses of the sulphate of morphia. The improvement on the second day was marked, the pulse falling from 108 to 78, and the respirations from 22 to 16. The treatment was continued, and no other remedy given except a saline laxative. The patient was convalescent on the eighth day from the date of the attack.

CASE 10. The remedy was given on the third day after admission into the hospital, the seventh day of the disease, in doses of gr. $\frac{1}{8}$ hourly and Dover's powder, gr. vi, every four hours. The next day the improvement

was marked, the pulse falling from 120 to 96, and the respirations from 56 to 32. The treatment was continued, and the case progressed favourably.

CASE 11. The remedy was given on the day of admission into the hospital, the fourth day of the disease, in doses of gr. $\frac{1}{8}$, with the tincture of opium, gtt. viii, every two hours. Death took place on the following day. The patient had great enlargement of the heart. This was the only fatal case in which tartar emetic entered into the treatment.

CASE 12. The remedy was given on the sixth day after admission into the hospital, the tenth day of the disease, in doses of gr. $\frac{1}{4}$, with opium, gr. $\frac{1}{2}$, every four hours. The patient was delirious, making efforts to get out of bed. The treatment was continued for two days. The delirium ceased, and the pulse fell from 92 to 68, the respirations not diminished. Three days afterward convalescence was declared.

An examination of these cases leads to the conclusion that small doses of antimony are often useful in this disease, as shown by diminution of the frequency of the pulse with or without the respiration becoming less frequent, and by relief of delirium. In view of the facts which the cases present, I cannot but think that, of late years, I have undervalued the utility of this remedy. It is true that to the sulphate of morphia or opium, generally given in conjunction with the antimony, may be due more or less of the improvement which was apparent in most of the cases. But the doses of the latter remedy were small, and it is fair to presume that, making also due allowance for the natural progress of the disease, the antimony exerted in several of the cases a certain amount of palliative influence. In connection with the facts presented in these cases, it is to be borne in mind that antimony was not given under circumstances which would be likely to render a depressing remedy injurious, either directly or indirectly, by conflicting with supporting measures. It is also to be borne in mind that these results relate only to the remedy as given in small doses, and discontinued when vomiting or much nausea were produced, not to large doses continued in spite of vomiting, until tolerance is established, after the method of Rasori, which was so highly extolled by Laennec.

My collection of cases furnishes but a single instance in which the *veratrum viride* was employed. That my experience with this remedy is so limited in this disease is not because I have not appreciated the testimony of many observers, within the past few years, to its utility, but simply because my attention has been pre-occupied with other points of observation.

In the single case in which this remedy was employed, it was given on the second day after admission into the hospital, the fourth day of the disease, in doses of gtt. v every two hours. The pulse fell on the following day from 116 to 112, and the respirations from 28 to 24. The treatment was continued, and on the next day the pulse fell to 96, but the respirations rose to 32. The remedy produced vomiting and considerable prostration and was discontinued. During the two days that the remedy was given the inflammation extended from the lower to the upper lobe. When

the veratrum viride was discontinued, full doses of the sulphate of morphia were given, the patient becoming partially narcotized. On the day after this change was made the pulse fell to 80, and the respirations to 12. The next day the pulse was 64, and the respirations 16. The case progressed favourably.

The Sulphate of Quinia.—This remedy entered more or less into the treatment in 32 cases. In 9 of these cases it was given in doses of gr. ii three times daily, generally in conjunction with opium in small doses, brandy in some cases being also given, and sometimes the carbonate of ammonia. It is impossible, from an examination of these cases, to form any estimate of the influence of the remedy. In nearly all of the remaining 23 cases the remedy was given in doses of gr. v three times daily, and continued for a variable period. In most of these cases opium in small doses was given conjunctively, and in several of the cases, also, alcoholic stimulants. Without introducing an account of the cases severally (which from their number would occupy considerable space), I will content myself with giving certain facts and conclusions based upon abstracts of the histories now before me.

Of the 23 cases, 3 only were fatal. This small fatality might be considered as evidence of the value of the remedy; but all the cases which recovered, save 3, were uncomplicated, and the inflammation was limited to a single lobe in all the other cases save one. Of the complicated cases, pericarditis existed in 1, phlebitis affecting the femoral vein in 1, and dysentery in 1 case; and in the case in which the inflammation extended beyond a single lobe, the whole of the right lung and the lower lobe of the left lung were affected. On the other hand, of the 4 fatal cases, in 2 the whole of the right lung was affected; in 1 case the whole of the right lung was affected, and pericarditis was a complication; and in 1 case yellow fever existed. In view of these facts it is doubtful whether any inference as to the value of the remedy is to be drawn from the fatality, except that it did not exert an unfavourable influence on the termination of the disease.

An examination of the symptoms before the remedy was given, and during its administration, furnishes no evidence that the immediate apparent effects were unfavourable in any instance. On the contrary, the immediate apparent effects were in several instances favourable in a marked degree. The histories show that the pulse in several cases diminished in frequency notably under the use of the remedy, a corresponding reduction in the frequency of the respirations sometimes taking place and sometimes not. In 1 case the reduction of the pulse in 24 hours was from 132 to 112, the respirations not being lessened. In 1 case the pulse fell, in 24 hours, from 100 to 72, and the respirations from 24 to 20. In 1 case, in 48 hours, the pulse fell from 108 to 88, and the respirations from 34 to 24. In 1 case, on the second day, the pulse fell from 124 to 104, and the respirations from 32 to 24. In 1 case, on the fourth day, the pulse fell to 56, the respirations

being 24. In 1 case, on the second day, the pulse fell from 145 to 120, and the respirations from 36 to 32. In 1 case, on the second day, the pulse fell from 88 to 76, and the respirations from 20 to 18; and on the day following the pulse was 56, and the respirations 16. In 1 case, on the second day, the pulse fell to 52, and the next day to 48, the respirations on the latter day being 12.

Making due allowance for changes like these which may occur naturally in the course of the disease, it may be assumed that this remedy, in doses of gr. v three times daily, produces often a marked sedative influence upon the circulation without any unpleasant consequences, and, therefore, it is to be considered as a useful remedy in the treatment of the disease.

Examining the cases which recovered in the treatment of which the quinia constituted the sole or the chief remedy, I find that in most of them the improvement was progressive, and the duration of the disease short.

In one of the fatal cases pericarditis was developed while the patient was taking the remedy; and in another case in which this was the most prominent remedy, the patient returned the day after being discharged, with a second attack of pneumonia affecting another lobe, which eventuated in abscess and death.

In prescribing the larger doses of this remedy, I have generally had in view chiefly the prevention of intermittent fever, as a complication of the pneumonia. I have been in the habit of prescribing the remedy to patients who were subject to intermittent fever, or, as has frequently occurred, who had experienced an attack of that affection shortly before the attack of pneumonia. This may perhaps, in a measure, serve to explain a fact which has already appeared, viz., the non-concurrence of pneumonia, and intermittent fever in my experience. The fact, however, is by no means thus fully explained, for it is to be borne in mind that in a large majority of the hospital cases, the disease had existed for several days before the patients came under my observation; if, therefore, intermittent fever predisposed strongly to pneumonia, or pneumonia tended to excite an attack of intermittent fever, the combination of the two affections should have been presented in some instances before an opportunity was offered of prescribing the quinia.

The reader need not be reminded of the difficulty, or rather the impossibility of arriving at the exact value of any remedial agency, since we can never assume that two or more cases of the same disease are precisely similar, and since we can never know what should have been the progress of the disease, and the termination in any case had other or no remedies been employed. If the analytical investigation of recorded cases be the best, or the only method of developing the fruits of therapeutical experience, we can only hope, by means of this method, to reach approximations to exactness in our deductions.

Opium.—Under the head of opium I include, of course, the sulphate of

morphia, which was the form of opiate chosen in a large proportion of cases, and Dover's powder, which was sometimes given. Opiates entered more or less into the treatment in 100 cases. In some cases it was given in conjunction with tartar emetic, and continued after the latter remedy was discontinued. The doses of the opiate were small in these cases, but it is fair to impute to this remedy a share of the apparent effects of the treatment. In other cases the opiate was given either with, or subsequent to the administration of quinia in full doses. It is difficult to say how much of the immediate apparent effects of the treatment in these cases is to be attributed to the opium. In a few cases the opiate was combined with calomel. Excluding the cases in which antimony, quinia, and calomel, entered prominently into the treatment, opium was either the only, or the most important remedy given in 49 cases. In many of these 49 cases, however, as will be seen presently, alcoholic stimulants entered more or less largely into the treatment.

Of these 49 cases, in nearly all the remedy was given in pretty full or large doses, that is, the sulphate of morphia was given in doses of gr. $\frac{1}{4}$, or upwards, every 4 or 6 hours, or opium in doses of gr. ii, or upwards, every 4 or 6 hours. This was true of 46 of the 49 cases. In the remaining 3 cases, the remedy was given in smaller doses merely to palliate cough, or to promote sleep. Eliminating these 3 cases, and we have 46 in which the quantity of opium given was sufficient, as we may suppose, to affect favourably or unfavourably, to a greater or less extent, the progress of the disease. We will proceed to interrogate these cases, in order to see what conclusions may be drawn as respects the utility of the remedy.

These cases are among those observed since the summer of 1855. During this summer my attention was first directed to the apparent utility of opium in full doses in pneumonia. One of the first cases in which I employed this treatment was the case in which the *veratrum viride* had been employed for two days, during which time the pulse was reduced, but the respirations increased in frequency, and the remedy was discontinued in consequence of vomiting and prostration. The sulphate of morphia, gr. $\frac{1}{2}$, repeated every 4 or 6 hours, was then prescribed. The whole of the left lung was affected in this case. On the next day the following record was made: "This patient reports much better. He says he is entirely free from pain, and complains only of weakness. He is very distinctly under the influence of the morphia. When not aroused, he drops at once asleep. The respirations are but 12 per minute (the day previous they were 32). He is, however, roused without difficulty. The pulse is 80 (on the day previous 96). The skin is moist. The cheeks are less congested than on yesterday. Cough has not been troublesome, and the expectoration is slight. The difference, as regards pain, cough, expectoration, etc., between to-day and yesterday is striking. Is it due to the morphia?"

The morphia was continued in doses of gr. $\frac{1}{4}$ every 4 hours, and on

the following day the pulse was 64, and the respiration 16. The following note was appended to the record on that day: "The cessation of all the rational symptoms of an important pulmonary disease, whilst the two lobes of the left lung are still solidified, as shown by the physical signs, is a highly interesting fact. It is also an interesting fact that this cessation occurred rapidly under the treatment with full doses of the sulphate of morphia."

Two days afterward a saline laxative was given, no dejection having occurred for a week, and the next day no medicine was prescribed. On the following day he sat up, and convalescence progressed rapidly.

At the same time another case was under observation, in which the whole of the left lung was affected. On the 3d day after admission into the hospital, and 10th of the disease, the pulse was 92, and the respiration 40. The patient was delirious and had had diarrhœa, which had been arrested by enemata of laudanum. Dover's powder, gr. v, 3 times was prescribed on this day, with brandy and nutritious diet. On the following day the delirium had disappeared, the pulse had fallen to 76, and the respirations to 28. This treatment was continued for two days, and the sulphate of morphia, gr. $\frac{1}{4}$ every 4 hours, was prescribed. On the following day the pulse was 60, the respirations 24, and the patient asked to sit up. Under this date the following note was appended to the record: "The same interesting facts noted this day in an analogous case of pneumonia with solidification of an entire lung, are illustrated in the case of McClellan, viz: 1st. Pulse below the average of health, no embarrassment of breathing, etc., while the two lobes are solidified; 2d. This state of things under the influence of morphia."

These cases led me to adopt this treatment frequently afterward in cases of pneumonia, and it has occurred to me in other instances to meet with the same marked improvement under the treatment.

Of the 46 cases treated with full doses of opium, 11 were fatal, a fraction over 23 per cent. This is somewhat under the fatality in the whole number of cases analyzed, which was a fraction over 26 per cent.

These 46 cases were distributed between New Orleans, Louisville, and Buffalo, as follows: At New Orleans, 28 cases; at Louisville, 4 cases, and at Buffalo, 14 cases. Now, inasmuch as we have seen that the rate of mortality from pneumonia differs in these three places, it will be proper to compare the places as regards the proportion of fatal cases in those under present consideration. Of the 11 fatal cases, 8 were at New Orleans, 2 were at Louisville, and 1 was at Buffalo. The percentage of fatality in the three places therefore, is as follows: New Orleans 28 per cent.; Buffalo 14 per cent., and Louisville 56 per cent. (disregarding fractions). At New Orleans and Buffalo the rate in these cases falls below the rate in all the cases of pneumonia observed at these places, whilst at Louisville the rate is higher. It is interesting to see that the difference in the

rate of fatality in these cases at New Orleans and Buffalo (where all the cases were observed except 4), is about the same as the difference in the rate in all the cases of pneumonia observed at these two places, the latter being as 32 to 17.

Of the 8 fatal cases at New Orleans, the pneumonia was complicated with pericarditis in 2, and with delirium tremens in 3, leaving only 3 uncomplicated cases which were fatal. Excluding the complicated cases, the rate of mortality would be only 10 per cent. Of the 2 fatal cases at Louisville, in 1 the pneumonia was associated with miliary tubercles. Excluding this case, the rate of mortality would be 25 per cent. In the single case at Buffalo, no complication was discovered, but the pneumonia was double.

In view of the foregoing facts, so far as any conclusion from the fatality is admissible, it is certainly favourable to the treatment of the disease with full doses of opium, as compared with the other methods of treatment pursued in the cases which I have collected. As bearing on this conclusion, the proportion of cases among those which recovered in which the inflammation extended over a whole lung, or to the lower lobes of both lungs, and the number of cases in which the pneumonia was complicated, are to be considered. Among the cases treated with full doses of opium, ending in recovery, there were 12 in which the inflammation extended over a whole lung, or 30 per cent. while the proportion in 110 cases treated by different methods and including fatal cases as well as those which recovered, was only a fraction over 31 per cent. In 1 of the cases treated with full doses of opium, and ending in recovery, the pneumonia was double; in 4 cases the pneumonia was associated with delirium tremens, and in 1 case with pericarditis. These facts certainly strengthen the conclusion as to the utility of this treatment, but they go to show that full doses of opium do not afford a protection against the extension of the disease beyond a single lobe.

The duration of the disease is determinable from the histories in 22 of the 35 cases which ended in recovery. The shortest duration from the date of the attack to convalescence was 5 days. The next shortest duration was 7, the next 8, and the next 9 days. The longest duration was 77 days, but in this case the pneumonia was complicated with pericarditis, and eventuated in abscess of the lung. The next longest was 20, the next 16, and the next 15 days. Excluding the case in which pericarditis and abscess of the lung occurred, the mean duration in the 22 cases was a fraction over 11 days. This is slightly under the mean fatality in the uncomplicated cases (ending in recovery) analyzed without reference to treatment.

In addition to the two cases already cited, in which marked improvement took place rapidly under the treatment with full doses of opium, these cases furnish 3 examples. In one of these, on the 4th day after admission into hospital, the sulphate of morphia, gr. $\frac{1}{4}$ every 4 hours, having been pre-

scribed, the pulse fell in 24 hours from 120 to 108, and the respirations from 26 to 18. In another case, the same remedy in the same doses having been prescribed, the pulse fell in 24 hours from 104 to 84, and the respirations from 28 to 20. This was a fatal case, the pneumonia being complicated with miliary tubercles. In the remaining case, on the second day after admission, under the same doses of the remedy, the pulse fell in 24 hours from 102 to 80, and the respirations from 36 to 24. The abstracts of some of the histories which I have made for the present analytical investigation do not contain the data for determining the immediate apparent effects of this remedy.

I have stated already that in a number of instances alcoholic stimulants were given more or less largely in connection with opium. This was true of 16 of the 46 cases, 3 of these cases being fatal. In 12 cases opium was the only remedy employed, and of these cases 3 were fatal. Carbonate of ammonia was conjoined in the treatment in 2 cases, and the muriate of ammonia in 1 case. Small doses of quinia were given in conjunction in 2 cases. Quinia on one day, and brandy during the disease, were added in 2 cases. Brandy and carbonate of ammonia were both conjoined in 5 cases, one of which was fatal. Brandy and the muriate of ammonia were conjoined in 3 cases. Wine was alone added in 1 case, which was a fatal case. Hyoseyamus was added in 1 case. A little quinia and antimony preceded the opiate treatment in 1 case. Finally, quinia and muriate of ammonia with brandy entered into the treatment in one case, but in this case the sulphate of morphia was given in doses of gr. $\frac{1}{2}$ every 6 hours. These facts are given to show that in the cases in which other remedies were added (excepting alcoholic stimulants), they were subordinate to the opium treatment.

In the statement already made as to the amount of opiates given, I have mentioned the minimum quantities of opium or of the sulphate of morphia in the cases which are considered as having been treated with full doses of this remedy. In 20 cases the amount exceeded the minimum quantities mentioned. Of these 20 cases, in 2, half a grain of the sulphate of morphia was given three times daily, one of the cases being fatal; in 5, half a grain was given twice daily, one of these cases being fatal, delirium tremens existing as a complication; in 9, half a grain was given every 6 hours, one being fatal, delirium tremens existing; in 3, half a grain was given every 4 hours, one being fatal, delirium tremens existing; and in 1 case six grains were given between 9 A. M. and 11 P. M., delirium tremens existing, and the patient recovered. I have already, in another connection, cited a case not embraced in these cases because bleeding was a prominent measure of treatment, in which four grains of the sulphate of morphia was given between midnight and 10 A. M., with marked temporary improvement, delirium tremens becoming subsequently developed and the case ending fatally. I have never observed more than slight narcotism produced by the foregoing

quantities of the remedy, a fact which shows that pneumonia involves tolerance of full doses of opium.

Alcoholic Stimulants.—Prior to 1852 I had not prescribed alcoholic stimulants in pneumonia except toward the close of life in fatal cases, and in small quantities sometimes during convalescence. The first of the cases in this collection in which I was led to employ alcoholic stimulants during the progress of the disease, came under observation in the year just named. I saw the patient in consultation on the eighth day of the disease with my friend, Dr. C. H. Wilcox. The whole of the right lung was affected. He had had delirium from the commencement, manifested by incoherency and efforts to get out of bed. He had been treated with antimony, calomel, vesication, and small doses of opium. The pulse was 120, and the respirations 20. Brandy was given tentatively with Dover's powder. Finding that the symptoms denoted improvement, the brandy was given freely. On the third day, after commencing the use of stimulants, the pulse had fallen to 80, and the mind was clear. Shortly afterward convalescence was pronounced, and he recovered rapidly.

From that time I have used alcoholic stimulants very frequently, and often freely in cases of pneumonia. I have long since ceased to think that there is much, if any, risk of doing harm by a premature use of stimulants, although I have not considered that all cases call for their use. On examining the cases that have since occurred I find that alcoholic stimulants in some form, and to a greater or less extent, entered into the treatment of 73. Of these 73 cases, 18 were fatal. This fatality is at the rate of 24 per cent., but it is to be considered that in cases tending to a fatal result stimulants are almost always given more or less freely, so that the proportion of deaths cannot be taken as any evidence of the treatment in this respect being unfavourable, certainly without examining carefully the circumstances of the cases individually. Moreover, in nearly all of the cases, fatal or otherwise, other measures of treatment were conjoined with the use of stimulants.

Among the cases in which stimulants were used I find but two in which they constituted the sole treatment. In one of these cases the patient, on admission into hospital, was delirious; the pulse was 130; the respirations 24; the whole of the right lung was affected, and the prolabia were livid. Brandy on the first day was given every half hour, the quantity not noted. On the following day the improvement was marked; the pulse had fallen to 112, the respirations remained the same; but the lividity was lessened, and the delirium had disappeared. The patient recovered, no remedy but brandy being given. In the other case the disease was less severe. A single lobe only was affected. Brandy half an ounce three times daily constituted the treatment. Convalescence was pronounced the twelfth day after the date of the attack.

In the cases ending in recovery in which alcoholic stimulants were used

in conjunction with other measures, it is impossible to determine demonstratively to what extent they affected the progress of the disease. I can only assume that in these cases no unfavourable effect was produced. It is by no means probable that they were inoperative either for good or harm and, if not hurtful, it is fair to presume that they were beneficial. I believe the judicious use of stimulants to be of not a little utility in the treatment of pneumonia, and that by their free use in certain cases lives are saved which would otherwise be lost. This, however, is an opinion, the correctness of which cannot be proved by the facts contained in these cases, nor can a similar opinion with regard to any therapeutical measure be demonstratively established. I shall content myself with giving some account of the quantity of stimulants given in the 55 cases ending in recovery.

Of these 55 cases, in 11 brandy or whiskey was given in the form of milk punch. The quantities given in this form were often as great as when the brandy and whiskey were given in water, but they are not definitely stated in the histories. In 8 cases it is simply stated that brandy or whiskey was given more or less freely. In 3 cases wine or porter was given. Of the remaining 33 cases in which either brandy or whiskey, given in water, was continued for a greater or less period during the progress of the disease, the quantities were as follows: Two ounces every 2 hours in 5 cases; every 3 hours in 2 cases; every 4 hours in 5 cases; three times daily in 3 cases. One ounce every hour in 1 case; every 2 hours in 1 case; every 3 hours in 1 case; every 4 hours in 4 cases; three times daily in 2 cases. Half an ounce every hour in 1 case; every 2 hours in 1 case; every 3 hours in 1 case; every 4 hours in 4 cases; three times daily in 2 cases.

The coexistence of pericarditis did not prevent the use of stimulants. They were given freely in two cases in which this complication existed, and recovery took place. They were also used freely, and, with moderate doses of the sulphate of morphia, constituted the treatment in the case of a patient affected with organic disease of heart, involving mitral and aortic lesions and considerable enlargement. In this case the pneumonia extended over the whole of the left lung. The pulse rose to 140, and the respirations to 48 in this case. Convalescence was pronounced in 6 days.

The use of alcoholic stimulants being conjoined with other measures, generally with quinia or opium, save in the two cases already cited, we cannot study their immediate apparent effects in these cases, nor their influence on the duration of the disease. An examination of the cases, however, leads to this important result, viz., in no instance does a comparison of the symptoms, before and after commencing their use, afford evidence of an aggravation of the disease having been produced by them.

The muriate of ammonia was the only remedy given, except a little wine, in one case. The patient was admitted into hospital on the sixth day of the disease; the pulse was 100, and the respirations 40. A drachm of the muriate of ammonia three times daily was prescribed; on the next day the

pulse was 84, and the respirations 36; the next day the pulse was 72, and the respirations 24; convalescence was declared on the following day. This remedy was given in several cases, but in conjunction with other remedies, so that its apparent effects cannot be studied. This remark will apply also to the carbonate of ammonia.

With regard to diet, in the cases in which alcoholic stimulants were given, concentrated nourishment was generally directed, viz., essence of beef, or chicken soup, and milk. These articles of food, given at short intervals, were considered as important adjuncts to the stimulants, together forming the sustaining treatment. As a rule, of late years, in cases of pneumonia, I have allowed solid animal and farinaceous food as soon as convalescence is declared, and a speedy return to full diet. I have also encouraged patients to sit up early, and to take moderate exercise as soon as their strength will permit.

In leaving the subject of treatment, I would remark that it does not fall within the scope of this paper to discuss the rational indications for different remedial measures in pneumonia, or to consider the circumstances under which different remedies are contraindicated. My present object has been to develop, by analysis, the facts pertaining to the treatment of the cases which I have observed, and to present the conclusions to be drawn from the facts. The latter may be summed up as follows:—

A few cases in this collection illustrate the favourable progress of the disease and recovery without medical treatment, and even under most unfavourable hygienic circumstances. Recovery, the favourable progress of the disease and a brief duration, even if an entire lung be affected, do not necessarily constitute evidence of the curative efficacy of any plan of treatment which may have been pursued. But on the other hand, taking into view the fact that in only two cases in this collection in which the disease was uncomplicated and limited to a single lobe, did it prove fatal, a considerable number of fatal cases when no important complication existed, and the disease did not extend to two or more lobes, should be considered as furnishing good grounds to suspect that the treatment pursued contributed to the fatality.

This collection of cases furnishes a number of examples (12) of persons who received no medical treatment for several days after having been attacked with the disease, in several instances having been exposed to fatigue and other unfavourable circumstances, and in all, save one, the disease progressed favourably and ended in recovery.

The analysis of 12 cases in which bloodletting was employed, furnishes no evidence that this measure affected favourably the progress of the disease, nor, on the other hand, any positive evidence that it affected the progress of the disease unfavourably. In some of the cases it appeared to afford relief.

Tartar emetic given in small doses when there existed high febrile move-

ment with heat of skin, appeared in several instances to be useful as a palliative remedy, reducing the frequency of the pulse and respirations.

The sulphate of quinia given in full doses, *i. e.* gr. v 3 times daily, was not found to exert apparently an unfavourable influence in any instance. It produced in several instances a marked sedative effect upon the circulation, and, as a palliative, appeared to be a useful remedy. It is probable that it may, in some cases, have prevented the concurrence of intermittent fever with the pneumonia, although facts previously developed go to show that this coincidence is not apt to take place when the remedy is not given.

Opium in full doses appeared in some cases to produce a marked improvement in the symptoms of the disease. It appears also to have affected favourably the progress of the disease, diminishing its fatality, and abridging slightly its duration. There is no evidence, however, that it prevents the extension of the inflammation beyond a single lobe of the lungs. Patients with pneumonia sometimes manifest a tolerance of this remedy in large doses, that is, narcotism may not be produced even when several grains of the sulphate of morphia are given within a few hours.

Alcoholic stimulants given more or less freely, and continued for a greater or less period during the progress of pneumonia, produced no apparent unfavorable effects. The facts developed by this analysis are consistent with the opinion that, given in conjunction with opium and nutritious diet, they affect favourably the progress of the disease, and diminish its fatality. They are not contraindicated by the coexistence of pericarditis or organic disease of the heart.

The scope of this paper has not embraced the study of the symptomatic phenomena of pneumonia. With reference, however, to one symptom, I propose, in conclusion, to interrogate the few cases in which it was made a subject of observation. I refer to the presence or absence of the chlorides in the urine. It has been recently stated that the chlorides disappear from the urine during the progress of the exudation in pneumonia, and reappear so soon as the exudation ceases. Hence, it is considered by some that the absence of chlorides is evidence that the inflammation is extending, while, on the other hand, their reappearance is a test that the inflammation has reached the limit of its extension. My attention was directed to this subject in recording some of the cases which came under observation last winter (1859-60). I shall proceed to interrogate these cases.

Analysis with reference to the Presence or Absence of the Chlorides in the Urine.

The urine was examined with reference to the presence of the chlorides in 11 cases. I shall give the facts, so far as they relate to the present object of inquiry, in the cases respectively.

CASE 1. The patient was admitted into the hospital on the 3d day of the disease. The lower lobe of the left lung was affected. On the 3d day

after admission, the symptoms were worse; the pulse was 128, and the respirations had increased from 32 to 40 per minute. The physical signs of solidification over the affected lobe were more marked. The urine examined on this day presented not a trace of the chlorides. On the 5th day after admission the patient's condition was much improved. The pulse and respirations were less frequent; the signs of solidification of the affected lobe continued. The urine examined on this day was found to contain the chlorides in moderate quantity. Convalescence was soon declared.

CASE 2. The patient was admitted on the 7th day of the disease. The physical signs denoted solidification of the lower lobe of the left lung. The chlorides were present in the urine in considerable quantity. On the second day after admission, there was no improvement in the symptoms, except that the respirations were somewhat less frequent. The urine was not examined on this day, nor afterwards. On the 4th day after admission there was marked improvement, and convalescence was soon declared.

CASE 3. In this case the urine was not examined until after improvement had commenced. The chlorides were then present in moderate quantity.

CASE 4. The patient on his admission (the previous duration of the disease not determined), presented physical signs denoting solidification of the upper lobe and of the upper portion of the lower lobe. The urine contained only a trace of the chlorides. No farther examination of the urine was made. Delirium tremens became developed, and death took place on the 9th day after admission.

CASE 5. On the second day after admission (previous duration of the disease not determined), the physical signs showed solidification of the upper lobe of the right lung. The chlorides were present in the urine in moderate quantity. On the 4th day after admission improvement had taken place, and the chlorides were still present in the urine in moderate quantity. The improvement was progressive, and the urine continued to contain the chlorides.

CASE 6. The patient was admitted on the 12th day of the disease. The physical signs showed complete solidification of the upper lobe of the right lung, and incomplete solidification of the lower lobe. The chlorides were present in the urine in moderate quantity. On the 3d day after admission, the urine did not contain a trace of the chlorides. No improvement had occurred, as shown either by the symptoms or signs. On the 4th day, the patient's condition was improved. The urine was not examined. On the 5th day delirium tremens became developed, but the physical signs showed that the lung was undergoing resolution. The chlorides were abundant in the urine on this day. Death took place on the 7th day after admission.

CASE 7. The urine was not examined until after the improvement was manifest. It then contained the chlorides.

CASE 8. On the day of admission into hospital, the 8th day of the disease, the signs showed solidification of the lower lobe of the right lung. On the second day after admission, the signs showed an extension of the affection to the upper lobe of the same lung. At this time the chlorides were present in the urine in moderate quantity. On the 3d day after admission, solidification of the upper lobe had taken place. The chlorides

were present in the urine in considerable quantity. Pericarditis was ascertained to exist on this day by the physical signs. On the 8th day the chlorides were abundant. The patient recovered, the inflammation of the upper lobe eventuating in abscess.

CASE 9. The patient was admitted into hospital with pneumonia and delirium tremens, the previous duration of the pneumonia not determined. On the second day after admission, the physical signs showed solidification of a portion of the lower lobe of the right lung. On the 3d day the solidification had extended over the lobe, and was more complete than on the preceding day. The chlorides were present in the urine in moderate quantity. Death took place on the 4th day, and on post-mortem examination the whole of the lower lobe of the right lung was to be found solidified, and the upper lobe of the same lung was in the first stage of pneumonia.

CASE 10. On the 4th day after admission, the physical signs in the morning showed commencing solidification of the upper lobe of the right lung. The chlorides were then present abundantly in the urine. At evening of the same day, the physical signs showed advanced solidification of the affected lobe. The following note was appended to the record of that day: "This patient entered while the pneumonic solidification of the lower lobe of the right lung was undergoing resolution. The pneumonia has attacked the upper lobe of the right lung after his admission, and is now advancing over that lobe, notwithstanding the abundance of chlorides in the urine." On the 5th day the solidification had extended over the upper lobe, and the chlorides in the urine were still abundant. The patient recovered.

CASE 11. On the 2d day after admission in the morning, the physical signs showed solidification of the lower, but not of the upper lobe of the left lung. In the evening of the same day, the signs showed commencing solidification of the upper lobe. The chlorides were abundant in the urine on the morning of this day. The following is quoted from the record on that day: "On an examination of the urine this morning, the chlorides were present in abundance; and in the mean time (evening), the pneumonic solidification has advanced so rapidly, that the breathing over the upper lobe, which this morning was vesicular, although feeble, has become, in 8 hours, intensely bronchial." On the 3d day there was some improvement in the symptoms. The chlorides were still present in abundance. On the 4th day improvement more marked, and the chlorides were abundant. On the 6th day the improvement continued, the chlorides still abundant. The patient recovered.

The facts contained in these cases show that the chlorides may disappear from the urine while the local affection is advancing, and reappear in the urine after the affection has reached the limit of its advancement, and resolution has commenced. Cases 1 and 6 warrant this conclusion. Case 4 showed a marked diminution of the chlorides (a trace only being present), probably under the circumstances just stated. Cases 8, 9, 10 and 11, however, show that the chlorides may be present in the urine while the inflammatory exudation is rapidly going on. It is to be remarked that in all these cases the advancement of the disease was in a lobe attacked

secondarily, and while resolution may have begun in the lobe previously affected. Perhaps further observations will show that, under these circumstances, the chlorides may be present exceptionally to a rule of their absence during the advancement of the local affection.

Judging from these facts, we can only say that the absence of the chlorides may be considered as evidence that the exudation is going on; but the presence of the chlorides does not constitute conclusive evidence that exudation is not going on. A large series of observations in which the presence or absence of the chlorides is noted in connection with the condition of the lungs as denoted by the physical signs, as well as symptoms, is a desideratum.

Cases 2, 3, 5 and 7, only show the presence of the chlorides during the progress of the resolution of pneumonia.

ART. II.—*On Uræmic Intoxication.* By WILLIAM A. HAMMOND, M. D.,
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WHEN we consider how important a part the kidneys fulfil in depurating the blood, we can readily believe that any serious interruption to the due performance of their function must be attended by great disturbance in the healthy action of the other organs of the economy. We find that such is actually the case. Physiological experiments, together with many well-established cases of disease, have taught us that suppression of the urinary excretion is one of the most dangerous events that can happen in the whole range of pathological occurrences.

In the present memoir, I propose to consider the subject of uræmia, or that condition of system due to the accumulation in the blood of matters which in health are removed by the kidneys, basing what I have to say mainly upon my own investigations.

When the renal arteries of an animal—as, for instance, a dog—are ligated, or the kidneys removed, death ensues in from two to four days generally, though occasionally life is retained for a longer period. In one of my own experiments, the animal, a small dog, lived for twelve days; and Marchand¹ mentions a case in which a sheep lived for nearly a fortnight after removal of the organs in question.

At first, the animal upon which this operation has been performed does not appear to be seriously inconvenienced thereby. It eats, sleeps, and follows its other instincts with but little irregularity. After a variable

¹ De l'existence de l'urée dans les parties de l'organisme animal autres que l'urine. *L'Experience* 1839, t. ii. p. 43.

period, noticeable symptoms begin to manifest themselves. These are loss of appetite, nausea, vomiting, and indisposition to exertion of any kind. Occasionally there is purging. Finally, there is either coma or convulsions, or both, and the animal dies in a state of stupor, or in epileptiform convulsions. If, previous to the death of the animal, the blood be examined, it is found loaded with urea, and this substance can generally be detected in the matters vomited and discharged *per anum*. *Post-mortem* examination reveals the presence of urea in the contents of the stomach, in the blood, the lungs, liver, and other parts of the body.

In Bright's disease, as it affects the human subject, we find that, owing to structural changes in the kidneys, the urine is imperfectly eliminated. An element, albumen, is separated from the blood, which does not normally exist in the urine. The excretion, upon analysis, is found to be deficient in urea, and may even, as I have myself found, be almost entirely free from it. Coma and convulsions at length appear, and death soon follows. If, previously to the above, the blood be analyzed, urea is discovered in large quantity, and a *post-mortem* examination reveals the presence of this substance in nearly every portion of the system. Such is the natural termination of Bright's disease. Sometimes, however, the retained elements of the urine, by their action on the brain and nervous system, produce inflammations of important structures, and death ensues before the stage of uræmic poisoning is reached.

In puerperal eclampsia, in the latter stages of scarlet fever, in yellow fever, and in cholera, symptoms and appearances similar to those above mentioned are frequently present, and probably depend upon a like immediate cause.

To these symptoms, taken collectively, the terms uræmia, uræmic intoxication, and others of like import have been applied.

Various theories have been propounded, relative to the immediate cause of uræmia. Thus, Osborne¹ considered it due to arachnitis; Prevost and Dumas² to effusion into the ventricles of the brain; Rees³ thinks it may be caused by a watery state of the blood; and Bence Jones⁴ supposes it to be induced by an accumulation of oxalic acid in this fluid. There is no evidence to support any of these hypotheses; in fact, each has been positively disproved.

The view most generally held, has been that which ascribes uræmia to the direct action of the urea retained in the blood. Later investigations have called the correctness of this hypothesis in question, and much cogent evidence has been adduced against it. Like most other physiological ques-

¹ On the Nature and Treatment of Dropsical Diseases. London, 1837, p. 36.

² Annales de Chimie et de Physique, t. xxiii. p. 90 *et seq.*

³ On the Nature and Treatment of Diseases of the Kidneys, &c. London, 1850, p. 67.

⁴ Lectures on Animal Chemistry. Medical Times, January 3d, 1852.

tions, the present is not to be solved but by the patient and thorough research of numerous investigators. I can only hope that the observations and experiments which follow may prove to be contributions in the right direction.

The fact that urea is formed in the blood may be regarded as sufficiently determined. It is not present in the muscles, for analysis fails to detect it; but other substances, the immediate products of their destructive metamorphosis, and which admit of still further degradation, are found; and the experimental evidence, which establishes the fact that increased muscular exertion leads to increased elimination of urea, indicates these organs as one, at least, of its sources of origin. In addition, we have the beautiful researches of Bechamp,¹ which are conclusive as to its production from proteinaceous substances.

Prevost and Dumas,² Marchand,³ and numerous other observers, have shown that, after extirpation of the kidneys, urea accumulates in the blood, and consequently these organs cannot be regarded as forming this substance. They, in fact, only separate it, as they do the other constituents of the urine, from the blood brought to them by the renal arteries. As the healthy kidneys are constantly in action, urea never normally exists in the blood in any very considerable amount. It is only when, through disease or other cause, the kidneys are prevented performing their function of elimination, or when large quantities are submitted to examination, that urea is to be detected in the blood in any but very small proportion.

The immediate cause of the production of the group of symptoms known as uræmia is at present, as has been already stated, a point upon which there exists some diversity of opinion, though numerous researches have been made with the view of elucidating the subject. Urea has been injected into the blood of animals, and as death did not often follow, this substance has been regarded by some as non-poisonous. Even urine, filtered so as to free it from mucus and epithelium, has, under like circumstances, proved harmless; but when injected unfiltered, death invariably ensued. The inference therefore from these last cited experiments is, that the urine, if not eliminated, is not, as such, capable of causing death, for the mucus and epithelium are not separated from the blood by the kidneys, and consequently are not, properly speaking, constituents of the urine. As, however, the injection of unfiltered urine has always resulted in death, the further deductions must be drawn that epithelium, or mucus, or both, are of themselves poisonous, or that one or both are capable of so acting upon the retained elements of the urine, as to cause the formation of some substance possessed of toxical properties. These conclusions are, however, of

¹ *Annales de Chim. et de Phys.* November, 1856.

² *Annales de Chim. et de Phys.*, 1823, t. xxiii. p. 90.

³ *L'Experience*, 1839, t. ii. p. 43.

course only legitimate, provided that the experiments referred to have been conducted with the care and accuracy so absolutely essential in all physiological investigations. It will be seen hereafter that it is more than probable the experiments cited were not altogether free from error.

In a philosophical treatise on the subject of Bright's disease, Frerichs¹ advances the opinion that uræmia is not due directly to the presence of urea in the blood, but to the conversion of this substance into carbonate of ammonia, through the agency of a ferment supposed to be present in the circulating fluid. This view has been accepted by Braun,² but has been controverted by Schottin,³ Zimmerman,⁴ Gallois,⁵ myself,⁶ Bernard,⁷ and others.

The arguments relied upon by Frerichs to support his theory are based, mainly, upon experiments on the lower animals—dogs—but partly upon observation of cases of Bright's disease which came under his notice. As this hypothesis, from the source whence it comes, from its apparent simplicity, and from certain facts which seem to give it support, has attracted to a considerable extent the attention of pathologists, I deem it important, before proceeding further, to enter somewhat at length into its consideration.

The fact that urea may be injected into the blood without uræmia following, and that even filtered urine may be thus introduced with impunity, are circumstances considered by Frerichs as incompatible with the hypothesis which ascribes this condition to the retention of the urea *per se*. That in animals, the kidneys of which have been removed, there was vomiting and purging of ammoniacal matters; that ammonia was exhaled from the lungs; and that after death, as well in them as in persons dying of Bright's disease, who during life had manifested symptoms of uræmia, ammonia was discovered in the blood and in the contents of the stomach and intestines; that urea has been frequently found in the blood of persons suffering from Bright's disease, in whom no uræmia was present, and *vice versa*—constitute, in his opinion, a mass of evidence strongly in favor of the theory he has advanced. Add to all this the results of experiments performed by him

¹ Die Brightsche Nierenkrankheit und deren Behandlung. Braunschweig, 1851, p. 111.

² The Uræmic Convulsions of Pregnancy, Parturition, and Childbed. Edinburgh edition, 1857. Translated by Dr. J. Mathew Duncan, p. 16. I regret that I have not the original work (*Lehrbuch der Geburtshülfe u. s. w. Wien*, 1857) to refer to. Dr. Duncan's translation, however, appears to be very correctly rendered.

³ Vierordt's Archiv., 1853, Heft 1, p. 170.

⁴ British and Foreign Medico-Chirurgical Review. Am. ed., January, 1853, p. 223. From Deutsche Klinik, No. 37.

⁵ Comptes Rendus, No. 14, Avril, 1857.

⁶ On the Injection of Urea and other substances into the Blood. North American Medico-Chirurgical Review, April, 1858.

⁷ Leçons sur les Propriétés Physiologiques et les Alterations Pathologiques des Liquides de l'Organisme, t. ii. Paris, 1859, p. 36.

with direct reference to the point in question, and we have the main points before us upon which he has constructed the ingenious and beautiful hypothesis, that uræmic intoxication is directly due to the presence of carbonate of ammonia in the blood, which substance has been formed from the retained urea through the action of a ferment. Of the nature of this ferment he does not attempt to furnish an explanation.

Two series of experiments were performed by Frerichs. In the first, a solution containing from two to three grammes of urea (thirty-one to forty-six grains) was injected into the veins of dogs, the kidneys of which had been previously removed. The animals remained for some hours to all appearance unaffected; a circumstance which Frerichs regards as indicating that the urea, as such, exerted no detrimental influence upon the nervous system. After a longer or shorter period (from one and a quarter to eight hours), the animals became restless, and vomited acid chyme, or a mucous matter of a yellow colour, according as the stomach was full or empty, at the commencement of the experiments. Under the latter condition, the vomited matter was of decided alkaline reaction. Convulsions occurred, and, at the same time, ammonia was expelled with the expired air. Finally, the animals fell into a state of coma, the respiration became stertorous, and death soon followed. Occasionally the convulsions were absent, coma being the first symptom of cerebral disturbance. After death, which generally occurred in from two and a half to ten hours after the injection of the urine, ammonia was always found in the blood. The contents of the stomach, in most instances, gave off a strong ammoniacal odor, and contained carbonate of ammonia in large quantity. In one case they were feebly acid, and even then ammonia was present. This substance was also found in the bile and other secretions.

In the second series of experiments, a solution of carbonate of ammonia was injected into the circulation, the kidneys of the animals remaining intact. Immediately afterwards convulsions ensued, which, in some cases, were very violent, but were soon succeeded by a comatose condition. The respiration was laborious, and the expired air was loaded with ammonia. There was also vomiting of biliary matters. The stupor lasted for several hours; so long as it was present, ammonia continued to be exhaled from the lungs. Gradually it ceased to be expired, and the animals slowly recovered their senses. If, during the coma, an additional amount of carbonate of ammonia was injected, convulsions again occurred, the vomiting was renewed, and the urine and feces were involuntarily evacuated. In from five to six hours, the ammonia again disappeared from the blood, and the animals regained their ordinary liveliness.¹

There are several objections to be urged against these experiments of Frerichs, and the inferences drawn from them by him and those who coin-

¹ *Op. cit.*, p. 111 *et seq.*

cide with him in his uræmic theory. In order to present these more connectedly and with greater clearness, I quote Frerichs' own details of the first experiment of each series, which may be taken as the type of the others of its class. The first series consisted of six experiments.

"No. 1.—Both kidneys were extirpated from a young full-grown dog, at 3 o'clock P. M., by opening the abdominal cavity. On the following morning, the animal appeared to be perfectly well; it ate, and wagged its tail when spoken to. The posterior extremities, however, appeared to be paralyzed. At 3 P. M., a solution of two grammes of urea was injected into the left jugular vein. The animal remained in the same state as before, its condition not being altered in the least. At 4 P. M., it became restless, seemed to be choking, and vomited several times. The matter thus ejected consisted of a slimy yellow fluid, with a strong alkaline reaction. Soon afterwards convulsions supervened, the animal rolled from one side to the other. Opisthotonos ensued in the posterior, alternating with violent contractions of the other muscles of the body. From time to time the animal was quiet, after which the convulsions returned with increased violence. In the meanwhile, the vomiting continued, and a glass rod, moistened with chlorhydric acid, and held to the nose, caused the formation of thick white fumes, showing the presence of ammonia in the expired air. By degrees, the convulsions abated in violence, and at length entirely ceased. The animal now lay in a state of sopor, the respiration being quick and difficult. Finally, the respiratory motions became weaker, and at 5½ P. M. death ensued.

"The cavities of the body were immediately opened, and the blood contained in the heart and large vessels collected. It was of a dark violet colour. The colour did not become markedly clear by heating, but by the following morning it had assumed a bright scarlet hue. After four minutes, the fibrin was strongly coagulated. The blood-corpuscles were not changed from the normal form. Ammonia in considerable quantity was contained in the blood—a glass rod previously moistened with chlorhydric acid giving rise to the white fumes indicative of its presence.

"A part of the defibrinated blood was mixed with water, and distilled in the water-bath. A fluid with an alkaline reaction came over, which, being neutralized with chlorhydric acid and evaporated, deposited crystals of chloride of ammonium. To another part of the blood caustic potash was added, and an ammoniacal odour was given off.

"The stomach was strongly contracted, and contained still a few corroded pieces of bone, and a small quantity of a yellow, tenacious, ropy fluid. The stomachal mucous membrane was of a livid red colour, partly through vascular injection and partly through imbibition. The fluid contained in the viscus exhaled a sharp odour of ammonia, reacted strongly alkaline, and formed thick fumes with chlorhydric acid. In an alcoholic extract of the same, no traces of unconverted urea were discovered. In the bile, also, urea was sought for, but only compounds of ammonia were detected. The substance of the brain, and the membranes covering it, contained the normal quantity of blood, and the fluid in the ventricles was not increased in amount. The lungs were healthy, except that posteriorly there was some congestion. The mucous membrane of the trachea and bronchia was slightly reddened. The liver and spleen were, to all appearance, healthy. In the abdominal cavity, a small quantity of bloody fluid was contained. No evidences of intense peritonitis were visible."¹

The other experiments of this series do not differ in any material respect from the one quoted, except that in the third no ammonia was detected in the expired air. The details of the fifth and sixth are not given.

Now, in these experiments there is not the least evidence that carbonate

¹ Op. cit., p. 278 *et seq.*

of ammonia was the cause of death, or even that this substance was present in the blood in any abnormal amount.

In the first place, it is asserted that ammonia was present in the pulmonary exhalation. This statement is doubtless correct, but the inference which Frerichs draws from it is, I think, unwarranted. Ammonia can generally be detected in the products of respiration in healthy dogs, into the veins of which no urea has been introduced, and in which the kidneys are in healthy functional activity, by employing the test made use of by Frerichs. It is, however, occasionally absent, and in several cases which will hereafter be more specifically referred to, in which urea had been introduced directly into the blood, ammonia was not to be found in the pulmonary exhalation, though recourse was had not only to Frerichs' process, but to others of far greater delicacy.

In the second place, the means employed by Frerichs to establish the existence of ammonia in the blood were faulty in the extreme, and such as will always effect this object if urea be present, even though ammonia, in the first instance, be altogether absent. The defibrinated blood was distilled in a water-bath (at what temperature we are not informed), and an ammoniacal fluid collected. Now, no matter how carefully this operation was conducted, if any urea was present, a portion of it would have undergone decomposition, and ammonia would have appeared in the distillate.

Thus, I took about thirty cubic centimetres of freshly-drawn dog's blood, defibrinated it by shaking it in a bottle with small strips of lead, and then added to it five grammes of urea. It was then placed in a small retort, and carefully distilled in the water-bath at a temperature of 85° Cent. (185° F.), till a sufficient quantity had passed over. A few drops of the distillate were then placed on a strip of glass, and held for a few moments over a vessel containing chlorhydric acid. Upon evaporation, and subsequent microscopical examination, crystals of chloride of ammonium were found in large quantity. A similar strip of glass was then exposed to the vapor of chlorhydric acid, and held for a short time to the ball of the retort whilst the distillation was progressing, in such a manner that the vapour was received upon it. Thick white fumes of chloride of ammonium were produced, and crystals of the same substance were formed upon the glass.

In order further to establish the presence of ammonia, Frerichs added caustic potash to another portion of the blood. This proceeding was equally objectionable as the other, for this substance decomposes urea into carbonate of ammonia, and, of course, therefore it was not surprising that an ammoniacal odour should have been evolved.

Now, I have no intention of denying that ammonia existed in the blood of the dogs submitted by Frerichs to experiment. On the contrary, I believe it to be generally present in the blood of most animals. The fact that Frerichs detected it by holding over the blood a glass rod moistened with chlorhydric acid, is perhaps sufficient to establish the point. But that

this circumstance is at all confirmatory of his theory is far from being the case. Richardson¹ has shown, by numerous experiments, that ammonia is a constant constituent of the blood of dogs and many other animals, and by repeated observations I have satisfied myself of the correctness of this conclusion. I have frequently failed when using Frerichs' test with the glass rod moistened with chlorhydric acid, which, besides its want of extreme delicacy, is liable, from several causes, to yield erroneous results. These, however, with care, may generally be avoided. I have, therefore, preferred either Reuling's² process with logwood, or Richardson's, which consists in moistening a slip of glass with chlorhydric acid, and exposing it to the vapour arising from the blood, when, if ammonia be present, minute crystals of chloride of ammonium will be formed, and may be readily perceived under the microscope, with an object glass of moderate power. This process has already been referred to as the means employed during the present researches in examining for ammonia.

I am aware that Davy³ has not been able to detect ammonia in the blood of the common fowl. Not having access at present to the original paper, I know not what process he employed. With Richardson's method, I have never failed to find it in the blood of this animal in the course of fourteen experiments with reference to this point.

It will be seen, therefore, that ammonia is not an abnormal constituent of the blood, as Frerichs evidently supposes. There is no proof, either, that any of the retained urea was converted whilst in the system into carbonate of ammonia.

The fact that ammonia was found in the contents of the stomach, which consisted of fragments of undigested bone and of mucus, should not have the least weight in the matter, as it is well known that this latter substance causes the decomposition of urea, and the consequent formation of ammonia. No urea was detected in the alcoholic extract of these contents, and it is therefore probable that it was, as Frerichs supposes, entirely decomposed. There is no evidence, however, to lead us to infer that this change was effected in the blood, but, on the contrary, much to warrant us in believing that it took place in the stomach, through the action of the mucus present in this viscus.

In support of this view, I adduce the following experiments :—

Expt. To a full grown dog, fasting, two grammes of urea were administered dissolved in water. Fifteen minutes afterwards, the animal was killed by injecting a solution of woorara under the skin. The stomach was opened, and found to contain nothing but a quantity of thick, tenacious

¹ The Cause of the Coagulation of the Blood. Astley Cooper Prize Essay for 1856. London, 1858.

² Archiv des Vereins für gemeinschaftliche Arbeiten, Zweiter B. 1856, S. 120.

³ North American Medico-Chirurgical Review, vol. iv. No. 1, 1860, p. 149. (From Dublin Quarterly Journal of Med. Science, Nov. 1859, p. 425.)

mucus, which exhaled a strong ammoniacal odour. On bringing a glass rod moistened with chlorhydric acid near it, the dense white fumes of chloride of ammonium were formed, and crystals of this substance appeared on a slip of glass used in the manner before specified.

Expt. To another dog, somewhat smaller than the other, five grammes of urea were given in the same manner as before, and at the end of ten minutes the animal was killed by dividing the medulla oblongata. The stomach was immediately opened. It contained a few fragments of bone, some pieces of undigested meat, and a quantity of thick mucus. The mass was of feeble alkaline reaction, and evolved a barely perceptible ammoniacal odour. The presence of ammonia was further established, as in the first instance.

The contents were then examined for urea by the process hereafter detailed, and this substance was shown to be present in considerable amount. Owing to an accident, it was not weighed.

If, however, the animal has eaten largely a short time previous to the injection of the urea, the change into carbonate of ammonia does not occur, and if the animal be killed soon after the administration of the urea, this substance is found intact in the stomach. If, however, sufficient time has elapsed, it is absorbed into the circulation and excreted by the kidneys.

The following experiments are adduced, as tending to establish these propositions :—

Expt. A full grown dog was fed largely on animal food, and thirty minutes afterwards two grammes of urea were administered to it. Fifteen minutes after taking the urea it was killed by section of the medulla oblongata. The contents of the stomach were of acid reaction. On testing for ammonia by Richardson's method, a negative result was obtained. Neither were fumes of chloride of ammonium formed by the proximity of chlorhydric acid.

Expt. A full grown dog was confined and fed during three days on raw beef, two and a half pounds being given to him in the twenty-four hours. A uniform quantity of water was allowed. The urine of each period of twenty-four hours was collected, and tested for urea by Liebig's process with the nitrate of mercury. On the fourth day, the animal was fed as before, but with each meal two grammes of urea were administered. Six grammes were administered in all. The immediate effect was to increase to a considerable extent the amount of urine eliminated, which contained an augmented quantity of urea. The following table exhibits the results:—

	1st day.	2d day.	3d day.	4th day.
Quantity of urine .	815 c. cm.	795 c. cm.	891 c. cm.	1073 c. cm.
Urea	15.25 gram.	14.75 gram.	15.10 gram.	20.22 gram.

Gallois,¹ from experiments on rabbits, arrived at a similar conclusion, and has also shown that urea acts as a violent poison on these animals when injected into the stomach in sufficiently large amount. A train of symptoms was induced similar to those of uræmia, of which the animals died. During the progress of these symptoms, there was no ammonia in the breath.

¹ Op. cit.

I cannot therefore perceive, from a consideration of Frerichs' experiments, and from those performed by myself, as well as from the other evidence adduced, that there are any facts to warrant us in concluding that urea is transformed whilst in the blood into carbonate of ammonia.

As has been already stated, Frerichs performed another series of experiments, in which the carbonate of ammonia was directly introduced into the blood. In this series, a filtered solution containing from one to two grammes of the carbonate was employed. The first experiment, from which the others do not materially differ, is detailed by Frerichs as follows :—

“No. 1.—Into the jugular vein of a strong, full-grown dog, the solution was very slowly injected. The dog moaned and fell into deep stupor, broken occasionally by convulsions. The respiration was quickened, and the expired air was loaded with ammonia. The coma lasted for three hours, after which the animal regained its ordinary liveliness. During the coma, ineffectual efforts to vomit twice occurred.”

The experiments of this series amounted to six in number. None of the animals subjected to them died—a fact which, to say the least, is not one calculated to support Frerichs' hypothesis.

That carbonate of ammonia, when introduced directly into the circulation, produces considerable disturbance in the phenomena of life is doubtless correct. It is, however, excreted so rapidly by the lungs, that the most enormous quantity may be injected without death ensuing. Thus Dr. Steiner,¹ late of the United States Army, performed some years since a series of experiments, some of which show this very strikingly. In one instance, two ounces of strong aqua ammoniæ, diluted with an equal amount of water, were introduced into the circulation of a horse, and although the animal suffered severely for a few minutes, it soon rose from its feet, and trotted about as though nothing had happened.

I have myself² also experimented with reference to this point, and have injected as much as sixty grains of carbonate of ammonia into the jugular vein of a dog in normal condition. Convulsions ensued, and ammonia was exhaled from the lungs. The animal, however, recovered perfectly in a short time. Urea was also injected with a like ultimate result. The symptoms observed were, however, by no means identical in both cases.

In a second series of experiments performed, and detailed in the same memoir, the kidneys were removed, and in these animals death followed in a few hours after the injection of ammonia and other substances. From these experiments it is apparent that carbonate of ammonia possesses no pre-eminence as a toxical agent over substances not regarded as poisonous. The investigations, however, are not sufficiently extensive to warrant the formation of decided conclusions from them. It is perceived, however,

¹ Medical Examiner, vol. v. No. 11, N. S., 1849, p. 644.

² North American Medico-Chirurgical Review, vol. ii. p. 291.

that when urea was injected it was not decomposed into carbonate of ammonia either in unmutilated dogs or in those deprived of their kidneys.

In Bright's disease, there is a condition of system present very similar to that existing in animals the kidneys of which have been removed. In persons, therefore, suffering from this affection, carbonate of ammonia, if injected in large quantity into the blood, might cause death, or, even if retained in this fluid to any very great extent, the same result might follow. As, however, we have no proof that there is any accumulation of this substance in the blood during the progress of the disease in question, we cannot ascribe uræmic intoxication to its influence.

In the breath of persons labouring under Bright's disease, Frerichs constantly detected ammonia, and this circumstance is advanced as an additional argument in favour of his hypothesis. Allusion has already been made to the fact that ammonia can generally be found in the pulmonary exhalation of dogs. It is perhaps even a more constant constituent of the human breath, even in that of persons who, as far as can be perceived, are in perfect health. Richardson,¹ in many examinations, failed to find it only in one person. Schottin² detected it in the breath of persons suffering under other affections than Bright's disease, and in sixteen cases of uræmic poisoning found ammonia in the breath but once, and then, he thinks, it was probably derived from the mouth.

Mettenheimer³ found white fumes produced by a rod moistened with chlorhydric acid, as readily with the breath of healthy persons as with that of subjects of uræmia; and, in a note to his memoir, Beneke states that he has arrived at similar results. Viale and Latini,⁴ by numerous delicate experiments, have also shown that with each act of expiration ammonia is exhaled from the lungs of the healthy human subject.

From my own investigations, it has resulted that I have scarcely ever failed to find ammonia in my own breath, or in that of very many healthy persons whom I have examined.

I have thus considered, at some length, the arguments and experiments brought forward by Frerichs in support of his theory, and have, I think, sufficiently shown that it is not tenable. It is because, if generally adopted, it may lead to very grave errors in diagnosis and practice, that I have deemed it important to show how many facts tend to disprove the hypothesis in question.

Another view of the cause of uræmia, inferentially supported by Ber-

¹ Op. cit.

² Vierordt's Archiv. 1853, Heft i. s. 170.

³ Archiv des Vereins für gemeinschaftliche Arbeiten u. s. w. Band 1, Heft iv. 1854, p. 605.

⁴ American Journal of the Medical Sciences, April, 1855, p. 438. From L'Union Médicale, t. viii. No. 98, Août 1854.

nard,¹ requires some notice. According to this hypothesis, the condition in question is produced by decomposition of the tissue of the kidneys, and the retention in the blood of the elements arising from their putridity. The experiments of Müller and Peipers,² and of Marchand,³ are adduced as tending to establish the correctness of this theory. These observers induced mortification of the kidneys by ligating the renal nerves, and though it is asserted that death ensued from uræmia, it is by no means clear that this condition was not due to the cessation of the function of the kidneys, rather than to the entrance into the blood of septic matters from the mortified organs. Moreover, when the kidneys of an animal are removed, there is certainly no retention in the blood of putrid matters arising from their decomposition, and yet death invariably follows. The entrance into the blood of substances in a state of putrefactive fermentation would more probably induce a pyæmic or typhoid condition of the system than one of uræmia. Finally, in Bright's disease, of which uræmia so frequently forms a prominent concomitant, pathology shows that the existing condition of the kidneys is not one at all analogous to putrefaction.

It is perhaps unnecessary to consider other objections which are applicable to this hypothesis.

The theory which much observation and numerous experiments have led me to think most probably correct, is that which ascribes uræmic intoxication to the direct action of the elements of the urine retained in the blood upon the brain and nervous system, in a manner which we do not at present understand. Of these elements, we have strong reasons for deeming urea the most poisonous.

It is true, that urea has been directly introduced into the circulation of healthy animals without death ensuing. Thus Vauquelin and Segalas⁴ injected a solution of this substance into the veins of dogs and cats, without the production of any remarkable effect other than an increase in the amount of urine excreted. They then injected urine, and death followed with symptoms of uræmia. Hence they drew the conclusion, that it is the urine as a whole which, when retained in the blood, induces toxication.

Frerichs⁵ thinks that death in these cases arose from the urine being unfiltered; that is, from the mucus and epithelium derived from the urinary passages. He has repeatedly injected from twenty to forty grammes of filtered urine, sometimes even adding urea to it, into the veins of animals, without causing death. An increased quantity of urine was evacuated, but the normal condition of the organism was not otherwise disturbed. A

¹ Leçons sur les Propriétés Physiologiques et les Alterations Pathologiques des Liquides de l'Organisme. Paris, 1859, t. ii. p. 34 *et seq.*

² Archiv. für Physiologie, 1836.

³ Journal für practische Chemie, Band xi. s. 149.

⁴ Journal de Physiologie de Magendie, t. ii. p. 354.

⁵ Op. cit., p. 106.

warm saturated solution of urate of soda, likewise produced no untoward result. The amount of urea excreted by the kidneys was, however, greatly increased.

Other observations have also been made relative to the point in question, and leading to the same general conclusions. It may therefore be regarded as a well-established fact, that urea, in considerable amount, may be introduced into the blood of healthy animals, without death being necessarily produced.

It has been definitely shown that urea is a normal constituent of the blood. It has been found in the chyle, the lymph, the saliva, the bile, the aqueous and vitreous humours, the perspiration, the liquor amnii, the fluid of blisters, in dropsical effusions, in fecal evacuations, and even in the milk. It cannot therefore be regarded as poisonous, unless when, from defective excretion, it accumulates in the blood.

The amount of urea formed within the organism is, as has already been shown, subject to very great variation; but so long as the kidneys continue to perform the function of elimination in accordance with the requirements of the system, the normal balance between the urea and the blood is not disturbed. Thus it is that urea, and even urine, may be directly introduced into the circulation without inducing continued uræmia; for so soon as these substances reach the kidneys (as Vauquelin and Segalas, Frerichs, and many other investigators have shown), there is an increased elimination of urine, they are expelled from the body, and the coma, convulsions, and other accompaniments of toxication cease.

Occasionally, it happens that when the kidneys do not properly deplete the blood, other organs assume their office, and then uræmic intoxication does not occur.

Several cases are on record in which, from total suppression of the function of the kidneys continuing for a long time, this condition existed. One of the most remarkable of these is that reported to the French Academy of Sciences, several years since, by Monte-Santo,¹ of Padua, the accuracy of whose statements was confirmed by MM. Graefe and Frank from personal observation. In the case in question, there was complete constipation of the bowels and suppression of urine for fourteen years. There was always vomiting in from two to five hours after each meal, and about once a month a large quantity of fecal matter was discharged in the same manner. Although it is stated that there was no odour of urine in these egesta, it is more than probable that the stomach vicariously performed the function of the kidneys, and perhaps the skin also shared with it this office. Some years previously, a case had been reported to the Academy, in which there had been no discharge of feces or urine (by the ordinary channels, at least), for a period of seventy-two years.

¹ Medico-Chirurgical Review, July, 1833 (American edition), p. 236.

Whatever doubt may be attached to such cases as the above, it is very certain that in several diseases it frequently happens that other organs than the kidneys eliminate some of the elements of the urine from the blood. This is especially the case in Bright's disease, in yellow fever, and in cholera. Immediately previous to the accession, and during the continuance of the stage of collapse, in this last named disease, the cutaneous transpiration often contains urea, which is deposited by evaporation on the skin. I have frequently had occasion to notice this circumstance, and in one case in particular, which fell under my charge, the skin of the face, chest, and arms presented the appearance of being dusted with a fine white powder from the large quantity of urea which covered it. The urine contained scarcely a trace of this substance, but it was found both in the fluid vomited and that discharged *per anum*. I do not cite this case as one at all singular, but merely for the bearing which it has upon the subject under consideration. In the numerous cases of cholera which have fallen under my observation, I have generally, whenever an examination was instituted, detected urea in the rice-water discharges from the stomach and bowels.

The experiments of Bernard and Barreswil,¹ which show that when the kidneys are removed from an animal urea is excreted by the gastro-intestinal canal, may also be adduced. These observers extirpated the kidneys from dogs, and found that in those animals which survived but a short time no urea was to be detected in the blood, but that the matters ejected by vomiting and purging contained large quantities of ammonia, the product of the decomposition of the urea through the action of mucus and other gastro-intestinal secretions. If, however, death did not soon follow, the stomach and intestines lost their vicarious office, and then urea was found in the blood.

The fact that urea may occasionally exist in large quantity in the blood without giving rise to uræmic intoxication, is no proof that this substance is not generally poisonous. No one will deny the poisonous properties of arsenic. Dr. Taylor² gives the opinion, that a medical witness would be justified in stating it to be fatal in doses of two or three grains, yet subsequently refers to cases in which half an ounce, an ounce and a half, and even two ounces, had been taken without causing death. In the first of these instances, there was not even vomiting. So in relation to opium, as small a quantity as four grains produced death in a robust man, whilst on the other hand, as much as five ounces of laudanum has been taken without even causing sleep. Similar instances might be brought forward in relation to almost every other poisonous substance.

It may perhaps be objected, that in such cases the poisons were not ab-

¹ Sur les voies d'élimination de l'urée après l'extirpation des reins. Archives Générales de Médecine, 1847, t. xiii. p. 449. Also, Leçons sur les Liquides de l'Organisme, &c., t. ii. p. 36.

² On Poisons. Second American edition, 1859, p. 341.

sorbed into the blood; but toxic agents have been introduced directly into the circulation, and like differences in the extent of their action have ensued. I might bring forward numerous examples in support of this assertion, but it will probably be sufficient to recall the fact of the total insusceptibility of some persons to the action of the vaccine virus.

In Bright's disease, the disorganization of the kidneys is generally of slow progress. They continue to perform their function, though imperfectly, and consequently the amount of urea contained in the blood is not, in the first stages, very excessive. Its accumulation is gradual, and there is therefore time for the system to become in a measure habituated to its presence in such an amount that, if suddenly introduced into the blood and not eliminated, uræmia would in all probability ensue. We find this ability of the system to adapt itself to gradual changes generally present in all animals, and with reference to the action of poisonous substances exceedingly well marked. Thus, by progressively increasing the doses, large quantities of arsenic, opium, strychnia, hydrocyanic acid, and other toxical substances may be taken without the production of the least poisonous effect.

When, however, in Bright's disease, the structure of the kidneys becomes so greatly disorganized as to unfit them entirely for depurating the blood, the elements of the urine continue to accumulate, and if not otherwise excreted, almost invariably give rise to uræmia.

As to the assertion that uræmic intoxication may exist without the accumulation of urea in the blood, I have only to say that there is no evidence whatever to support such a conclusion.

The following investigations were undertaken with the hope of being able to contribute somewhat to a fuller and more exact understanding of the cause of uræmia. I have endeavoured to avoid every source of fallacy, and though (knowing the difficulties which attend researches of this character) I can perhaps scarcely assume to have succeeded, I am, nevertheless, unaware of any circumstances which would invalidate the conclusions drawn.

To say that I entered upon the inquiry without certain preconceived opinions would be far from correct. That such views as I had conceived have, however, blinded me to the truth, or warped my judgment of things as they actually were, I do not believe. Theories are true but for the time being, and physiological hypotheses are even more ephemeral than any others. We should therefore be prepared to yield our convictions without regret, when they do not accord with the results of experiments better devised and more accurate than our own, for only by so doing can we entitle ourselves to be considered useful labourers in the fields of science.

The chemical processes used in the several determinations were as follows :—

In examining the *blood* for urea, a weighed portion was mixed with its

volume of strong alcohol, and evaporated over sulphuric acid or chloride of calcium, *in vacuo*, to dryness. The residue was pulverized, and extracted with cold alcohol. The alcoholic extract was filtered, and carefully evaporated to dryness at a low temperature in the water-bath, and the residue washed repeatedly with small portions of ether. The ether extract was filtered and evaporated to dryness. Nitric acid was then added to the residue, the whole thrown upon a filter of known weight, and subjected to strong pressure. It was then dried at 100° C., and weighed. The difference between the weight of the whole and that of the filter gave the amount of nitrate of urea, and from this the quantity of urea was calculated.

Whenever the amount of urea was too small to determine quantitatively, the dried ether extract last obtained, as in the foregoing process, was placed upon a glass slide, and nitric acid added to it under the microscope. The production of rhombic and hexagonal tablets, the opposite acute angles of which measured 82° , was deemed sufficient evidence of the presence of the substance sought for.

The same methods were used to determine the existence of urea in the vomited and fecal matters, whenever they were examined for it.

For ascertaining the amount of urea in the urine, Liebig's volumetric process with the proto-nitrate of mercury was always employed.

In the determination of ammonia, Richardson's process, already detailed, or Reuling's, was made use of. Whenever negative results were obtained by the one, recourse was always had to the other.

In the first place, I was desirous of ascertaining more definitely than had hitherto been done, the action of urea when injected into the blood of sound and healthy animals.

Expt. A large adult dog, weighing 65 pounds, was fed for three days on fresh meat. During this period, ammonia was constantly found in the breath.

On the fourth day the jugular vein of the left side was opened, and a sufficient quantity of blood abstracted. 100 grammes of this contained 0.019 gramme of urea. Ammonia was present, as it was likewise in the expired air.

The urine passed on this day amounted to 1025 cubic centimetres, and contained 11.28 grammes of urea.

The food was the same as on the preceding days.

On the morning of the fifth day, at 9 A.M., 3 grammes of urea dissolved in 30 cubic cen. of distilled water, were injected into the jugular vein. The animal, from the very first, appeared to suffer pain. It moaned; the breathing became laboured; and it trembled violently, as if from fright or cold. At the end of about 20 minutes, the animal became more quiet, and even appeared to be somewhat stupefied. After nearly an hour had elapsed, convulsions ensued. These were confined almost entirely to the posterior extremities, though at times the other portions of the body were in spasms.

At 10 o'clock, whilst the convulsions were still present, I abstracted 100 grammes of blood. It contained 0.135 grammes of urea. Ammonia was

also present, though in no larger amount than on the previous day. Examined microscopically, the red blood-corpuscles were found to be of normal size, shape, and colour. They appeared, however, to be diminished in quantity. The white corpuscles were very evidently increased in amount.

The animal continued to be convulsed till about 2 o'clock P. M., when coma ensued. This lasted $3\frac{1}{2}$ hours. The dog then awoke, and passed a large quantity of urine. It amounted to 280 cubic centimetres, and contained 2.15 grammes of urea.

Before, during, and after the convulsions, ammonia was exhaled with the breath.

Immediately on the dog awaking, I again abstracted 100 cubic centimetres of blood from the jugular vein. This contained 0.014 of urea.

The total amount of urine voided on this day was 1381 cubic centimetres, containing 14.63 grammes of urea.

The dog ate as much on this day as on any previous one. It recovered perfectly.

Expt. For this experiment, a dog, weighing $38\frac{1}{2}$ pounds, was used. As in the preceding experiment, it was fed for three days on fresh meat. During this period, at only one examination (at 10 o'clock A.M., on the second day), was ammonia detected in the expired air. On the fourth day, 100 grammes of blood were abstracted from the jugular vein, and found to contain 0.027 gramme of urea. Ammonia was also present, and likewise in the pulmonary exhalation.

The urine voided on this day amounted to 834 cubic centimetres, and contained 4.09 grammes of urea.

On the fifth day, at 10 o'clock A. M., 5 grammes of urea dissolved in 30 cubic centimetres of distilled water, were injected into the jugular vein. No immediate effect was produced. After the lapse of 45 minutes, there were slight spasms of the muscles of the eyelids; and 50 minutes after the injection, a severe general convulsion ensued. The vein was now reopened, and 100 grammes of blood taken. Upon analysis, this was found to contain 0.254 grammes of urea. The convulsions continued with great violence for 15 minutes. Coma followed, and lasted till 6 o'clock P. M., when the animal died. There was no excretion of urine after the injection of the urea. The breath was examined every hour for ammonia, but at no time was it detected; neither was it present on this day before the urea was injected. It was, however, found in the blood last drawn. There was neither vomiting nor purging.

Immediately after death the post-mortem examination was commenced.

The substance of the brain appeared to be perfectly healthy; but there was considerable injection of the vessels of the meninges. The ventricles contained about 15 cubic centimetres of serous fluid. Urea was detected in this by the process mentioned, and microscopical examination. It was likewise found in the blood from the sinuses.

The vertebral canal was laid open, and the spinal cord examined. Its substance presented a normal appearance, but there was some congestion of the vessels of its membranes.

The chest contained a small quantity of serous fluid. The lungs were congested, but were otherwise healthy. The heart was of normal size, and did not appear to be in the least diseased. It contained a considerable quantity of fluid blood; 100 grammes were collected from it and the large vessels. The urea in this quantity amounted to 0.873 grammes.

Upon microscopical examination of this blood, the red corpuscles were

found to present a crenated margin, and to be in decidedly less than the normal quantity. The white corpuscles were very much increased in quantity; as much as in well-marked leucocythemia.

The cavity of the peritoneum contained a small quantity of serous liquid. The membrane was in places slightly congested.

The liver was healthy in appearance, but the spleen was considerably enlarged, and contained much more than the normal quantity of blood. The tissue of this latter organ, when examined microscopically, was found to present several important deviations from the normal structure. The Malpighian corpuscles were almost entirely absent, and there was a very great increase in the number of parenchyma cells. These latter were much larger than I have ever found them in the spleen of the dog. The red blood-corpuscles in the splenic blood were generally aggregated in groups, and were of irregular forms.

The stomach was opened, and presented nothing abnormal. The contents, consisting of mucus with a few pieces of bone, were of alkaline reaction, and contained both urea and ammonia, the latter in considerable amount.

The kidneys were enlarged and very much congested. Upon cutting into them, the blood poured out from innumerable orifices. There was no obstruction to either the renal arteries or veins that was discovered after death. The tissue of the kidneys, when submitted to microscopical examination, showed excessive congestion of the capillaries, and enlargement of the Malpighian bodies. Into many of these latter extravasation of blood had taken place, and the tubes were gorged with this fluid.

The bladder contained a small quantity of bloody urine.

Death in this case was, I think, obviously due to non-elimination of urea, and perhaps of the other elements of the urine, through excessive hyperæmia of the kidneys. The cause of the kidney affection I do not know. It was undoubtedly caused either directly or indirectly by the injection of the urea, for up to the time of that operation the function of these organs was perfectly effected.

The experiment cannot but be regarded as exceedingly instructive. There was complete arrest of excretion from the kidneys, and the blood, besides retaining the elements of the urine, received in addition a large quantity of urea, which remained in the organism. In many respects, therefore, the experiment resembled those in which the kidneys have been extirpated, and urea subsequently introduced into the blood. In all such experiments, death has invariably taken place within a few hours. There is, however, this important difference, that the system was saved the shock of a serious operation, and therefore one source of error was eliminated.

In relation to the alteration in the form of the blood-corpuscles, it can scarcely (having Kölliker's experiments in view) be ascribed to the direct physical action of the urea, as the proportion of this substance present in the blood was altogether too small (less than 1%) to effect the change. Kölliker¹ found that a solution containing 30% of urea caused the red cor-

¹ Zeitschrift für wissenschaftliche Zoologie, B. vii. s. 183. Also, Quarterly Journal of Microscopical Science, vol. iii. 1855, p. 289.

puscles of the frog to assume the form of stellate cells, and finally to melt down and disappear. These alterations were also produced, though much more slowly, by solutions of 15% and 12%. When weaker solutions were used, they were not caused. Human blood-cells were only rendered smaller and colourless. The phenomenon observed in the case under consideration, taken in connection with the diminution of the number of red and increase in that of the white corpuscles, must probably be ascribed to some defect in the process of sanguification.

For the purpose of ascertaining the effect of introducing repeated quantities of urea into the blood, I proceeded as follows :—

Expt. A dog weighing forty-eight pounds was fed as the others for three days. Ammonia was always found in the breath.

On the fourth day, at 10 o'clock A. M., one hundred grammes of blood were abstracted, and upon examination found to contain 0.021 grammes of urea. The urine voided during the twenty-four hours amounted to 1224 cubic centimetres, and contained 8.15 grammes of urea. Ammonia was present both in the blood and expired air.

The following morning, at 10 A. M., I introduced into the jugular vein five grammes of urea, dissolved in thirty cubic centimetres of distilled water. No immediate effect was produced, the animal remaining perfectly quiet. At 11.15 convulsions ensued. One hundred grammes of blood were now taken, and yielded 0.193 grammes of urea. The convulsions, at first slight, became more violent. They continued about twenty minutes, and were succeeded by stupor. At 12 M., I injected, as before, five grammes of urea. Immediately afterwards, the animal voided 365 cubic centimetres of urine, in which were contained 3.17 grammes of urea. The coma continued, and at 2 P. M. I again injected five grammes of urea into the blood. At 3.20 P. M. the dog passed 425 cubic centimetres of urine, containing 4.06 grammes of urea. The coma was still present. At 4 P. M., I injected ten grammes of urea, dissolved in thirty-five cubic centimetres of distilled water, into the blood. The stupor was now very profound, the heart beat slowly, and the respiration was laboured and stertorous. I again, at five P. M., abstracted one hundred grammes of blood for examination. It contained 1.683 grammes of urea.

At 5.20, the dog evacuated *per anum* a small quantity of yellow, serous fluid. Urea and ammonia were detected in it.

At 5.45 the animal died, having in eight hours received directly into the blood twenty-five grammes of urea.

Ammonia was found in the breath during the whole course of this experiment, and likewise in the blood.

The urine voided from the commencement to the end of the experiment amounted to 890 cubic centimetres, and contained 7.23 grammes of urea. In the twenty-four hours, there were evacuated 1625 cubic centimetres of urine, in which were contained 12.37 grammes of urea.

On post-mortem examination, the brain and spinal cord were found healthy; the membranes of both were of perfectly normal appearance. About five cubic centimetres of fluid were collected from the ventricles of the former. By simply placing a drop on a glass slide, and adding nitric acid, crystals of nitrate of urea were formed.

The lungs were found congested, but there was no effusion into the pleural cavities. The pericardium was very much congested. The heart contained

a large quantity of blood. Its lining membrane, both in the auricles and ventricles, was redder than natural.

One hundred grammes of the blood from the heart and large vessels contained 1.385 grammes of urea. The blood-corpuscles were of normal size and shape, but, as in the former experiments, were remarkably diminished in number, whilst there was an increase in the number of white corpuscles.

The peritoneal membrane was found congested in patches. The spleen was enlarged, and contained a large quantity of blood. Beyond this the structure, when examined with the microscope, exhibited no abnormal appearance. The kidneys were healthy.

The stomach contained about one hundred and fifty cubic centimetres of fluid, resembling the rice-water discharges of cholera. The intestines also contained a quantity of the same kind of fluid. Both urea and ammonia were present in it.

For the purpose of still further determining the action of large quantities of urea, when introduced into the circulation, the following experiment was performed :—

Expt. A small adult dog, weighing thirty and a quarter pounds, was fed as the others, for three days, on fresh meat, before any investigations were commenced. On the fourth day, at 10 o'clock A. M., one hundred grammes of blood were drawn from the jugular vein. This quantity contained 0.024 grammes of urea. The total amount of urine evacuated during the twenty-four hours was 830 cubic centimetres, containing 5.12 grammes of urea. On this and the preceding days, ammonia was always found in the breath. The food remained the same.

On the fifth day, at 10 A. M., twenty-five grammes of urea, dissolved in thirty cubic centimetres of distilled water (thus forming a nearly saturated solution), were injected into the jugular vein. The animal lay down quietly in its box, and at first did not seem to be greatly disturbed. After a few moments slight twitchings of the muscles ensued, and at 10.30 there was a strong convulsion. Some of the spasms, subsequently, were decidedly tetanic. During the convulsions, I reopened the jugular vein and allowed 100 grammes of blood to flow out. The amount of urea contained therein was 2.005 grammes. The convulsions lasted with undiminished violence till 11.10 A. M., when 481 cubic centimetres of urine, containing 7.50 grammes of urea, were excreted. They then became slighter, and at about 12 M. were succeeded by coma. This continued without intermission till 4.15 P. M., when the animal quietly died in most profound stupor. Two hundred and twenty-five cubic centimetres of urine, containing 3.12 grammes of urea, were excreted a short time before death. There was neither vomiting nor purging. Ammonia was constantly found in the breath, but not in greater quantity than during the previous days.

The blood drawn during the convulsions was examined with the microscope. The red corpuscles were altered in shape, and had become much paler than natural. Scarcely one could be found which was not more or less irregular in outline. They, besides, appeared to have lost their ordinary consistence, and when two or more came together they fused, forming an irregularly formed mass. It was thus impossible to determine microscopically their relative numbers. The white corpuscles were very much increased in quantity. The blood coagulated firmly.

The cavities of the body were opened immediately after death. The

membranes of the brain were found in a state of intense congestion, and the sinuses and large vessels at the base of the cranium were gorged with blood. The substance of the brain, when cut into, exhibited a uniform pink tinge from excess of blood, and the red spots indicating the situation of capillaries were greatly increased in number. This was the first case in which the substance of the brain presented direct evidence of hyperæmia. About twenty cubic centimetres of fluid were collected from the ventricles.

The membranes of the spinal cord were likewise congested, especially in the lumbar region, though more or less throughout their whole extent. Into the cavity of the arachnoid there was an effusion of serous fluid, amounting to fifteen or twenty cubic centimetres.

The lungs were also congested, and there was a considerable amount of bloody fluid effused into the pleural cavities. The pericardium contained a quantity of liquid. The right side of the heart was distended with blood. The left contained but a small quantity. The heart did not exhibit any indications of pre-existing disease of any kind. I was prevented examining the blood chemically. The blood-corpuscles presented the same appearances as those in the blood abstracted before death.

The peritoneum was not diseased; there was no effusion. The liver was of normal appearance. The spleen was enlarged, and contained a large quantity of blood. The red corpuscles were here found almost entirely broken down into a liquid substance. No Malpighian corpuscles were discovered. Large quantities of acicular crystals were scattered through the substance of the spleen, and were visible by microscopical examination.

The kidneys were slightly congested, and some of the tubes contained blood. There were no other appearances of disease.

Two other experiments, similar to the last, were performed. As the results were almost identical, I refrain from detailed descriptions of them.

From the foregoing experiments, it is perceived that there is a limit to the power of the system to eliminate urea, and that when this substance is introduced into the blood in large quantity, it causes death by uræmia. By this I mean that the urea induces such an abnormal condition of the blood, that the brain primarily, and subsequently other organs (the kidneys included), are brought into an abnormal condition, and are thereby prevented performing their functions. From the results of the *post-mortem* examination, it is apparent that this state is one of congestion. I am therefore disposed to think that if the brain had been able to resist the toxic power of the urea for a considerably longer period, the kidneys would have eliminated the surplus urea, and death in these latter experiments would not have ensued.

It is well known that in Bright's disease death frequently occurs from congestions and inflammations of important structures. Thus this termination may be caused by œdema of the glottis, by pericarditis, by pneumonia, by peritonitis, by apoplexy, &c. Have we not reason to regard these several affections as due to an abnormal condition of the brain and nervous system, induced by the retention in the blood of excrementitious matters which in health are removed?

It is, I think, very evident that in neither of the foregoing experiments

was there the least reason to suppose that there was any decomposition of urea into carbonate of ammonia. In the second experiment, this substance was not found in the breath after the injection of the urea into the blood, although it was present at two examinations before this operation.

The alterations in the blood observed with the microscope are very important, and constitute one link in the chain, connecting the retention of urea with derangement of the brain and nervous system. They, perhaps, show that it is not necessary that a toxic condition of the blood should consist altogether in disturbances of the chemical balance existing between its several component parts. The morbid condition of the spleen which was found to exist, is also an interesting circumstance, taken in connection with the changes in the form of the red, and alterations in the relative number of these and the white corpuscles.

Leaving, for the present, the further consideration of these experiments, I proceed to the detail of those constituting the second series, and having relation to the effects following ligature of the renal vessels, or removal of the kidneys:

Exp. A large adult dog, weighing $68\frac{1}{4}$ pounds, was selected. At 10 A. M. the breath was examined for ammonia, and this substance was found to be exhaled from the lungs in considerable quantity. 100 grammes of blood taken from the jugular vein contained 0.026 gramme of urea.

At 3 P. M. the animal was placed under the influence of chloroform, and the kidneys removed, the abdomen being opened to the smallest possible extent. Scarcely a drop of blood was lost; the anæsthesia passed off without the least untoward effect.

The next morning the dog was in apparently good condition, but manifested no desire to eat, or to move about. At 3 P. M., twenty-four hours after ablation of the kidneys, 100 grammes of blood were drawn, and found to contain 0.083 gramme of urea. The animal refused all food; ammonia was constantly in the breath.

On the third day, at 10 A. M., the dog seemed weaker. Up to this period, however, there had been no convulsion, or coma, neither was there any vomiting or purging. At about $2\frac{1}{2}$ P. M. there was a slight spasm, succeeded in a few moments by a violent general convulsion. At 3 P. M. 100 grammes of blood gave 0.093 gramme of urea. At 4 P. M. the animal vomited a quantity of alkaline mucus, containing both urea and ammonia—the latter in large amount. After the vomiting, the convulsions abated in violence, but soon became as intense as at first. At intervals, however, the animal was free from spasm, and appeared to be also free from pain and uneasiness. At about 8 P. M. coma ensued, alternating with the convulsions till $10\frac{1}{2}$ P. M., when it became persistent, and very profound. Death ensued at about 4 A. M. the next day, sixty-one hours after the removal of the kidneys.

The post-mortem examination of the body was commenced at 9 A. M. The membranes of the brain were intensely congested, and there was an effusion of about twenty-five cubic centimetres of serum into the cavity of the arachnoid. The substance of the brain, when cut into, exhibited a pinkish hue, and numerous bloody points appeared, showing enlargement, and increase in the number of capillaries. The sinuses and vessels at the base of

the brain were turgid. Fifteen cubic centimetres of fluid were found in the ventricles.

The spinal cord was apparently healthy, but its membranes were slightly congested. There was no abnormal amount of fluid found.

Both lungs were congested, and both pleuræ bore evidences of recent incipient inflammation. There was slight effusion. The heart was gorged with blood. The pericardium was healthy. 100 grammes of blood from the heart and large vessels contained 0.097 gramme of urea.

When submitted to microscopical examination, the blood taken from the heart exhibited appearances similar to those previously noticed. The white corpuscles were increased, and the red diminished in number. These latter were also of irregular shape, and decidedly paler than natural.

The peritoneum exhibited traces of recent inflammation; a small amount of bloody serum was found in its cavity.

The spleen was very much enlarged, being at least three times the size of the organ in its normal condition, as noticed when the kidneys were removed. Upon cutting into it, the substance was found entirely disorganized, and of semi-fluid consistence. No traces of Malpighian corpuscles were to be found, and the finer trabeculæ and many of the larger, were entirely detached from their connections. These, with masses of blood-pigment, broken-down corpuscles, a few muscular fibre-cells, and numerous acicular crystals of hæmato-crystallin, were all the morphological elements to be discovered. The parenchyma cells, white corpuscles, free nuclei, &c., had been destroyed.

The liver was also enlarged. It was not examined microscopically.

The stomach, on being opened, exhaled a strong ammoniacal odour. It contained a quantity of yellow, alkaline mucus. Ammonia was present in large quantity, and traces of urea. The intestines contained a like substance.

Exp. At 9 o'clock A. M., 100 grammes of blood were abstracted from the jugular vein of a large dog, weighing $60\frac{1}{2}$ pounds. The urea contained therein amounted to 0.014 gramme. Ammonia was present in the breath. At 1 P. M. the renal arteries were ligated, the animal being under the influence of chloroform. As soon as the anæsthesia passed off sleep ensued, from which the dog did not awake for several hours. At 10 P. M. it appeared to be in good condition, and lapped a little milk. It was quiet, but when spoken to, manifested undoubted signs of intelligence.

The following morning at 8 o'clock the animal was quite lively. It stood up, and even walked a few steps. It ate a little bread and milk. At 4 P. M. it was somewhat drowsy, though it could be easily roused by speaking loudly, or knocking on the side of its box. 100 grammes of blood taken from the jugular vein contained 0.038 of urea. Ammonia was present in the breath. The stupor continued to increase, and at about 8 P. M. was profound. When last seen for the night, at 12 M., the animal was in a very comatose condition.

The next day at 8 A. M., the coma was still present. 100 grammes of blood contained 0.043 gramme of urea. Ammonia was exhaled from the breath, but not to a very abnormal extent. The dog remained in the same condition till about $11\frac{1}{2}$ P. M., when it died, $58\frac{1}{2}$ hours after the ligation of the vessels. There was no vomiting, nor purging, and no visible spasms of any kind.

The following morning, at 9 A. M., I made the post-mortem examination. The membranes of the brain were congested, and about twenty-five

cubic centimetres of fluid were effused into the cavity of the arachnoid. The substance of the brain likewise exhibited evidences of having been in a hyperæmic condition. The ventricles were distended with fluid, which, however, owing to an accident, was not measured. The sinuses and vessels at the base of the cranium were turgid with blood. The spinal cord was not examined.

The pleuræ, the lungs, the pericardium, and heart, were healthy; 100 grammes of blood collected from the latter, contained 0.069 of urea. Examined microscopically, the white corpuscles were found in largely increased quantity, but no other abnormal condition was discovered.

The peritoneum was congested in spots, and exhibited evidences of recent inflammatory action. The spleen was large, and softer than natural. No Malpighian corpuscles could be found. Masses of extravasated blood, in larger quantity than usual, were met with. The liver was not markedly affected. The stomach and intestines contained the residue of undigested food, with some yellow, alkaline mucus. Ammonia and urea were both present.

From the foregoing experiments, it is seen that, after the removal of the kidneys, or ligature of the renal arteries, the amount of urea in the blood was increased threefold within a short period, and that there was no reason to suppose any conversion of this principle into carbonate of ammonia. The pathological changes are interesting, and congestion and inflammation of important organs are seen to be produced as well after ablation of the kidneys, as after the direct injection of urea into the blood, or during the course of Bright's disease. The immediate cause, in each of these instances, is probably the same—derangement of nervous influence through a morbid condition of the blood.

Four other experiments, similar to the two foregoing, were performed. In all, the quantity of urea in the blood was greatly increased after removal of the kidneys, or simple ligature of their arteries, and the post-mortem appearances were in general the same. In three of these there were convulsions and coma, in one coma alone—as in the second experiment of this series. In one experiment, the animal lived forty-nine hours after the operation, in one fifty-three, in one sixty-eight, and in one seventy-three hours. Ammonia was found in the breath both before and after the operation. In none was there any vomiting or purging.

But that these last-mentioned results do sometimes happen, there can be no doubt, and, in fact, judging from the investigations of these observers, they are usual concomitants. In one experiment which I performed subsequently to the above cited, they were present from the first, and doubtless by these means the urea was removed from the blood, and this fluid preserved in a comparatively normal condition. In no other way can we account for the lengthened continuance of life after the kidneys were extirpated. As this experiment is important in several respects, I give the details of it in full.

Expt. A dog weighing thirty-eight pounds was selected for operation.

Before the extirpation of the kidneys, the breath was ascertained to contain ammonia. In 100 grammes of blood were 0.009 grammes of urea.

The kidneys were removed at 9 A. M., the dog being under the influence of chloroform. After the operation, the animal fell into a quiet sleep, and did not awake for six or seven hours. At about 6 P. M. it ate a little bread and milk.

The following morning at 8 o'clock it was in apparent good condition, wagged its tail when spoken to, and ate quite freely of bread and milk. It remained in the same condition all day, manifesting, however, no desire to move out of its box, though it occasionally turned from one side to the other.

The next morning it was found that in the night a little mucus had been vomited. This contained ammonia, but no urea. No other circumstances worthy of note occurred during the day.

On the fourth day, at 7 A. M., the animal seemed somewhat uneasy from nausea. Several efforts to vomit occurred, but nothing was ejected. At about 4 P. M., there was a fecal evacuation of a thin, serous fluid, of a yellow colour. Ammonia was present, but no urea. Crystals of ammonio-magnesian phosphate in large quantities were visible by the microscope. No food was taken on this day.

At 7 o'clock the following morning, it was found that there had been both vomiting and purging of ammoniacal matters during the night. One hundred grammes of blood were abstracted at 9 A. M., and found to contain 0.011 grammes of urea.

On the sixth day, there were both vomiting and purging. The animal was sensible, but refused food.

On the seventh, eighth, and ninth days, vomiting and purging occurred several times. Ammonia and urea were present in each evacuation. On each of these days, a pint of milk was conveyed to the stomach through a tube.

On the tenth day, at about 12½ P. M., a slight convulsion occurred. A pint of milk was injected into the stomach. There was neither vomiting nor purging.

On the eleventh day, at 9½ A. M., another convulsion took place, more violent than the first. The animal was, however, still sensible.

On the twelfth day, at 7 A. M., the dog was found in a comatose condition. One hundred grammes of blood were abstracted at 12 M. This quantity contained 0.041 grammes of urea. The wound in the abdomen had nearly healed. The ligatures on the renal vessels came away. Coma was present during the whole day, and death took place some time in the night after 11 o'clock. The animal was cold the following morning at 7 o'clock.

Thus life had remained for at least 278 hours after the kidneys were extirpated.

Ammonia was detected in the breath on every day but the last.

The post-mortem examination was commenced at about 11 o'clock A. M. The membranes of the brain were apparently healthy, but there was slight sub-arachnoidal effusion. The substance of the brain was of normal appearance. The spinal cord and its membranes were healthy. The lungs were congested, and there were several recent pleuritic adhesions. The pericardium was in several places adherent to the heart. This latter organ was in a state of incipient fatty degeneration. It was full of uncoagulated blood, 100 grammes of which contained 0.046 grammes of urea.

The red corpuscles, both in this blood and in that abstracted on the day of death, were very much diminished in number, whilst the white corpuscles were correspondingly increased. The former were also broken down, and softer than is normally the case.

The peritoneal membrane was congested, and several intestinal adhesions had taken place. The spleen was enlarged, and felt like a bag of water. When the enveloping membranes were cut, the substance flowed out, like molasses in colour and consistence. Examined microscopically, nothing was perceived but shreds of white fibrous tissue, masses of decomposed blood-corpuscles, blood pigment, and large quantities of acicular crystals. The liver was enlarged, and in a state of congestion. The stomach and intestines contained nothing but a little mucus.

It is seen from this experiment that so long as vomiting and purging continued, there was no accumulation of urea in the blood, and no consequent uræmic intoxication. It was only when these ceased that the latter event ensued. It is therefore strongly confirmatory of the conclusion arrived at by MM. Bernard and Barreswil, which has been previously referred to. The connection between the retention of urea in the system and the occurrence of uræmia is so well marked, that it is difficult to deny to these events the relation of cause and effect.

In the third and last series of experiments, the kidneys were removed, and urea or urine, subsequently directly introduced into the blood.

Expt. From the jugular vein of a medium sized dog, 100 grammes of blood were abstracted at 10 o'clock A. M., and found to contain 0.021 grammes of urea. Ammonia was detected in the expired air. At 11 A. M. the animal was placed under the influence of chloroform, and the kidneys removed. At 12 M., five grammes of urea dissolved in thirty cubic centimetres of distilled water, were injected into the jugular vein. No immediately noticeable effect was produced. At 1½ o'clock P. M., convulsions of great violence suddenly ensued. One hundred grammes of blood from the jugular vein yielded 0.042 grammes of urea. A large quantity of highly ammoniacal fluid was vomited. It contained urea. The convulsions continued with undiminished violence till about five o'clock, when coma gradually ensued, and remained present till death, which occurred at about 8 o'clock P. M.

Before death, and during the height of the convulsions and coma, the breath was examined for ammonia, which, although constantly found, was not present in larger quantity than before the kidneys were removed.

At 7½ o'clock the following morning, the post-mortem examination was commenced.

The membranes and substance of the brain were healthy, but there was a little sub-arachnoidal and ventricular effusion.

The lungs were highly congested, as were also the pleuræ in several places. The pericardium was healthy, and the heart was of normal appearance. It contained a small quantity of uncoagulated blood. One hundred grammes taken from it and the large vessels, yielded 0.032 grammes of urea.

The peritoneum was anteriorly much congested, and there was about one hundred cubic centimetres of bloody serum in its cavity. The spleen

was hyperæmic, and its normal structure disorganized, as in the previous experiments. The liver was not perceptibly diseased.

The stomach and intestines contained a small quantity of alkaline fluid. It was highly ammoniacal. Urea was not detected in it.

Three other experiments, similar to the foregoing, were performed. The results were almost identical, and it is therefore, perhaps, unnecessary to refer to them more in detail.

In the two following experiments, urine was introduced into the blood.

Expt. A dog weighing somewhat less than forty pounds was used for this experiment. The breath examined for ammonia gave distinct evidence of containing it. Previous to removing the kidneys, it was determined that 100 grammes of blood from the jugular vein contained 0.017 grammes of urea.

Anæsthesia was induced by chloroform, and the kidneys extirpated at 9 o'clock A. M. As soon as the insensibility had passed off, one hundred cubic centimetres of fresh urine (voided during the induction of the anæsthesia), unfiltered, were injected carefully into the jugular vein. No immediate effect was produced. At 12 M., thirty cubic centimetres more of the same urine were introduced into the blood. The animal remained quiet in its box till about 2 o'clock P. M., when a slight convulsion ensued, lasting only a few seconds, and confined to the anterior muscles of the body. Shortly afterwards, another occurred more violent than the first. This was followed by others. At about 4 P. M. they ceased, and coma commenced to make its appearance. One hundred grammes of blood from the jugular vein were found to contain 0.030 grammes of urea. The coma continued till after 12 P. M. The dog was found dead the ensuing morning at 5 o'clock. There had been neither vomiting nor purging. Before death, the breath was frequently examined for ammonia, and always with affirmative results.

The post-mortem examination was made at 8½ o'clock A. M. The appearances observed were not materially different from those noticed in the case just detailed, except that the pericardium was congested, and there was some little effusion into its cavity; 100 grammes of blood from the heart and large vessels, contained 0.036 gramme of urea.

Exp. A dog, weighing $49\frac{3}{4}$ pounds, was the subject of this experiment. Ammonia was found in the expired air; 100 grammes of blood from the jugular vein gave 0.027 gramme of urea. The animal was at 10 A. M. brought under the influence of chloroform, and whilst anæsthesia was present, the kidneys were extirpated. When insensibility had passed off, 100 cubic centimetres of filtered urine—evacuated during the process of inducing anæsthesia—were injected into the jugular vein.

After the expiration of about two hours, convulsions ensued. These at first were slight, but more violent ones soon followed. They lasted till a few minutes before 3 P. M., when coma supervened; 100 grammes of blood taken from the jugular vein were found to contain 0.035 gramme of urea. The coma persisted, and at 8½ P. M. the animal died. There was no vomiting nor purging. Ammonia was found in the breath throughout.

The post-mortem appearances were not essentially different from those observed in the preceding experiment. The contents of the stomach were however acid, and did not contain ammonia.

From the experiments, constituting this last series, it is clearly seen that the introduction of urea, or filtered or unfiltered urine into the circulation of animals deprived of their kidneys, induces death more speedily than if such substances had not been thrown into the blood; for, we have perceived from the experiments of the immediately preceding series, that animals, the kidneys of which have been extirpated, or the renal arteries ligated, live for from forty-nine to 278 hours after the operation; whereas, in the series last detailed, death occurred in from eight to fifteen hours, in a condition of system not to be distinguished from that known as uræmia. Taken, therefore, in connection with the experiments of the first series, in which urea was introduced into the blood of sound animals, and with those of the second, in which, through extirpation of the kidneys, the elements of the urine were retained in the blood, and we have almost demonstrative proof that the resulting uræmia was directly due to the operation of these causes.

Taken as a whole, from an attentive consideration of the foregoing investigations, I think the following conclusions are legitimately deducible:—

1st. That the injection of urea, in limited quantity, into the blood of animals, produces a certain amount of disturbance in the nervous system, similar in its symptoms to the first stages of uræmia, but that this condition even disappears, if the kidneys are capable of so depurating the blood as to eliminate the toxic substance.

2d. That urea, when introduced into the circulation in larger quantity than can in a limited period be excreted by the kidneys, induces death by uræmia.

3d. That by ligature of the renal arteries, or removal of the kidneys, the elements of the urine being retained in the blood, render this fluid unsuitable to the requirements of the organism, and, consequently, induce a condition of system not essentially distinguishable from the uræmic intoxication of Bright's disease, or that caused by the direct introduction of urea into the blood. As, however, was pointed out by Bernard and Barreswil, so long as the urea, or the products of its metamorphosis, are discharged by the stomach or intestines, uræmia does not take place, but, that when these channels become closed, convulsions and coma are produced, and death soon follows.

4th. That the introduction of urea or urine into the circulation of animals, the kidneys of which have been ablated, shortens the life of such animals, as Frerichs and others have already shown.

5th. That there is reason to believe that the urine, as a whole, is more poisonous than a simple solution of urea, for, in those cases in which urine was injected into the blood, the amount of urea thus introduced was much smaller than that previously thrown in, in a pure state, and yet symptoms of as great intensity followed.

6th. That urea, or the elements of the urine, as a whole, induce such a condition of the nervous system, as strongly to predispose to congestion

and inflammation of the viscera, especially the lungs, pericardium, and spleen.

7th. That urea, when directly injected into the blood, or suffered to accumulate in this fluid by extirpation of the kidneys, deranges, in some manner, the process of sanguification, so as to disturb the normal relation of proportion existing between the white and the red corpuscles, and either to hasten the decomposition of these latter, or to interfere with the due removal from the blood of such as are broken down and effete.

8th. That there is no reason to suppose that, under the circumstances specified, urea undergoes conversion into carbonate of ammonia, but that, on the contrary, there is sufficient evidence to warrant the conclusion that no such process ensues. The fact that in the foregoing experiments a larger amount of urea was generally found in the blood taken from the body after death, than in that abstracted during life, is, of itself, conclusive against any such hypothesis.

ART. III.—*On the Cerebellum, as the Centre of Co-ordination of the Voluntary Movements.* By JNO. C. DALTON, JR., M. D., Professor of Physiology and Microscopic Anatomy in the College of Physicians and Surgeons, New York. (With four wood-cuts.)

OF all the physiological theories with regard to the peculiar function of the cerebellum, the only one which has been received with any continued and extensive favour, is that first advocated by Flourens. According to his opinion, there resides in the cerebellum, as a nervous centre, the power of co-ordinating or associating together the action of the different voluntary muscles, so as to give to this action a positive and definite direction. Flourens derived his conclusions from the result of direct experiment. In wounding or destroying the substance of the cerebellum, he found that the most palpable effect produced by such a mutilation was an immediate loss of co-ordinating power; so that the voluntary movements became at once irregular and uncertain in their character, while the perceptive faculties, and the senses generally, remain uninjured.

These results are, in point of fact, of constant occurrence, after sudden lesions of this part of the encephalon. There are few, if any, experiments which have been performed upon the nervous system, in which the phenomena produced are, at the same time, so striking and so invariable as in these. I have very frequently repeated them upon the pigeon, and have always found that, as soon as any considerable portion of the cerebellum has been injured, the animal shows an irregularity in its gait, in its posture, and in the movements of its head, neck, and wings; and this irregularity increases in proportion to the quantity of the nervous mass which is re-

moved, so that when nearly the whole of the cerebellum has been taken away, the animal can neither stand, walk, nor fly, and is only capable of making confused and ineffectual struggles.

This is not the effect of debility or of partial paralysis; for, at the time when the condition of the animal is most helpless, his muscular contractions are often very vigorous and even violent. It is just such an effect as would be produced by the loss or diminution of co-ordinating power. When the injury has been moderate in extent, so that the pigeon can still stand and walk, though imperfectly, there is often a very close and ludicrous resemblance to the effects of intoxication—the movements being still quite natural in force and rapidity, but their harmony and certainty being lost.

The operation of removing the whole of the cerebellum is exceedingly dangerous; and even when two-thirds or three-quarters of it only are taken away, the animal generally dies within twenty-four hours. This seems to be owing, in great degree, to the proximity of the medulla oblongata, which must suffer much from the hemorrhage and irritation consequent on an extensive mutilation of the cerebellum.

But, in a few instances, in which I have done this operation, it has happened that the animals have survived altogether, notwithstanding the destruction of a considerable part of the cerebellum; and in these cases I have observed the very remarkable fact, that in process of time *the power of muscular co-ordination is restored*. Subsequent examination proved that this was not owing to a reproduction of the lost parts of the cerebellum. The animal being killed and examined, the mutilation of the nervous centre was found to have remained nearly in the same condition as at first; so that the bird had recovered its power over the voluntary movements, although a large part of the cerebellum was permanently destroyed.

The first case of this kind which came under my observation was the following:—

In the month of January, 1859, I removed from a young, but well-grown male pigeon, while under the influence of ether, the upper and middle portions of the cerebellum, constituting about two-thirds of its entire mass. Immediately afterward the pigeon showed all the usual effects of this operation to a very marked degree. He was incapable of walking, of flying, and even of standing still, but struggled and sprawled about, exactly as other pigeons had done when subjected to a similar mutilation. He was used very successfully to illustrate the effect of this operation, before the Medical Class.

This pigeon, however, contrary to my expectation, survived; and, in the course of a few days, it was evident that he was recovering the control of his limbs. This recovery continued to go on, at the same time with the general re-establishment of the animal's health; and, at the end of five or six days, he was again very nearly capable of executing all his natural

motions. His appetite was also restored, and he ate and drank freely, as before the operation.

The pigeon lived for sixteen days, and at the end of that time, when he was killed intentionally by decapitation, there was no apparent reason why he should not have survived indefinitely, since his health, to all appearance, was thoroughly re-established. His appetite and digestion were good. His voice was natural in force and tone; and his power of muscular *co-ordination* was so nearly perfect, that its deficiency, if any existed, was practically inappreciable. The only peculiarity remaining was a moderate general debility, not noticeable except when the animal made some unusual exertion. Thus, when he flew off a high perch on to the floor, he found some difficulty in alighting, and generally tumbled forward on his face as he struck the ground; but he always recovered himself immediately, and resumed a natural position. When he fought with other pigeons kept in the room at the same time, which he frequently did, in order to drive them away from the food, his blows, though somewhat wanting in force, were always perfectly well directed.

On examination after death, the wound in the integuments, skull, and membranes, was found to be in rapid process of healing. The wounded cerebellum was free from signs of inflammation, but was covered with a loose, fibrinous coagulum, which was easily removed, leaving the wounded surface underneath perfectly distinct. The remainder of the cerebellum was healthy in appearance.

About two-thirds of the cerebellum, as stated above, was found to have been removed, and apparently there had been no attempt at its reproduc-

Fig. 1.



Fig. 2.



Fig. 3.



Fig. 4.



Fig. 1. Brain of healthy pigeon—Profile view: 1. Hemisphere. 2. Optic tubercle. 3. Cerebellum. 4. Optic nerve. 5. Medulla oblongata.

Fig. 2. Brain of operated pigeon—Profile view, showing mutilation of the cerebellum.

Fig. 3. Posterior view of brain of healthy pigeon.

Fig. 4. Posterior view of brain of operated pigeon, showing mutilation of cerebellum.

tion. The entire brain, with the medulla oblongata, was preserved in spirits, and the accompanying drawings will serve to show the proportionate quantity of the cerebellum which had been removed, and the appearance of that which was left behind. I have also introduced drawings of the healthy brain of the pigeon for purposes of comparison.

Since the date of the above experiment I have met with several other instances of a similar kind in which the injury inflicted on the cerebellum was less extensive, but in which the immediate effects of the operation were distinctly marked, and the final recovery of the animal equally complete.

In October, 1860, I operated upon two pigeons, a light-coloured and a dark-coloured one, by opening the posterior part of the skull and removing a portion of the cerebellum. In the light-coloured pigeon the attachments of the posterior longitudinal muscles of the neck to the occiput were partially separated. In the dark-coloured one they were left entire.

Next day both pigeons showed distinct signs of imperfect muscular co-ordination. The dark-coloured one was the most affected of the two. Both pigeons could both stand and fly, but neither of them would willingly trust themselves to their wings, and they required to be almost pushed off the perch before they would do so. The flight in each was weak, ill-directed, and soon exhausted. Both seemed to experience a similar difficulty in balancing themselves when in the upright position. When standing upon a plane surface their bodies had a tendency to tip forward; and whenever they attempted to walk their gait was tremulous and uncertain, the bird tottering or "tottling" forward upon the three front toes, and the hind toe being constantly raised from the ground for a quarter to half an inch. Neither of them would take food or drink. The light-coloured one perched upon a beam, and remained there until driven off the following day. The dark-coloured one passed the night standing on the floor, in a corner of the room.

Next day, they were nearly in the same condition, though the dark-coloured pigeon seemed to walk a very little better. When corn was thrown to him he looked at it, and seemed to desire to pick it up, but did not trust himself to make the attempt. He approached a basin of water, however, and, with much care and balancing, tried to reach the fluid by leaning over the edge of the basin, but without success. On the basin being filled to a higher level he repeated the attempt, but still did not succeed in drinking, though sometimes his bill was thrust beneath the surface of the water. Occasionally he seemed to miscalculate the distance, and to suppose his bill to have reached the water when it did not yet touch the surface of the fluid.

The light-coloured pigeon showed no desire for either food or drink.

On the sixth day both pigeons fed for the first time. Both were improved in walking, but still showed a decided unsteadiness of gait when hurried or excited. The tipping forward on the toes was most marked in the dark-coloured animal, the lateral unsteadiness in the light-coloured one. The

dark-coloured one picked up the first two or three grains of corn with considerable readiness, but afterwards could not succeed in reaching them. He made the attempt constantly and perseveringly, but his bill would never quite reach the grains, though its movements had generally the right direction. The light-coloured pigeon, on the other hand, picked up many grains and fed well.

On the eighth day both pigeons were still improving. Both flew down from the perch when grain was thrown upon the floor, and succeeded pretty well in picking it up. They also flew back to the perch after having been fed. In both, the gait was still somewhat awkward and uncertain when the birds were excited.

On the sixteenth day both pigeons appeared quite well. They had both so far recovered the control of their motions that they would not have been noticed as at all peculiar in their manner of standing, walking, flying, alighting, or picking up grain. They were then both killed by decapitation, and their brains examined.

The perforations in the skull were filled by coagulated blood and lymph. The wounds, in both cases, looked perfectly healthy. In the light-coloured pigeon, traces of the separation of the muscular attachments at the occiput were still visible. In both, the cut surface of the cerebellum was covered by a very thin, transparent, and yellowish exudation. In the dark-coloured pigeon (which had been the most affected), about one-half the cerebellum was found to be wanting; and in the light-coloured one, a little less than one-half.

In one other instance of a similar kind very nearly the same result was obtained.

On the 19th of October, 1860, I exposed the posterior part of the skull in a full grown pigeon while under the influence of ether, perforated it from the side, and broke up and extracted a portion of the cerebellum.

Next day, the animal was very much affected with want of "co-ordination." He could maintain the standing posture when undisturbed, but the least attempt at moving caused him to totter forward and backward, and to reel sideways, very much as if slightly intoxicated. His flight was feeble and uncertain, and he found great difficulty in getting foothold when alighting, even on a tolerably wide place. He could not get over a bar, one or two inches high, without much stumbling and reeling. He could not pick up the corn which was thrown to him, though he evidently wished to do so, and once or twice made the attempt; but after a few trials he abandoned it, and was apparently conscious that he could not succeed.

On the third day he appeared better. He could then stand pretty well, only tipping forward a little upon his toes, with the hind toe slightly raised from the ground; but he still reeled whenever he was disturbed and forced to move quickly. He also succeeded in picking up grain, though with some difficulty, and with long intervals between the attempts. When forced

to fly, he did so with great reluctance, but succeeded in getting up to his perch, upon a beam in the upper part of the room, where he finally alighted after some troublesome balancing.

On the fifth day he could stand and walk very much better, and could pick up grain without much difficulty. He could also fly and alight upon his perch pretty well, though he was still very evidently affected in the same manner as before.

On the ninth day, the pigeon was very nearly recovered. He flew about the room and alighted upon his perch in a natural manner. There was but little irregularity perceptible in his gait, even when he was hurried and frightened; and he picked up his food from the ground perfectly well. At this time he was killed by decapitation, and the brain examined.

The wound in the skull beneath the external integuments was closed by a partly decolorized, dry, and condensed coagulum. Of the cerebellum, about one-quarter at the upper part was wanting. Of the remainder, about two-thirds presented externally a greenish-yellow colour, as if infiltrated with lymph; and internally a part of its substance was replaced by a semi-fluid mixture of coagula and pus. Altogether, considerably over one-half the entire mass of the *cerebellum* was disintegrated and destroyed. The remaining portions of the organ were healthy in appearance.

These instances, I think, show that the permanent loss of a portion of the cerebellum does not permanently impair the power of muscular co-ordination. If, therefore, we are still to believe that the power of co-ordination resides in the cerebellum as a nervous centre, we must admit that, after the removal of a part of this ganglion, the remaining portions gradually become enabled to supply its place. If, on the other hand, we reject the doctrine of Flourens, then we find a difficulty in explaining why a loss of co-ordinating power is always the immediate consequence of a partial removal of the cerebellum. In that case, the irregularity of motion, witnessed in these experiments, can only be accounted for by supposing it to be in some way the effect of the *sudden injury to the cerebellum as a whole*, rather than the simple loss of a part of its substance.

We must also recollect that many cases of abscesses, tumours, &c., of the cerebellum have been observed in the human subject, by Andral and others, without any marked disturbance of the co-ordinating power.

At all events we must acknowledge that the recovery of co-ordinating power, after permanent removal of a large portion of the cerebellum, weighs as much against the doctrine of Flourens, as the loss of co-ordinating power, immediately after the operation, weighs in its favour.

ART. IV.—Notes on "*Euphorbia Prostrata*" as an Antidote to the Poison of the Rattlesnake. By B. J. D. IRWIN, M. D., Assist. Surgeon U. S. Army.

DURING the summer of the last year my attention was attracted through the public press to the unusually large number of persons reported as having lost their lives from the bite of the rattlesnake. Many fatal cases were recorded from almost all parts of the United States, but the greater number appeared to have occurred in the South and Southwestern regions. Knowing how abundant the several varieties of *Crotalus* are in this region of country, and never having heard of any case of death from the poison of this reptile amongst the inhabitants, I was led to make inquiries of the Mexican population as to what protective measures they resorted to in cases of injury from the rattlesnake or the many other poisonous creatures that infest the country. After many inquiries among the natives, Mexicans and civilized Indians, I learned that, although injuries from poisonous reptiles were very common among them, they had an efficacious antidote against the poison of the rattlesnake and other noxious reptiles. The remedy resorted to I found to be what they designate as "*Gollindrinera*" (*Anglice*, swallowwort), an herb of very frail, delicate appearance, found growing abundantly throughout the southern portion of Texas, New Mexico, Arizona, and Sonora. A specimen forwarded through the Smithsonian Institution to Professor Torrey was pronounced by him to be "*Euphorbia Prostrata*," a plant belonging to a family entirely different from that of the swallowwort. It grows plentifully in dry, hard, sandy places, especially in road-ways, farm-yards, pathways, and in a hard, compact, gravelly soil, and has a frail, delicate appearance, resembling in its external character the gold-thread (*Coptis trifolia*), with long, filiform, reddish stems, that spread and interlace with each other. Leaves, petaloid, obcordate, regular, opposite, of a deep-green colour, and varying in length from three to five lines. Flowers, axillary and very small, white, with dark-purple throat; sepals four, petals four, pentandria monogynia. Root, quite large, dark-brown colour, and possessing an abundance of milky juice, which pervades all parts of the plant; taste insipid; odourless. Flowers from April till November.

Having heard much of the rare virtues of this plant, I determined to satisfy myself of its properties by proper investigation, and, after a series of experiments, carefully performed, I had the gratification to find that its qualities as an antidote to the poison of the rattlesnake are equal to that now used and known as "Bibron's Antidote." The experiments were made on many dogs, and extended through a period of several months, with like satisfactory results. The medicinal properties of the plant I found to reside

in the milk-like juice of the stem, root, and leaves; but that usually used was the fresh juice extracted from the plant by bruising it in an iron mortar, and diluting it with a considerable quantity of water. As to its *modus operandi*, I am not prepared to offer an explanation; its physiological effects are emetic and cathartic, when given in large quantity. As it would be tedious to give the history of all the experiments made, I will confine myself to giving the notes of one or two cases in detail.

Expt. 1. August 16, 1859. At eleven o'clock A. M. a full-grown slut was subjected to the bite of a large rattlesnake (*Crotalus confluentes*), which was very much infuriated, and inflicted a severe wound at the root of one of the ears, where it hung by its fangs for several moments. The dog immediately became greatly agitated, very restless, and whined most piteously. Ten minutes afterwards she was unable to stand up; respiration hurried; drunken appearance, with bloodshot eyes; involuntary evacuation of urine and feces; no swelling about the injured part. Administered four fluidounces of the watery juice of the freshly-gathered "Gollindrinera," and applied some of the bruised plant to the wound. Two minutes afterwards the dog got up and walked about, with only a slight staggering gait.

12 o'clock M. Respiration is still very hurried and difficult; restless; eyes very red, but drunken appearance is rapidly diminishing; no swelling of the injured part; dog is walking about, but with an unsteady, rolling gait; disposition to vomit. Comes when called, and evinces pleasure on being caressed. Repeated the dose.

1 o'clock P. M. Is almost well; walks with firmness, and appears to have ceased suffering. Between two and three o'clock vomited freely, first food, and afterwards some dirty-looking watery fluid, consisting in part of the remedy administered. About eight ounces were given, of which two ounces were pure juice. The dog partook of food and drink about five o'clock, and was running about, in seemingly good health, at evening time.

Expt. 2. August 20, 1860. A large dog was bitten on the nose by a rattlesnake having nine rattles. Thirty minutes afterwards he was unable to stand up, and apparently insusceptible of external impressions; respiration hurried; eyes congested; general appearance of speedy death; head and neck enormously swollen. Four fluidounces of the watery juice of the "Gollindrinera" were poured down his throat, and a cold epithem of the bruised plant applied over the swollen parts. Within fifteen minutes he got up and walked about with a listless air and unsteady gait. Two hours afterwards the condition was vastly improved; swelling greatly diminished, intelligence restored, respiration almost natural, and the eyes and expression had assumed almost their natural characters. Repeated the medicine in the same quantity. By evening the dog was entirely well, save some swelling, which disappeared completely by next morning.

The phenomena manifested in all the other cases, excepting one, were similar in every respect to the foregoing. The exceptional case proved fatal to a *very old little* dog, to which I did not give the antidote until he was apparently dead. He resuscitated shortly after taking the remedy, but death by asthenia took place before the remedy could produce its effects. In one particular dog no constitutional symptoms could be produced by the poison of the rattlesnake; although she was severely bitten by two snakes

at separate intervals, and I inserted some of the poison from a fang into a wound made for its reception, nevertheless nothing more than local swelling and pain could be induced in her system. I cannot account for the immunity enjoyed by that animal, as the same reptiles caused deadly poisoning in other dogs.

From the universal dissemination of this plant throughout the southwestern portion of the United States and Mexico, I believe a knowledge of its therapeutic properties will prove of much importance to the public, as it grows plentifully through the whole year; its preparation is simple, and its administration is unattended with danger to the animal economy. I have learned from the Mexicans of many cases of poisoning by the rattlesnake-bite cured by its use, and in no case has it failed to cure, or has it done injury to the patient.

In my reading, I find that other varieties of the *Euphorbiaceæ* enjoy a reputation as remedies against the poisons of noxious reptiles. The *Euphorbia capitata*, *E. corollata*, *E. palustris*, and *E. villosa*, are somewhat celebrated in southern countries as specifics against the poison of venomous serpents and the bite of rabid animals. One of the strongest arguments that I may adduce in favour of the *Euphorbia prostrata* is, that amongst the Mexican population of Arizona and Sonora, who are frequently subjected to poisonous wounds from the rattlesnake, coral snake, "vinegrilla" (*Thelyphonus*), scorpion, centipede, tarantula, and a host of other hideous creatures, in no instance does the injury result fatally, as they resort at once to their specific, ycleped "*Gollindrinera*," which never fails to produce a sure and speedy cure. In the hands of unscientific persons, I look upon it as an invaluable specific, possessing many advantages over that of M. Bibron, as it can be readily procured, easily prepared, and administered by any person, free from the many difficulties and disadvantages attending the preparation, preservation, and administration of M. Bibron's valuable remedy.

PORT BUCHANAN, ARIZONA, Sept. 7, 1860.

ART. V.—*On Loss of Sight from Effusions under the Retina.* By HENRY D. NOYES, M. D., Assistant Surgeon to the New York Eye Infirmary.

OUR knowledge in relation to cases of blindness, in which the immediate cause of loss of sight depends upon detachment of the retina from the choroid coat, by effusion of fluid between the two membranes, has been widely enlarged by the use of the ophthalmoscope; but before this instrument was introduced, the lesion was well recognized as one of the most fatal injuries to vision, and, in some cases, with the pupil widely dilated, it could be discerned by the naked eye in the living subject.

The kinds of effusion I mean, are of serum and of blood.¹ The effusion takes place, 1st, as the result of violence; 2d, as the result of disease.

1st. When it is the effect of injury from blows or falls, there may of course be more or less damage done to other parts of the eye, such as rupture of sclerotica, or cornea, wounds of the iris, or capsule, etc. I have in view cases where the loss of sight would not occur, or would not be so serious, unless the effusion under the retina took place, in addition to the remaining injuries.

CASE 1. *Detachment of the retina by a blow upon the eye.*—Israel W——, aged 21, native of New York, farmer, came to the Infirmary April 1st, 1860. The right eye is entirely blind; sight in the left eye is imperfect, and he has muscæ volitantes. About fifteen months ago he was struck with the fist upon the right eye; there followed ecchymosis of the lids, and swelling, that kept the eye shut for several days. No inflammation of the eye occurred, or anything to excite suspicion of injury to sight. In three weeks afterwards he discovered, accidentally, that this eye was sightless. Has never had pain or external redness of either eye.

Examination by the ophthalmoscope shows that in the blind eye the retina is lifted from the choroid over all its extent, except for a narrow space, upon the nasal side, by a drab-coloured fluid; the optic nerve clearly seen. In the other eye the vitreous is hazy, and in it is seen a flocculent membrane floating about, just behind the lens; the optic nerve is very dimly seen. The clear evidences of choroiditis in this eye, invite the conclusion that similar disease may have existed in the blinded one, and that this predisposed to the disastrous sub-retinal effusion which the blow produced.

2d. Effusions under the retina, resulting from disease. Among the causes to be assigned are, alcoholic intoxication; choroiditis, whether syphilitic or not; sclerotico-choroiditis-posterior; retinitis, from albuminuria or from any cause; that meningitis may cause this accident, there seems some ground for believing.

CASE 2. *Intoxication, effusion of blood under the retina.*—John W——, aged 35, born in New York, came to the Infirmary in August, 1860, and was under the care of Dr. Bumstead, to whom I am indebted for notes of the case. He is usually a temperate man, and during the shooting season his business is to kill game for market. He was hunting with a party of gentlemen in the early part of the season, and one night drank wine with them until he became intoxicated. He felt badly the next day, had a severe headache, and upon trying his gun, found, to his utter astonishment, that one eye was blind. Vision remained the same until he came to the Infirmary; to external appearance, there is nothing abnormal in the eye; the pupil is of natural size, and acts feebly. When the pupil is fully dilated

¹ When the effusion is of blood, I mean cases where it is in such quantities as to push the retina an appreciable distance from the choroid, so as to permit the membrane to undulate, as it floats upon the subjacent fluid. A less quantity of blood upon, or in, or under, the retina, constitutes what may more correctly be called apoplexy of the retina, and in its subsequent history has features quite different from the cases now under consideration.

with atropine, a dusky or smoky-coloured reflex is given out from the bottom of the eye, which is to be seen only in certain positions. With the ophthalmoscope, the retina is seen to be detached upon the temporal and inferior side of the globe. The limit between the applied and separated portions is sharply defined. The vessels of the retina bend at an angle, as they pass from one region to the other, and tremble with all the movements of the projecting sac. The colour of the sac, that is, of the fluid beneath it, is a deep blue. Patient can see objects held at the temporal side of his eye, and when held much below the axis of vision, but in other parts of the field sees nothing. This limitation of the visual field is explained by the position of the effusion.

CASE 3. *Constitutional syphilis; irido-choroiditis; detachment of the retina.*—Mary McCaffrey, aged 43, Ireland, widow, came under my observation, four months ago, at the Infirmary. She has strabismus divergens of the right eye, and a central opacity of the cornea; with this eye she sees nothing. Sight in the left eye is bad, can only read coarse print, No. 13 of Jaeger's test print, and cannot discern objects at a little distance. She complains of numerous black specks floating in the air, and wears a deep hood, to shade her eyes from the light. In looking at objects, she turns her eye upon them obliquely, bending her head downwards.

Ophthalmoscopic examination of the squinting eye shows that in it the retina is completely stripped from the choroid, by serous effusion; the colour is a light gray. In the other eye, the vitreous humor is filled with floating particles, and the pigment is much removed from the epithelium of the choroid.

The patient was nine years ago a patient of the Infirmary, in the service of Dr. Halsted. She then had syphilitic iritis, and a secondary cutaneous eruption. She has had also syphilitic rheumatism. She admits having contracted chancre from her husband, and she afterwards gave birth to an infant, which was a well-marked specimen of hereditary syphilis. She was treated at that time with mercurials, and there are now no evidences of syphilitic disease, except in the ravages done to sight. There are no adhesions of the pupil indicating foregone iritis. She says she lost the sight of the right eye suddenly one day, while standing at a table ironing, and that the diverging squint came on gradually afterwards.

CASE 4. *Sub-retinal effusion in an eye probably affected with sclerotico-choroiditis-posterior.*—The patient, E. F., aged 23, born in London, married, was in charge of Dr. Hinton, who kindly permits me to make use of the case. She is a stout woman, with florid face, and was in such agitation when she came to the Infirmary, that she could hardly speak coherently. Was nursing an infant of twelve months; had always had good health, and while suckling, drank about a pint of beer per diem; not subject to headaches; is prone to costiveness. One eye had been lost at a very early age, and evidently from irido-choroiditis, for it has cataract, complete posterior synechia, and liquefaction of the vitreous humour; the cornea is clear, and the bulb not atrophied. The remaining eye has always been extremely near-sighted, so that without glasses her reading distance was from three to four inches. A week ago, while sewing in the evening, suddenly the light seemed to her to become of a red colour, her sight was confused, and presently extinguished. During that evening she had a violent headache. After the lapse of a week, vision improved a little, that is, she could discern objects upon the outer side, but they had a greenish hue;

straight lines seemed crooked and bent to the left side. Her visual field is limited to the temporal side; an object held directly in the axis of vision, above, below, or on the nasal side of the axis, is not seen at all. The globe has a strongly myopic form; the pupil acts feebly.

With the ophthalmoscope, the retina is seen to be pushed forwards into the vitreous humour by an amber-coloured effusion, which has detached all the retina, except for a space upon the nasal side. The optic nerve cannot be seen at all, because concealed by the retina floating in front of it; hence, the existence of the crescent-shaped atrophy of the choroid, at the margin of the optic entrance, which is the mark of sclerotico-choroiditis posterior, could not be demonstrated. I feel confident it must have been there, from the excessive myopia, from the manner in which the other eye had been destroyed in childhood, and from the sub-retinal effusion. All these are symptoms and consequences of sclerotico-choroiditis-posterior.

CASE 5. *Detachment of retina by effusion; (from meningitis?)*—Louisa H., aged 32, born in Germany, single. Patient's account of herself is so confused, as to permit little more than a guess of the etiology of her blindness. She has strabismus divergens; the pupils dilated to the medium size, and sluggish; she holds her head up, and to one side, when trying to see objects; direct vision is annulled. She came to the Infirmary for the first time in August, 1859. She was taken as an in-patient, but the evening after her admission, and before a complete investigation of her eyes could be made, meningitis set in, and she was removed from the institution. She returned again in February, 1860. She stated that one eye, the right, became blind, suddenly, three years before, while at work in the fields; whether this was coincident with sunstroke, or any other cerebral disturbance, could not be determined. The remaining eye was lost at a subsequent period; whether this happened at or before the attack of meningitis, she could not tell. She says she has been near-sighted, has had abundance of luminous flashes, and *muscæ volitantes*, and occasionally pain in the eyes.

The ophthalmoscope shows that in both eyes there is effusion of light-coloured fluid under the retinæ; in the right eye there are opacities, like "mother-of-ivory," pervading the vitreous humour; the choroid has spots of atrophy through which the white sclerotica can be seen; the vessels of the retina are partly obliterated.

In the retinitis, which sometimes occurs in Bright's disease of the kidney, effusion behind the retina has been observed. A case of this kind has been reported by Professor Graefe, but the effusion was small in quantity. Dr. Liebreich gives a figure of such a case, *Archiv. für Ophthalmologie*, Band. v. abth. 2.

In eyes that have become disorganized through Panophthalmitis, where the bulb is atrophied, and often a calcareous shell formed in the posterior segment of the globe, upon post-mortem inspection, the retina is often found to be completely separated from the choroid. It is rolled up into a cord or forms a cone with its base at the *ora serrata* and its apex at the optic nerve. The vitreous humour will be almost wholly absorbed, and the space between the retina and choroid will be filled with turbid yellowish serum. This complete detachment of the retina is not to be known usually during life, because the accompanying lesions of the globe will prevent inspection of the parts behind the iris.

There are certain diseases liable to be complicated with sub-retinal effusion, in which the patient little regards the morbid state, and much less suspects that a calamity so dread impends over him. So it was in Case 4; the great myopia was not known to depend on sclerotico-choroiditis posterior. Sometimes choroidal disease is as little heeded, and its power to occasion disaster as sadly felt. The looseness of connection between the choroid and retina favours the intrusion of fluid in this direction, while the intimate union between the choroid and sclerotica makes effusion between them very rare. It is readily understood how an effusion of blood or serum would be apt to occur from a membrane so vascular as the choroid when it is in a state of chronic congestion, when weakened by atrophy, or when acute inflammation gorges its vessels.

Symptoms of Sub-retinal Effusion.—The patient is suddenly struck blind, either totally or partially; perhaps has headache and vertigo, has chromoptic appearances, and if able to see at all, says that straight lines appear crooked, that objects are bent or their outlines waver. To external inspection the eye may seem unharmed, except a little torpor of the pupil. If the patient be directed to fix his eyes upon one hand held directly before him in his axis of vision, while the other hand be carried in a circle around this, the exact limits of his visual field can be mapped out and an approximate estimate made of the extent of the separation of the retina.

Certainty of diagnosis can be had only by aid of the ophthalmoscope. It is not necessary for the first glance to use the double convex lens, the mirror alone will reveal the sacculated and projecting retina: to trace the vessels and to make a complete examination the lens will be needed. I allude to examination with the inverted image. The pupil should be widely dilated with sulphate of atropia. Instead of the uniform red reflex from the fundus oculi, an object is seen projecting into the vitreous humour, with waving, fluttering outlines. Its colour varies greatly; sometimes it has almost the natural hue of the bottom of the eye, and at first glance might be overlooked; it may have a gray or drab shade; it may be even a deep bluish green, as is figured in Sichels' "*Iconographie Ophthalmologique*," and in Edward Jaeger's "*Beitrage zur Pathologie des Auges*." Sometimes the colour is mottled, parts being light, other parts dark. Besides the waviness of outlines, the surface of the object will exhibit folds that form, change, and disappear. The decisive point of diagnosis is in the relation which the branches of the arteria centralis retinae bear to the object. They will be of a darker or a lighter colour according with the colour of the sub-retinal fluid. They will be seen to tremble with the movements of the retina, and in tracing their course they will be seen to bend at a sudden angle, at the point where they leave the applied surface of the retina, to traverse the floating portion. This bending of the vessels and their tremulousness are characteristic of sub-retinal effusion; these signs distinguish it perfectly from free floating bodies in the vitreous

humour. If the effusion be sub-choroidal, and the fluid not too opaque, the choroidal vessels will be observed in addition to the retinal vessels. The circulation in the retinal vessels is not destroyed as was once supposed; the flow of blood is hindered, yet the current moves. The varieties in colour above described are due to the varying degrees in which blood composes the sub-retinal fluid; a less or greater admixture of hæmatine will give all the tones of colour from light drab to deep blue. When clots are mingled with nearly clear serum, the mottled hue will appear. Sometimes crystals of cholesterine are seen in the fluid.

The most usual situation for the fluid is at the most dependent part of the eye. It does not always originate in this spot, nor does it always flow down. It will not generally have a horizontal level, but rise up on the temporal or nasal side.

The loss of function is not confined to that portion of the retina which is stripped from the choroid, but a margin beyond this is impaired in function, by the violence inflicted upon it indirectly, and by the inflammatory action which is often set up.

Prognosis.—The fluid is very seldom removed; sometimes it bursts through the retina into the vitreous humour; if it have first been poured out under or near the macula lutea it may gravitate downwards, and sight be thus improved. Two interesting cases of this sort are reported by Dr. Liebreich (*Archiv. für Ophthalmologie*, B. v. abth. 2). The rational signs, to borrow the language of auscultation, of this change of position of the fluid, will be, that whereas the patient was at first quite blind, he can afterwards see better, especially by holding his head well up. It is not always the fact that the retinal membrane, when pushed away, and afterwards re-applied, as in the above cases, will recover its function—its intimate structure may be irrecoverably changed. In general, the prognosis of effusion of fluid under the retina is unfavourable to the recovery of sight. Moreover, the patient is in some cases exposed to a repetition of the effusion—so that a partial separation may become complete detachment of the retina. This is more liable to happen in sclerotico-choroiditis-posterior.

Treatment.—It has been proposed to puncture the retinal sac through the sclerotica, to evacuate the fluid; but this operation has not yet been rewarded by amendment of sight. This could not be expected unless the puncture were made very soon after the effusion took place, because when much time has elapsed, the elements of the retina lose their normal structure, and consequently their sensitive qualities.

What can be effected by treatment is to control the inflammation of the adjacent portions of the retina. This is to be done by cupping from the temples, rest of the eyes and protection from light, and use of corrosive sublimate. In plethoric persons, diuretics and revulsive purgatives may become needful.

ART. VI.—*Report of Two Cases of Fracture of the Thigh, with an account of a new apparatus.* By ROBERT P. THOMAS, M. D., Surgeon to the Episcopal Hospital, Philadelphia. (With two wood-cuts.)

HAVING experienced, like most hospital surgeons, great difficulty in the management of compound, and of compound-comminuted fractures of the thigh, and having found it almost impossible to prevent undue shortening of the limb by any forms of apparatus in general or occasional use, my attention was directed to the necessity of endeavouring to modify them so as to secure more favourable results. In the public institutions of this city, Dr. Physick's modification of Desault's splint is in common use, and it is found to fulfil the requisite indications in simple fractures, whether oblique or transverse, as well as any other hitherto presented. But in compound, and in compound-comminuted fractures, a serious objection exists to its employment, based on the necessity of relaxing the extension and counter-extension every time the dressings have to be changed, if the flesh-wound be on the lateral or posterior aspect of the limb. In the following case this difficulty occurred, and it gave rise to the substitution and use of the apparatus described below :—

CASE 1. Levi B., a machinist, aged 57, was engaged in raising a heavy iron casting, when the tackle gave way and the mass fell upon his half bent thigh, crushing the right femur at its lower third, and causing a laceration and a severe contusion of the soft parts. He was admitted into the hospital August 7, and the limb placed in a Desault's apparatus. Cold water dressings were applied, the wound having been closed by a strip of muslin saturated with a solution of gutta serena in chloroform. The entire thigh became enormously swollen, ichorous matter appeared in the wound on the third day, and the febrile symptoms assumed a serious character. Eversion of the foot was decided and difficult to control. Finding it impossible to prevent shortening when the dressings were changed, and fearing the results of the irritation produced whenever the extension was renewed, I determined to substitute the modified fracture-box for the splints of Desault.

A semicircular piece of soft buckskin (two inches wide, and long enough to reach half round the thigh) was coated with soap plaster and applied to the skin so as to protect the sulcus between the scrotum and thigh, resting against the fold of the buttock behind, and terminating above the trochanter on the outer side. The counter-extending pad, made of soft buckskin and loosely filled with cotton, rested against the plaster without touching the skin at any point. A soft pillow, partly covered with oiled-silk, was placed in the fracture-box and the limb laid upon it. Extension and counter-extension were effected, as shown below, and lateral pressure was secured by shutting the sides of the box and tying them with tapes. A broad bandage carried under the patient's back and around the abdomen secured his body to the upper part of the long outside leaf or splint, and thus effectually prevented lateral twisting of the pelvis. The limb was under perfect control, and the requisite change of dressing could be made by un-

fastening the sides for a few minutes and then re-securing them without disturbing either the extension or the counter-extension in the slightest degree.

Once in every forty-eight hours the edge of the gutta percha coating was raised from the flesh-wound, and the abscess having been partially evacuated it was again immediately protected from the atmosphere by a fresh coating. In this way about a quart of pus was gradually drawn off, and the whole cavity contracted and healed in four weeks.

The fracture having united, the apparatus was removed at the end of seven weeks, and the patient was permitted to use crutches. A fortnight later he left the hospital.

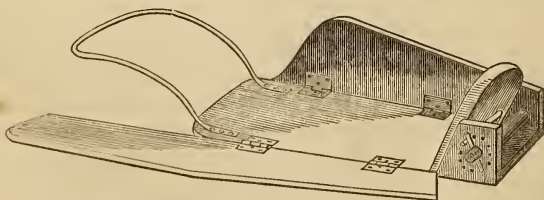
Nov. 7. Three months have elapsed since the injury, and the patient has resumed his avocation. An examination with the measuring-tape fails to detect any shortening; nor is the man himself aware of any difference between the two limbs. Much of the credit of this unusual result is due to the constant supervision of the house-surgeon, Dr. B. E. Fryer.

CASE 2. Bridget C., aged 41, admitted, August 22, with a simple fracture of the neck of the right femur. In passing down a dark cellar stairway, unprotected by a handrail, she stepped accidentally upon a kitten, and in the sudden fright fell sideways from the stairs directly upon the trochanter. Upon admission there were slight shortening of the limb and eversion of the foot, with a clear, well-marked, grating sound, when the limb was extended and rotated. She was placed in the new apparatus, and experienced immediate relief from the pain peculiar to fractures. At the expiration of six weeks it was removed, the union being firm.

Nov. 7. The patient is using crutches, and thinks herself restored without shortening. My own impression is, that a slight but almost imperceptible difference exists between the two limbs.

The apparatus used in these cases is a modified form of the common fracture-box employed in injuries of the bones of the leg, and embracing the long and short side splints of Desault. The windlass arrangement for making extension is similar to that first used by Dr. H. Hartshorne. The idea was derived from the plan adopted by carters for loading and carrying heavy stones. The mode of effecting counter-extension is peculiar, and can only be made intelligible by a reference to the wood-cuts below.

Fig. 1.



Description of the apparatus as used upon an adult of medium size.

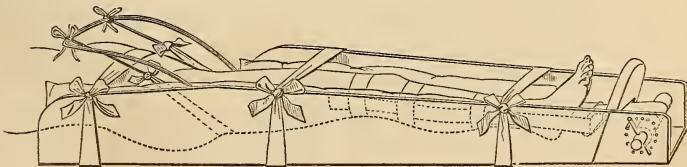
The floor of the box is a single board, one inch thick, and three feet six inches long, viz., eight inches long below the foot-board, and two feet ten inches above it. It is six inches wide at the lower end and nine inches at

the upper, where it is rounded off to prevent excoriation of the soft parts. Each side consists of two portions—one fixed, the other movable. The former is eight inches in length, and is firmly fastened to the side of the foot-board, and also to the floor-board. The latter, or movable part of each side, is secured to the floor-board by two hinges. The boards forming the sides are five-eighths of an inch thick. The short leaf or side is six inches wide at one end, and seven and a half inches at the other; and is two feet eight inches long, the upper end being rounded off. The long leaf or side measures four feet four inches in length, and corresponds with the other in width, being six inches at the foot-board and seven and one-half inches at the hip; and it is continued of this latter width up to the axilla.

In the centre of the foot-board is a transverse opening through which the extending band passes down to be attached to the wooden roller that revolves in the two holes made in the sides of the immovable part of the box, as seen in the engraving. This roller is about an inch and a quarter in diameter, and is nearly a foot in length, each end projecting two or three inches beyond the side of the box. Through each end a hole is made for the reception of a wooden pin, by means of which the roller or windlass is turned, and the extension effected. When the requisite tension is attained a common tenpenny nail is placed, on each side, in one of the circle of small holes bored in the sides of the box. The wooden pins rest against these nails, and thus effectually maintain the acquired tension.

The counter-extending frame is made of a single piece of iron wire, seven-sixteenths of an inch or half an inch in diameter, and four feet ten inches in length. It is bent, as represented, somewhat in the form of the letter U, or, more correctly speaking, like the handles of a plough. Or to be more precise, the transverse part of the frame measures nine inches; each curved leg is nineteen inches, and each flattened foot is five and a half inches long. The degree of curvature in the frame is just sufficient to clear the thickness of the patient's body, and to admit of its extension well up over the thorax. In other words, the curvature of the frame corresponds to that

Fig. 2.



Application of the apparatus.

of the body, the distance between the two nowhere exceeding two inches. As intimated, each foot of the frame is flattened out and drilled for four screws, by which it is secured at the margin of the upper part of the floor-board.

A soft pillow having been placed in the open box, the foot and limb are passed through the wire frame and laid upon the pillow. The groin and buttock being protected by soap-plaster, as described in Case 1, the counter-extending pad is placed under the thigh against the buttock, and the tapes attached to its ends are tied to the respective angles of the transverse bar of the frame. A strip of good adhesive plaster, three inches wide and long enough, when doubled, to extend from the sole of the foot to the lower third of the thigh, is lined at the middle by a short strip of the same width for the extending block to rest against, and is then applied along both sides of the leg and lower part of the thigh, and is further secured by a few transverse slips of plaster.

The extending band having been tied to the wooden block under the sole of the foot is carried through the opening in the footboard, and is fastened by one or two tacks to the wooden roller. A few turns of the latter effect the extension, and the surgeon has merely to make an extra turn from day to day to keep it up. No relaxation can possibly occur except from stretching of the bands. A pad or small pillow is then placed along the body from the hip to the axilla, and the leaves or sides of the box are closed and secured by tapes; a broad bandage encircling the body and the upper part of the long splint.

In making extension I think it advisable to attach the adhesive strip a few inches above the knee, as the severe stretching and subsequent stiffness of the joint are thereby diminished. The form and direction of the counter-extending frame require attention in the construction of the apparatus; for if short, or too perpendicular, there is danger of elevating the upper fragment of the fractured bone; but if, on the contrary, the frame arches over the body, and extends up to the thorax, this danger is obviated, the line of counter-extension being in the axis of the limb.

In my cases padded buckskin was used for the counter-extension, but if a preference is entertained for the adhesive plasters, as recommended by Dr. D. Gilbert, the substitution can be easily made, the wire frame furnishing an immovable base for their firm attachment.

From the foregoing description it will be observed that an attempt has been made to combine the advantages presented, respectively, by the fracture-box and the two forms of apparatus previously alluded to (Desault's and Hartshorne's). In simplicity of construction and ease of application, in the firm lateral support to the injured limb, in the unyielding extension and uniform counter-extension, and in the facility afforded for making applications or renewing dressings, the new apparatus presents, I think, points worthy the attention of practical men. It can be adapted to either limb by unscrewing one segment of each hinge and shifting the leaves of the box to the opposite sides. Although designed particularly for the treatment of compound fractures, it will answer every indication in the management of the simple form, as shown in Case 2.

If the fracture be transverse, or nearly so, the shortening will be scarcely perceptible, if at all; but if oblique, I cannot flatter myself that it will do more than to effect a diminution of the shortening of the limb, which inevitably results from a positive overlapping of the fragments.

ART. VII.—*Hydrocele of the Neck, and its Treatment by Excision, with Two Cases.* By A. REEVES JACKSON, M. D., of Stroudsburg, Pa.

It is a pretty well established pathological fact that the form of cystic disease termed *hydrocele of the neck*, is produced by an enlargement of the interspaces naturally existing in the areolar and fibro-areolar tissues, and by the retention within them of an increased amount of the moisture normally present; and that the pressure on the surrounding tissues condenses them into a fibrous wall of varying thickness, usually rough and adherent on the outside, and in which is generated an epithelial lining membrane endowed with the power of independent secretion.

The term *hydrocele of the neck* was first applied to this disease by Prof. Maunoir, of Geneva, who defines it to be “simply a collection of serous or lymphatic fluid.” Previous to the publication of his views, the disease had been looked upon as “softened bronchocele,” and the term “hydro-bronchocele” was also erroneously used to designate it. It is now, however, generally admitted to differ from bronchocele in any of its forms or conditions; for although frequently connected more or less intimately with the thyroid gland, from the cellular spaces of which it may and does arise, yet it quite as frequently occupies the space between that body and the sternum, apparently originating in the cellular substance between the sterno-hyoid and the sterno-thyroid muscles, and does not in any case necessarily involve the proper structure of the gland. Indeed, all the modern authors who have written upon the subject, and among whom may be mentioned O’Beirne, Cesar Hawkins, Bransby Cooper, Mütter, Dupuytren, McClellan, and others, agree in stating that it may occur in *any portion of the neck*.

Structure, Course, and Development.—The disease usually begins as a small, insensible tumour, growing slowly and steadily until some accidental cause, as a blow, produces inflammation and causes a more rapid growth. Commonly the development is very slow and regular, although occasionally the tumour attains a considerable size in a few months. In some cases the tumour goes on increasing steadily in size for a time, and then remains stationary for months or even years, when, without the supervention of any apparent cause, it again commences to increase. The maximum size usually attained by them is that of a small orange.

These cysts, like those occurring in other parts of the body, may be

either *simple*, *multilocular*, or *included*. They are also *congenital* or *acquired*.

The walls of the cysts vary considerably in thickness, consisting sometimes of a single lamella, exceedingly thin, delicate, and transparent, and in other cases they are a quarter of an inch thick. In one specimen in the possession of the writer, the cyst wall throughout a considerable portion of its extent is three-eighths of an inch thick. It is of a dense, fibrous, non-elastic tissue, with the fibres crossing and interlacing with each other in every direction.

In a remarkable case of the congenital form of the disease related by Dr. Mütter,¹ "the fluid was contained in several sacs of varied size, and each apparently independent of the others." "There were three large and two small sacs; also a number of smaller cysts lay scattered along the trachea, and through the cellular tissue of the neck, but each was distinct from the others, and none had any communication with the cavity of the thorax." The larger sacs "ran up as far as the pterygoid processes of the sphenoid bone, lying on the anterior part of the spine, and one of them covering the sternal end of the clavicle and the end of the sternum itself."

In this case death ensued "from œdema glottidis caused by pressure of the distended sacs." "The mucous membrane of the larynx was reddened slightly, and the rima glottidis was almost closed by effusion into the sub-mucous cellular tissue."

In another case reported by Mr. Bransby Cooper, there were "three distinct sacs not communicating."

The fluid contained in the sacs also varies very greatly both in regard to its physical and chemical properties. So long as the secreting surface of the sac performs the functions assigned it by nature, and is not changed by inflammation, the contained fluid is pale, limpid, almost colourless, or slightly tinged with yellow, resembling the serum of the blood both in colour and consistence, and with which it is perhaps identical. When the lining membrane of the cyst has been altered by inflammation or its consequences, the secretion also undergoes change, and may then become thick, glairy, yellowish, or greenish in colour, purulent or bloody. It is sometimes dark and thick, resembling tar or treacle. It is always more or less coagulable by heat, alcohol, and acids.

Symptoms.—Hydrocele of the neck presents itself as a smooth, globular tumour, covered with healthy, natural skin. Although usually painless, and free from the least tenderness on pressure, yet occasionally patients complain of a sharp, darting pain in the part. Fluctuation, although sometimes obscure, is always present with more or less distinctness. Sometimes there is produced a violent spasmodic cough, with oppression of the breathing when the tumour is large. In cases where the vessels are en-

¹ Philadelphia Medical Examiner, vol. vi. N. S., p. 257.

croached upon we may have epistaxis, headache, tinnitus aurium, and vertigo. In one case, mentioned by Mr. Cesar Hawkins, the vessels of the chest were in a varicose condition. When the œsophagus is pressed upon by the tumour, deglutition is sometimes seriously interfered with, and a sense of tightness and constriction about the throat is produced.

Etiology.—The exciting cause of the disease, as it occurs congenitally, is involved in obscurity. The predisposing cause is unquestionably to be found in the weak and relaxed condition of the parts and tissues during foetal life.

The acquired form of the disease may occur after birth from any cause producing inflammation in the cellular tissue of the neck. Vidal suggests that it may arise from “deposits” of blood, encysted and gradually transformed into a serous fluid. Boyer inclines to the belief that they may be a mere development of a mucous bursa.

Diagnosis.—The diseases with which hydrocele of the neck is liable to be confounded are goitre or bronchocele, aneurism, varix of the internal jugular vein, swelling of the lymphatic cervical ganglia, fatty tumours, chronic abscess, tumour of the parotid gland, subcutaneous nævus, and malignant tumours.

From *goitre* it may be distinguished by its partial translucency, its globular shape, its soft, elastic feel, and the absence of the enlarged condition of the veins almost invariably present in bronchocele.

Hydrocele of the neck may pretty closely simulate *aneurism of the carotid* when it is situated over an artery. The points of distinction are the slow growth of the former, the absence of the bellows-sound, and the ease with which the swelling may generally be drawn from the vessels when the muscles are relaxed by bending the head forward.

Varix of the internal jugular vein appears in the form of a tumour just at the upper end of the sternum, sometimes as large as an egg, having a bluish colour, and a tremulous, vibratory movement. It may be made to disappear in a greater or less degree by pressure made pretty firmly upon the distal portion of the vessel.

Enlargement of the cervical ganglia is usually accompanied by similar enlargement in the immediate vicinity, and by other evidences of the scrofulous diathesis on which it depends. In these cases there is always a strong tendency to suppuration. These points will be sufficient to enable the surgeon to avoid a mistake.

Fatty tumours are painless, obscurely fluctuating, circumscribed, and present a smooth surface. When situated in the neck, these characteristics cause them to simulate very closely hydrocele of the neck. In cases of doubt the exploring needle or trocar will promptly enable one to decide correctly.

Chronic abscess may be and has been mistaken for hydrocele of the neck. Here, also, the use of the needle or trocar affords a ready means of diagnosis.

Parotid tumour. When hydrocele of the neck is small and occupies the parotid space, and is bound down by the fascia of this region, it is very difficult to make the diagnosis. It is firm and smooth, as is also tumour of the parotid. As the diagnosis is of very great importance in reference to treatment, we must of necessity resort to the needle or trocar. When the hydrocele is of large size the diagnosis is sufficiently simple.

Subcutaneous nævus, involving the front of the neck, has been confounded with hydrocele. Dr. Mütter relates a case of this kind. The distinguishing marks are the soft, pulpy feel of the tumour, its variation in size at different times and under pressure, the usually purplish colour of the skin, and the unusual vascularity of the neighbouring parts. When there are many small cysts, some half empty, as sometimes occurs, the diagnosis would be attended with much difficulty.

Malignant tumours might be confounded with the multilocular form of the disease whenever in the latter *the skin becomes red and the tumour painful*, as in a case related by O'Beirne. The points of distinction are to be found in the history of the case, and the general health of the patient.

Prognosis.—In the acquired form, under proper treatment, this is usually favourable. The congenital form is frequently, perhaps generally, fatal in a few months, although sometimes susceptible of successful treatment. The danger in any given case depends, of course, upon circumstances. If there is much involvement of important organs, either from the size or direction of the growth, the danger will be much greater.

Treatment.—Cysts as a class, although non-malignant, are especially liable to degeneration, more so, perhaps, than any other class of benign tumours. Prof. Miller attributes this to the fact of their having an independent secretory surface which may at any time take on perverted action. Hence the propriety of early and entire removal by operation.

They are not amenable to absorption; and any attempts to discuss them can only result in disappointment to the surgeon, and injury to the patient. Mere failure to remove such growths by stimulating applications, is not the only evil of such treatment. By frequent stimulation their growth is accelerated; adhesions become more numerous and firmer; and degeneration is favoured and sometimes even produced by it.

The different methods of treatment which have been proposed and practised for hydrocele of the neck, are the following, viz: 1. Acupuncture; 2. Repeated tappings; 3. Incision followed by pressure; 4. Incision followed by stimulating applications to the inner surface of the cyst; 5. Stimulating injections; 6. The seton; 7. Excision of the cyst.

1. *Acupuncture.*—This measure is of no practical value. It has been tried in several instances, and, although the size of the cyst was temporarily diminished, in a few days it became as full and as large as before. It is therefore abandoned.

2. *Repeated Tappings.*—This operation may be used in some cases as a

palliative measure where incision might not be deemed prudent, or where the patient objects to excision. The objection to this mode of treatment is the danger of serous infiltration of the cellular tissue which would be productive, as in a case related by Prof. Maunoir, of grave consequences.

Should this plan of treatment be adopted in any case, care should be taken *to draw off every particle of fluid*, in order that the small opening made by the trocar may be healed before any great amount can be again secreted. It would be well, also, after the evacuation of the fluid, to apply sufficient pressure to keep the walls of the sac in contact. This would increase the chances of a radical cure.

3. *Incision followed by Pressure.*—Heister, Delpech, Lawrence, and some others, recommend, in certain cases, “free incision of the sac, followed by moderate compression.” It is only adopted to the simple variety of the disease, and when superficially located. It is sometimes followed by very severe inflammation, and should, therefore, not be employed, in the cases of young persons. It is not adapted to the congenital form of the disease, and even when successful it leaves a thickening of the parts from the presence of the sac, and a large unsightly scar.

4. *Incision followed by Stimulating Applications.*—It has been attempted to cure hydrocele of the neck by exciting a “new action” in the secreting surface of the sac; and for this purpose different substances have been used by different surgeons. Thus, after incision the surface has been painted over with a strong solution of *iodine* and *iodide* of potassium—pieces of lint have been inserted; also, *tincture of camphor*, solution of *sulphate of copper*, port wine, vinegar, and alcohol. It is, however, a very dangerous operation, and should be abandoned. In order to be successful a very high degree of inflammation must be produced, and the treatment is tedious, painful, and a large irregular scar is produced.

5. *Stimulating Injections.*—Prof. Maunoir, at an early day, made attempts to avoid the scar of incision, and to effect the obliteration of the sac by injections of substances into it after withdrawing the contained fluid. It was soon abandoned, however, as he found that mild injections produced no effect upon the dense walls of an old cyst, and when very stimulating the effects were alarming, spasmodic symptoms being quickly produced.

In a case related by McClellan,¹ the cyst was punctured, and the sac, after being emptied of about “a pint of thick greenish serous fluid tinged with blood,” was injected with warm water. In two days the wound had united, but the sac had filled up to its former size, with *great difficulty of breathing*. During a week the patient had great pain, and suffered from severe and even alarming constitutional irritation. The sac was then punctured opposite the former incision, and a quantity of bloody serum was discharged to the great relief of the patient. Free suppuration then sat in,

¹ McClellan's Surgery, page 318.

which obliterated the sac, although much induration and enlargement of the neck remained.

This case exemplifies the hazard attending this mode of treatment, which is now, I believe, never used.

6. *The Seton*.—This remedy should take rank next to excision, both as regards certainty and safety. It was first introduced by Maunoir, and has been successfully used by many surgeons subsequently. Prof. Mütter relates a case successfully treated in this way. It is not applicable to cases of multilocular cysts unless several setons are used. Neither is it applicable, usually, to cases of the congenital variety of the disease.

The plan of O'Beirne, which obviates the danger from infiltration which may arise when the threads are passed through openings made with a trocar is as follows: "*A free incision, not involving the sac, is made at the upper and lower extremities of the tumour by raising and afterwards dividing a transverse fold of the integument at each of these points. The sac is then opened above, and a long probe, armed with a sufficient number of threads, passed down to the most depending point against which it is firmly pressed. The sac is then cut upon the probe so as to allow the instrument to pass and the seton to be introduced.*"

The tumour should, for a few days, be dressed with a light emollient poultice, and afterwards with lint and a roller, until the removal of the seton.

The advantages of this method of treatment are, that it is safe, readily performed, and tolerably certain. Besides these, it has the merit of leaving a very small scar. On the other hand, it is very tedious and uncleanly, and in some cases may produce mischief by the inflammation it excites. Prof. Pancoast gives an account of a case¹ in which the inflammation resulting from the use of a seton introduced for the relief of a patient with hydrocele of the neck, produced a *second cyst* in the immediate vicinity, on the top of the right thyroid lobe, and which was finally extirpated by him successfully after the manner of Prof. Porta.

7. *Excision*.—The excision of the sac is, undoubtedly, the best mode of treating this disease in all cases to which it is at all applicable. It was first proposed and successfully practised in several cases by Prof. Porta, of Padua, and, subsequently, by Profs. Pancoast and Mütter. The operation is not usually difficult so long as the cyst remains entire, although much caution and a knowledge of the anatomy of the parts concerned is essential. I believe there are but few cases where excision will not be a more certain, safe, speedy, and less painful plan of treatment than any of the other modes proposed. We must bear in mind the fact that these cysts are not parts of the proper structure of the neck, but merely a new development in the tissues—independent growths. Even in those cases in which they originate

¹ Med. Examiner, vol. vii., N. S., p. 504.

in the cellular interspaces of the thyroid gland, they are not generally accompanied by any change in the proper gland structure. It has been supposed that this measure was entirely inapplicable to the congenital variety of the disease (see Case 2). Even here it may be successfully employed, provided other circumstances are favourable for the operation.

CASE 1. Hydrocele of the Neck successfully excised.—Mrs. H., aged about forty, applied to me, some time during the spring of 1849, with a tumour situated on the front part of the neck. She stated that she had first observed a small, hard swelling on the middle of the neck when about twenty-eight years of age, and that it had been growing slowly and steadily since that time. It had never been painful, and was not tender under pressure. Neither had it produced any inconvenience in any way, and she now only objected to the deformity, and desired to have it removed on that account.

At the time of the examination the tumour was the size of an orange, globular, with its greatest bulk a little to the left of the mesial line, and movable to a slight extent. Fluctuation was tolerably distinct, and the tumour was entirely free from pulsation. The skin covering it presented a natural appearance.

I had no difficulty in recognizing it as a case of hydrocele of the neck, and advised its removal by excision. Accordingly, a few days subsequently, having previously placed her under the influence of an anæsthetic mixture consisting of two parts by measure of sulphuric ether, and one part chloroform, the operation was performed with the assistance of Dr. G. A. Kaski, of Bartonsville.

A vertical incision was made in the middle line of the neck, extending from a point just below the chin nearly to the upper end of the sternum. The layers of cellular fascia and the platysma myoides muscle were then divided upon a grooved director, and the cyst, which presented a yellowish-white appearance, brought fully into view. The dissection was continued upon each side, alternately, with the edge and the handle of the knife, and, finally, the attachments being quite feeble the evulsion was completed with the fingers. A small branch of the superior thyroid artery was found passing over the right side of the cyst in such a manner as to make it necessary to tie it. No other vessel required ligature.

The wound was closed with stitches and adhesive strips, and cold water dressing applied. The entire length of the wound, with the exception of the point where the ligature was brought out, healed by the first intention, and the patient made an excellent recovery without any untoward symptom.

The cyst was simple, and its wall was about one line in thickness. The contained fluid was pale, slightly turbid, and in it were floating a number of small white flakes.

CASE 2. Congenital Hydrocele of the Neck; Numerous Included Cysts; Excision.—In the early part of June, 1856, Mr. James Smiley, of this county, consulted me with regard to a tumour on the neck of his child, a boy three years old. The swelling was noticed immediately after birth, and had been growing up to the present time. It had now become so large as to interfere materially with motion of the child's head in a forward direction. I advised its removal, and, on the 30th of June, 1856, it was excised with the assistance of Dr. A. Levering.

The operation was essentially the same as that described in Case 1; but during the operation the wall of the sac gave way under the grasp of the forceps, and its removal was completed with some difficulty in consequence.

In this case the main cyst was divided into three chambers by two septa, one of which was complete and the other crescentic in shape, incomplete. Attached to the main cyst walls were numerous included cysts varying in size from the size of a pin's head to that of a grape. There were, perhaps, as many as thirty of these, each containing a clear, amber-coloured fluid. The walls were exceedingly thin and delicate, and were very easily ruptured.

The contents of the main cyst consisted of a dark-brown, turbid fluid, evidently containing partially decomposed blood.

No vessels required ligature. A slight venous hemorrhage from the bottom of the wound was checked by pressure with sponges previously dipped in cold water.

The edges of the incision were brought together and retained by means of metallic sutures, and cold water dressing applied. Union was complete in about two weeks, during the whole of which time the sutures were allowed to remain without producing any appreciable amount of irritation.

The wall of the cyst in this case varies very greatly in thickness at different parts; in some being not more than a fourth of a line, while at others it is fully three-eighths of an inch thick.

Remarks.—I have not been able to find an account of any other case of congenital hydrocele of the neck in which excision was used. Indeed, the opinion of those persons who appear to have investigated the subject is, that such cases are not amenable to treatment at all, and they even discourage attempts to deal radically with it in any manner. The late Prof. Mütter states that, in such cases (congenital) treatment can be of but little avail, and that children so affected seldom live more than a few weeks. The foregoing case shows, however, that they should not be abandoned, and that where the child has attained a proper age, and the general health is good, we may reasonably expect good results from proper treatment.

TRANSACTIONS OF SOCIETIES.

ART. VIII.—*Summary of the Transactions of the College of Physicians of Philadelphia.*

1860. March 7. *Rupture of the Uterus and Death from Hemorrhage in the Sixth Month of Pregnancy.*—Dr. CORSE exhibited a specimen of ruptured uterus, with its contents, and gave the following sketch of the case:—

I was called to see Mrs. B., æt. 28, about the middle of the day, in consequence of incessant vomiting and cramps in the abdomen, with great pain.

The patient was sitting in a chair near an open window, the weather being very warm. Her pulse was frequent and feeble, her tongue furred, and the surface of her body pale and covered with perspiration. She had occasionally indulged in the excessive use of alcoholic drink, and on this day had undertaken a promenade through the streets while under its influence, but was unable to accomplish it, and had a carriage called to take her home; soon after which the vomiting and cramps began. I was informed that she was near six months gone in pregnancy.

I prescribed remedies appropriate to the gastric irritation of the inebriate, and left her. The vomiting soon almost ceased, and the cramps diminished in frequency but rather increased in violence.

About seven o'clock in the evening I received a message to come in haste to her, and found her sitting in a chair, reclining in the arms of a female friend, in a state of insensibility, with her feet in a warm mustard bath, while another female friend was rubbing her legs. They supposed she had just fainted; we immediately laid her upon the side of the bed, and soon found life to be entirely extinct.

The corpse was placed in ice; and, after much persuasion, I obtained the privilege of making an autopsy.

On opening the abdomen, which was performed in the manner for delivery of a living child by gastrotomy, the whole anterior part was found covered by a thick clot of blood extending from one lumbar region to the other. Upon removing this I discovered the source of the hemorrhage in a rupture of the fundus of the uterus. The rupture was not a complete opening of two edges as if from ramollissement or other disease, but a separation of the uterine structure, tearing the ruptured margins into fibrous fringes. Section across the rupture transversely, through the substance of the uterus, did not show any disease whatever, and we are left to conjecture in regard to the predisposing cause. The direction of the rupture was transverse, its length about two inches, and the location in the posterior slope of the fundus, not low enough down to come in contact with the spinal column.

The implantation of the placenta was left lateral, the edge extending across the front; the funis was central.

The location of the lesion does not admit of the supposition that it arose from mechanical injury applied from without directly to that part of the body, nor from pressure against the sides of the vertebræ; and, in the

absence of any known liability, I have been led to surmise that it was due to some injury in one of her former pregnancies, this being the third. Of this her friends have no knowledge.

The true etiology of the accident admits of some difference of opinion. Some suppose violent contractions of the organ itself set up by the disorder of the general nervous system from drink as the exciting cause; others attribute the injury to the violent contractions of the abdominal muscles pressing upon the uterus during spasm; others think it due to the violent straining to vomit, while I suspect a fall upon her abdomen some time during the afternoon, after I left, to have been the immediate source of the disaster.

Intra-Uterine Hydrocephalus, and delivery accomplished by Craniotomy.—Dr. CORSE also narrated the following:—

Mrs. H., pregnant of her fifth child, was taken in labour a little after midnight; pains being active, the os was soon dilated; examination per vaginam showed some hard body presenting of unusually large size. There being no urgent symptoms, labour was allowed to go on for about two hours; it was then supposed that she would not be able to deliver herself unaided, and I was called. I discovered an unusual size of the fontanelles and a great width of the sutures, but supposed that the shape of the head might be made, by aid of the long forceps, to undergo such change as would admit of its delivery by that means. The forceps were applied, and traction and compression made, as far as prudence would permit, without avail.

The most dependent part of the head at the centre of the pelvis was then perforated, and a large amount of fluid evacuated; the size of the head being thus reduced, it was extracted by means of forceps without much difficulty. The patient soon reacted, and no bad symptom arose from the operation.

The measurements of the head are as follows: From the occipital protuberance to the frontal protuberance, six and a half inches; from the occipital protuberance to the chin, six inches and a quarter; from one parietal protuberance to the other, five inches and three-quarters; from the posterior margin of the foramen magnum occipitis to the top of the head, five inches; from the top of the frontal bone to the chin, four inches and a half; from the chin to the nose, one inch.

April 4. Fungus Hæmatodes involving the Ovaries and Uterus at the age of twelve years.—Dr. CORSE read the following note of a case of cancerous disease of the abdominal and pelvic viscera:—

Miss N., aged twelve years, rather small for her age, and slender, but generally healthy, presenting no signs of puberty, and apparently of rather retarded development, was taken with vague abdominal pains, like the colic of young persons; the pains were referred to one part, then another, of the abdomen. They were at first treated by gentle domestic remedies, such as carminatives, gentle stimulants, and laxatives. These remedies being unavailing, they sent for their family physician, who at once discovered an abdominal tumour, pronounced it extraordinary, and proposed a consultation, upon which I was called. When I first saw her, she had become somewhat thinner, but was yet going to school. Upon a careful examination, we found an irregular tumour in the hypogastrium which descended low down into the pelvis, and a per vaginam and per rectum touch showed the whole mass to be connected together. After summing up all the particulars, we agreed upon an unfavourable prognosis, and I expressed the opinion that the disease was fungus hæmatodes. The patient rapidly grew worse in spite

of tonics and alteratives. The abdomen became enormously distended, and fluctuation indicated the presence of a fluid. At length the diaphragm was so much pressed upon as to threaten immediate death by asphyxia. It was now thought proper to perform paracentesis abdominis, not as a cure, but as offering the only means of prolonging her life. A puncture was made with a trocar, in the median line, about two inches below the umbilicus, and a ropy sanguineous fluid slowly poured out, to the amount of about three pints. This fluid, subjected to microscopic examination, presented the peculiar cells said to indicate cancer. A marked amelioration of suffering followed the operation. The improvement, however, was of short duration; the abdomen quickly filled again, and the emaciation became extreme. Now vomiting came on; the matter ejected was black and resembling coffee grounds, and somewhat slimy. Soon after she became moribund, and expired on the night of the twenty-third day after I first saw her, and not more than five or six weeks after the first discovery of the disease.

On post-mortem examination the next day a quantity of sanguineous fluid, like that obtained by the previous operation, was first evacuated; then a soft pultaceous mass occupied the opening in the abdomen; and transverse incisions, laying open the whole abdomen in four flaps, showed the entire space filled with a soft cancer matter, even up between the stomach and diaphragm. The whole uterus and ovaries were in one soft mass, a part only of which would hold together to be removed. The once tolerably hard tumour had, as is usual with these growths, become quite soft with its increase in size. The greater part of the fungous growth was scooped out with a saucer; and we could then see that the walls of the abdomen were the seats of attachment for this matter; numerous tumours of all sizes, from that of a small shot to an inch or more in diameter, were found to be appended to the omentum and to the intestines and mesentery.

The whole substance removed was placed in a common bucket and weighed eleven pounds and a half. The fluid which escaped when the abdomen was opened, was supposed to be about two pounds, and the portion remaining in the abdomen about one pound, making in all *fourteen* pounds and a half.

I could not learn of any case of cancer ever having occurred before in the family.

June 6. Feigned Mutism detected by a Strong and Prolonged Shock of Electricity.—Dr. ADDINELL HEWSON read the following history of a case of this:—

On the 16th day of July last (1859), a boy, apparently 14 years of age, was taken up in the street for begging, and committed to the House of Refuge by one of the aldermen of the city. At the time of his arrest he intimated to the officer that he was unable to speak. He could, however, evidently hear perfectly well. He wrote his name in the alderman's office as John Thompson, born in the State of New York, and gave his age as thirteen years. At the Refuge he also gave them to understand that he could not speak, and that he had lost the ability to articulate sounds in his early infancy. He was found to be able to read with facility anything that was written for him, and to write a quite distinct and well-formed hand himself. My colleague, Dr. Haskell, who was on duty at the time of his admission, examined him carefully, and desired that he should be closely watched, so as to detect, if possible, whether his mutism was feigned or not. But from that date up to the close of February, a period of nearly eight months, during which he was constantly at work or at play with the other boys, no one ever heard him utter a single distinct word save once, shortly

after his admission, when he was locked in his room for some misdemeanor. He then called to a boy passing the door to know if the bell (for dinner) had rung. When accused immediately afterwards of having uttered these words, he admitted that he had, but indicated his inability to repeat the same or any other words, even under the threats of punishment. His whole conduct, subsequently, was so very consistent with his statements that those of the managers and officers, with whom I conversed in reference to his case, expressed themselves convinced of the boy being either absolutely unable or under a delusion as to his ability to talk. Indeed, one of the managers, who took a special interest in the boy, wrote me, in February, a letter in regard to him, requesting me to examine him carefully as to the possibility of any surgical operation being performed to restore his voice; and if such was not possible, that I should recommend his being sent to the Deaf and Dumb Asylum.

On my first interview with the boy I found him remarkably bright, with an expression of face quite characteristic of deaf mutes. He had a restlessness of eye, and remarkable quickness of manner. He communicated with me very freely with the single hand alphabet, which he stated he had learned whilst in the institution from one of the teachers. To my inquiry as to where he had learned to read and write so well, he gave me to understand that his mother had shown him the letters and taught him their meaning; that she had been very devoted and assiduous in her efforts to teach him. The teacher, from whom he stated he had learned the mute alphabet, assured me afterwards that he had never shown him the letters but twice.

His organs of speech, on careful examination, were found by me to be in a perfect state. His palate, tongue, fauces, and larynx were, as far as I could ascertain, perfectly natural, and to my frequent solicitations for him to utter a single word or letter, I got only a negative shake of the head. The boy evidently would not attempt to articulate a sound; and the fact that he had spoken once in the institution satisfied me that there was merely an unwillingness and not an inability to speak. I so expressed myself at the time, but stated then as there seemed such a strong conviction against the correctness of my conclusion that I should like to have more facts from him in relation to his case. In consequence of this wish of mine, one of the managers intimated to him that he wished him to write down his history, which he did in the following letter, which I will read; it will give some idea of the boy's intelligence:—

PHILADELPHIA, February 27, 1860.

Dear Sir: I now take the opportunity of writing the most of my history to you as you desired I should. I was born near New York, on the banks of the North River. My mother died when I was nearly ten years old. My father went to sea about one year before and was drowned, and the neighbours said that they thought it caused her death, for she slowly pined away from that day and died about one year after. I then went to live with my uncle, who was a sailor, and often got drunk, until one day, when I was nearly twelve years old, in the year 1858, and would not let me come home again. I then went to the farmers to get work, back of Brooklyn. I then went to farmer Mr. Dixon, who was kind to the poor, and often gave them money to help them along during the winter; but he had too many boys engaged to work for him during the summer; he, therefore, kindly put me under the care of one of the neighbour farmers whose name was Mr. Henderson. He was a kind but a harsh man in speech. I stayed with him until the first of July, and being of a rambling disposition I ran away from him with a dollar and a half in my pocket. I intended to visit the United States with it. I got as far as Buffalo with a half of a dollar. I then worked my way through Ohio, and thought I would like to visit Philadelphia;

so I started back to the way of Buffalo, and reached Philadelphia on the 15th of July, 1859, and come here on the 16th, and have been here ever since. Mr. McKeever can tell you the rest.

Respectfully yours,
JOHN THOMPSON.

To Mr. F.,
Manager of the House of Refuge.

At subsequent interviews I frequently tried to discover where, precisely, the persons were to be found to whom he referred in this letter, that I might communicate with them in regard to him, but my efforts were not successful. He said he did not know. I also made frequent attempts to get him to utter some sounds. At first he would not, then he gave utterance to a squeaking guttural sound. Then, by dint of perseverance, I got him to pronounce after me some labial sounds, such as *b*; some of the hissing sounds, such as *c* and *s*; but I never could get him to attempt the distinct lingual and guttural sounds by which I hoped he would betray himself. My conviction of his feigning was not in any ways, however, diminished by these interviews, although my researches in works on feigned diseases and medical jurisprudence were not rewarded by the discovery of a single case approaching to it; but, on the contrary, in the appendix of Mr. Wilde's work on the Ear (see p. 438, American edition), I there saw some cases of real mutisme so analogous to this one that I might well have had some misgivings as to the correctness of my opinion, especially with the strong testimony of the experienced officers of the institution in opposition to me.

The fact, however, that the boy had been heard to speak, and admitted that he had, was an insurmountable one, and two expedients presented themselves to me by which I thought I might be able to determine the truth. These were a strong shock of electricity and the intoxication of anæsthetic agents. I determined to try the electricity first, and accordingly on Saturday, the twentieth of March, with the assistance of my friend, Dr. Charles Lee, I administered a strong shock of the induced current from a large magneto-electrical apparatus by means of moistened conductors applied over the larynx. This application, I assured him, would bring his voice back, and I told him I would continue it until he made some effort to speak—until he uttered some articulate sound. He held out for some time, at least twenty minutes, although the application was evidently very painful. Finally, however, he cried in quite a suppressed tone, "Enough!" I then ceased; and feeling satisfied that the boy had been severely punished, I did not resume the application, although he would not utter another word.

On the following Monday I intended to repeat the battery, and, if it was not more successful, to put him under the influence of ether. When he saw me, however, on that day approaching, in the hall, with the battery and bottle of ether in hand, and in company with my colleague, Dr. Haskill, he came forward and said, in a slow measured cadence, and as though he articulated with difficulty, "How do you do, sir?" and I was informed that he had been gradually *recovering* his voice since I had used the battery on Saturday. He then reiterated, in answer to my inquiries, the facts contained in his letter of February 27, to the managers in reference to his history. Some of those circumstances I was confident were not correct, and I told him so; but he assured me he was telling the truth. He continued to *improve*, and in the course of a few days it was discovered that he could read poetry in a very creditable manner, and when questioned as to where he had learned to read with such good measure, he quickly answered that he had noticed the boys reading poetry at the Public School, near Brooklyn.

He now became quite a *lion*, and when I took him before the managers to induce them to resort to some coercion to get at the truth in his case, I found the most experienced of them under the conviction that I had either brought the voice to a child where it had never been developed, or that the child had been suffering under a hallucination that he could not speak. These impressions prevailed for six weeks, when one day the boy, of his own free will, gave the superintendent the following statement in his own handwriting:—

PHILADELPHIA HOUSE OF REFUGE, May 4, 1860.

Having feigned dumbness for about nine months, and made several erroneous statements while in this institution in regard to my history, I now truthfully declare that my object in doing so was to avoid the possibility of detection as a runaway from my parents, who reside in Alleghany City, opposite Pittsburg, on the Alleghany River, in Pennsylvania. I left Alleghany City on the 1st of July, 1859. I took with me a box partly full of lozenges, so as to make my parents believe I would sell them. When I got to the river I commenced to call the boys' attention by daring feats; at last I got a thick plank, and went out into the middle of the Alleghany River. I floated down the river on it, and the boys on the shore thought I was drowned, I suppose. I must now come back to Mr. S. S. East, he was a news dealer in Alleghany City. He always trusted me, and I now owe him two dollars and a half, which I would like to pay very much. He resides near the dépôt. I then travelled partly through Ohio, and from Ohio to New York City. Stayed there two or three days and then came to Philadelphia, and on the next day, Monday, July 16, 1859, where I remain to this very day. I am not induced to make the above statement through fear of punishment, but that I may be relieved of a great uneasiness which I feel on account of practising such a deception.

CHARLES L. C., alias JOHN THOMPSON.

P. S.—I forgot to write that I had five brothers and two sisters, and father and mother both living in the city of Alleghany.

Names.—Mr. Isaac C., Mrs. Sebrina, David, John, me Charles, William, Albert, Jane, George, and Emma.

A letter was then written to his father, in Alleghany City, and the following answer received, showing the case to be one of the most extraordinary of feigning mutisme on record:—

ALLEGHANY CITY, May 14, 1860.

Mr. WM. KEEVER, *Dear Sir*: Yours of the 8th inst. I have received. I hasten to reply. We are truly thankful to you for this favour. 'Twas read with gladness by us all. Our son, Charles L. C., left us July 2, 1859. This is the first intelligence we have had of him. C. L. was not a very bad boy until he became acquainted with some very bad boys in this city. I think it was those who led him into mischief. He went to school until in April, 1859. The hard winds had injured our public school-house so much that it was torn down and a new school-house has been erected. When there was no school he was in the streets with these boys, who, we think, have led him off. Before he left he often talked of running away, as I have learned since. We think that the experience he has had in running away has learned him a lesson. We hope that it will have a good effect on him for time to come. We are now about removing some distance in the country to live on a farm. We think that our children (especially Charles L.) would not be so easily influenced wrongfully in the country as they would be in the city. I am a machinist. I am in the employ of the Pittsburg, Fort Wayne, and Chicago Railroad Company. My wages is sufficient to keep my family and but very little over. When I received the letter I at first thought that I would go immediately to Philadelphia to bring home the absent boy, but to do so would cost more than I could very well afford. Sir, you would oblige me very much, in my present circumstances, if you would send him on the Pennsylvania Central Railroad, in care of the conductor, to Pittsburg. Please to let me know what day and what hour you could send him and I will meet the con-

ductor in Pittsburg when the train arrives and pay his fare, &c. Please to answer soon, we are very anxious to hear from you again.

Yours, very respectfully,
ISAAC C.

Address, Alleghany City, Pa.

Local Paralysis, with Softening of the Brain, supervening on Hypertrophy of the Heart.—Dr. OWEN J. WISTER read the following history of the case:—

J. R., a butcher, aged 62 years, of temperate habits, had been affected for several years with hypertrophy and valvular disease of the heart; but with the exception of some shortness of breath, on occasions of unusual exertion, suffered little from this cause, and was in possession of vigorous health.

About two years ago he had an attack of apoplexy, accompanied with paralysis of the left side, which, however, gradually disappeared, though some confusion of mind, when his attention was particularly engaged, especially with business accounts or other troublesome subjects, continued for several months; and the clearness and decision of his mind seemed never to have been completely regained. His motions were less active, and the integrity of the nervous system was slightly but perceptibly impaired.

His general health, however, was good after this attack, until January 3, 1860, with the exception of two or three very slight recurrences of dullness and confusion of mind, with pain and a sense of pressure about the head, which were readily removed.

On the night of January 2d last, having gone to bed in his usual health, he was seized with pains and a sense of weight at the epigastrium, accompanied by nausea and violent efforts to vomit. Large quantities of warm water were administered, which, however, were rejected almost unmixed with any other matter. The emetic and a stimulating enema having afforded considerable relief, he fell into a natural sleep, from which he awakened in a state of great excitement, complaining of pain in the head and raving in a wild, incoherent and almost inarticulate manner. He was entirely deaf, the optic axis greatly distorted, and the vision very dim; the left side, including the face, completely paralyzed; the pulse rapid, weak, and fluttering, and the extremities cold: coma speedily supervened, from which he was aroused by cups to the back of his head and neck, hot mustard foot-baths, and repeated enemata, the total inability to swallow preventing the administration of any remedy by the stomach.

Under this treatment, together with blisters to the occiput, the symptoms had all abated, at the end of the second week, with the exception of partial paralysis of the facial muscles and complete loss of the power of deglutition. Speech, though somewhat slow, was distinct; the deafness had moderated to slight dullness of hearing; the strabismus had disappeared, and vision was entirely restored.

At the end of the fourth week the left submaxillary gland became swollen, red, and painful, and finally suppurated, producing some relief from the heat and fulness of the head, which had been more or less complained of.

On the first day of March, nearly two months having elapsed, the power of swallowing suddenly returned. I say suddenly, because the attempt had been made every day since the 3d of January, and utterly without success.

During the suspension of deglutition life had been sustained by means of the stomach tube; and although the condition of the patient had varied very considerably as to strength and vitality, though the respiration was at times laboured and interrupted, the extremities cold and œdematous, the pulse scarcely perceptible, and the tongue dry and covered with dark brown

fur, but little emaciation had taken place, a proof that nutrition had been pretty well sustained.

With the power of swallowing came appetite; but though he was well fed, and the functions pretty well carried on, the abscess in the submaxillary gland did not heal, nor his strength or condition in any way improve.

Until the 14th of April little change was observed, and, with the exception of a distressing sense of heat, which was not appreciable on feeling the surface, there was but little suffering. During the evening of that day he had a severe chill, followed by great agitation and trembling, after which he gradually sank, and died about two o'clock the ensuing morning.

Autopsy.—The head only was examined. The dura mater was more than usually adherent, but otherwise normal, with the exception to be mentioned below. On opening it about three ounces of serum escaped. The arachnoid was normal, except that at the base of the brain it was somewhat opaque. The pia mater was thickened as if by inflammatory deposits over the outer side of the middle lobe of the left hemisphere. The cerebrum was firm, the convolutions being distinct and full except at the outer and lower part of the left middle lobe, where the gray matter was softened over a space of two square inches. The softening extended to the depth of one or two lines, the tissue being of a dirty white colour. The right lateral ventricle contained a small amount of serum; the left contained none. The choroid plexus was much enlarged on each side, and on the right to thrice or four times its natural size. The remaining central part of the brain and medulla oblongata were healthy. The left lobe of the cerebellum was softened throughout, so that on section it was partly diffuent, and the usual striated appearance was gone. The tissue was of a dirty white colour. The greatest degree of softening was on the under surface, extending from before backwards in a line the breadth of the thumb. The right lobe was softened in a less degree, though on the under surface, a line the width of the finger from before backwards was diffuent. The right lobe was atrophied decidedly, the left slightly. The tentorium over the central part of each lobe was reddened and had lost its polish. No undue redness of the brain, tissues, nor any evidence of a clot, recent or old, could be found.

Continued Success of Case of Imperforate Rectum operated on Nine Months previously.—Dr. E. WALLACE stated that he was glad to report the uninterrupted good health of the infant upon whom he had operated by incision with the bistoury and subsequent slight dilatation with the little finger, in December of last year, and whose case he had described at some length to the College at the last January meeting.

The little patient had not manifested the slightest indisposition, having hitherto escaped colic and the ordinary disorders of the dentition period, as well as other diseases.

The stools, which were flattened and attenuated during the first six or eight weeks, had since that time presented the usual moulded form, and had been voided with ease and regularity.

Union of the Lid to the Ball of the Eye successfully treated by a New Method.—Dr. HAYS called the attention of the Fellows to a plan of treatment for the relief of symblepharon, which he had recently employed with satisfactory results.

He stated, it was well known that adhesion of the lid to the ball of the eye, resulting from the action of caustic substances on the conjunctiva, or from purulent inflammation of that tissue, was generally considered not to admit

of cure. Mr. Lawrence says¹ that "the inconvenience may be considered as irremediable." Dr. Mackenzie states² that "when the adhesion is very extensive, interference is useless." Demours asserts that he "has often seen the adhesions rather increased than diminished by the operations for their removal." The operations of Ammon and Dieffenbach are complicated and painful, and, as remarked by Dr. Mackenzie, "it may be doubted whether the deformity which must result from them would not counter-balance the gain from the cure of the symblepharon."

Dr. H. stated that he had tried, with great care and perseverance, to relieve a case of adhesion of the lids to the ball of the eye, the result of purulent ophthalmia, which presented itself in the Pennsylvania Eye and Ear Infirmary thirty-five years ago, but with such entire failure that he had since that time avoided operative procedures in all cases where the adhesions were extensive.

Seventeen years ago he succeeded in a case, in Wills Hospital, where the adhesion was limited, in affording relief, by dividing the uniting band, and then drawing the cut edges of the palpebral conjunctiva together by ligatures. These edges united readily, and the adhesion between the palpebral and ocular conjunctiva was not reproduced. (See *Lawrence's Treatise*, Philada. ed. 1854, p. 335.) Mr. Wilde has since resorted to the same method, but it is evidently only applicable to cases where the bands of adhesion are of limited extent.

In April last, however, a very interesting lad, from Washington, D. C., seventeen years of age, applied to him for relief from the condition in question. About eight years previously, when playing in a building then being erected, a portion of lime had fallen into the inner angle of his right eye, cauterizing it, and resulting in adhesion of the lid to the ball over a considerable space. Some months subsequently to the accident Dr. H. was consulted, but advised that no operation should then be attempted, but promised, when the lad became older, if a more successful mode of treatment than any then practised should be devised, to try to do something for the boy's relief. At intervals the patient had been brought to Dr. H. for treatment, but the same advice was reiterated.

When he applied in April last, with much anxiety for relief, Dr. H. gave the case very earnest consideration, and a plan of treatment presented itself which seemed to him likely to prove beneficial.

In reflecting on the impunity with which recent experiments had shown that metallic ligatures might be inserted into the tissues, it occurred to him that if, after dividing the adhesions, a metallic plate were introduced between the surfaces, reunion might be prevented without irritation of the eye resulting from the presence of the metallic substance. He therefore determined to try this method.

The upper lid was solidly adherent to the globe from the inner angle over two-thirds the extent of the cornea, towards the outer angle. The lower lid was adherent at its upper edge over the same extent, but below a probe could be passed some distance towards the inner angle, where the lid was adherent through the remainder of its extent. The motions of the eyeball were greatly restricted; the lids, at the outer angle, could be separated to only a small extent, and were closely bound together at the inner half. He decided to try the operation on the under lid first.

With the assistance of Dr. G. W. Norris, a probe was passed beneath the horizontal band at the margin of the lower lid, and, the lid being

¹ Treatise, Am. ed. 1854, p. 172.

² Pract. Treatise, 4th ed., p. 655.

drawn from the eyeball, the band was divided with a probe-pointed scissors. A band running perpendicular to this at its inner termination was then divided with the same scissors, and the liberation of the lid at the inner angle completed with a small scalpel.

A piece of tinfoil, cut of the proper shape, was then, with the aid of a flat probe, placed between the lower lid and ball of the eye, bent over the down lid, the lids closed, and strips of isinglass plaster applied so as to keep the lids closed and the foil in its place. The foil was removed the next day, and, as it did not appear to have produced any irritation, a new piece was introduced. This was repeated every day until the surfaces healed. The motions of the eyeball tended to displace the foil, and prevented its being kept as close to the inner angle of the eye as was desirable. Reunion consequently took place to a small extent at this point, but the other parts healed entirely in three weeks, without any adhesion between the opposite surfaces. The result was sufficiently satisfactory to encourage him to try to liberate the upper lid.

The tinfoil being somewhat difficult to insert in its place, from its great flexibility, he had a thin plate of silver moulded of the proper shape to fit over the inner half of the eye. With the aid of Dr. N., the upper lid was dissected from the ball, and also the lower lid at its inner angle, where it had reunited. The silver plate was then introduced, but it proved to be too small, and was displaced by the motions of the eye, so that it could not be maintained in its proper position. Had the plate been large enough to cover the whole anterior surface of the ball, it would, Dr. H. thought, have answered. A thin hemisphere of glass, shaped something like an artificial eye, would also, he thought, have answered. Not having either of these at hand, it was determined to employ the tinfoil again. A piece was cut of a somewhat triangular shape, one edge passed by means of a flat probe under the lower lid, the upper lid then drawn out, and the other edge passed with the same instrument under that lid. The lids were then drawn together, and secured as before by isinglass plaster. The foil was renewed daily, and except that there was a recurrence of the difficulty previously experienced of keeping the foil close in the angle, and which allowed of slight adhesion at that point, everything went on satisfactorily, and at the end of three weeks the divided surfaces had healed over, and the motions of the eyeball were sufficiently free. The patient could also open his eyelids to a much greater extent than previously; but the power of the levator palpebræ had been weakened by the long period of disuse, so that the lids could not be opened as wide as those of the other eye; it may be hoped, however, that, now the lid is free, the muscle, by exercise, will acquire strength.

Anemia from Prolonged Lactation, Softening of the Brain.—Dr. LEVICK remarked that he had recently lost a patient in the Pennsylvania Hospital, whose case was an unusual one, and had particularly interested him.

She was a married woman, thirty years of age, and the mother of three children, of whom the youngest was about four months old and the second about twenty months. The interval between the births of her last two children, therefore, was only sixteen months, and she had suckled the elder of the two until within three months of the birth of the younger; so that gestation and lactation had gone on together during six months. Being very poor, she was destitute of all suitable provision for her lying-in, and remained very feeble after her confinement. When admitted into the hospital she was very pallid, and complained of a buzzing in her ears, or, as she expressed it, of "a feeling as if there were a dashing of water in the head."

The *bruit du diable* could be distinctly heard along the jugular vein, and the heart's action was frequent and feeble; but no other symptoms of disease of any kind were manifested. Full diet, and pills of proto-carbonate of iron and sulphate of quinia, were prescribed. In the course of a few days after her admission, she began to complain of sick stomach in the morning, and believed herself to be again pregnant. The sickness occurred only in the morning; so that she was able to leave her bed in the latter part of the day, and was well enough to be walking in the hospital garden only the day before her death. She was suddenly seized with convulsions, which continued for twelve hours, until she died.

At the autopsy, minute tubercles, in their incipient stage and without any evidence of irritation near them, were found on the pleura and the mucous membrane of the intestines, the freedom from injurious effect upon these structures having been shown during life by the absence of cough and of diarrhœa. A remarkable thickening of the whole muscular coat of the large intestine was noticed, but no disease of any other structure presented, excepting an alteration in the substance of the brain.

This was found to be of a pale cream colour and *completely softened*, almost diffident in consistence. Not only the thalami optici and corpora striata but large portions of each hemisphere were involved in the ramollissement. There was no evidence of meningeal inflammation. No tubercles were discovered, although carefully sought for. Nor could any evidence be found of previous inflammation in the substance of the brain, notwithstanding a careful examination, in the mass and subsequently in detail with the microscope.

No compound granular corpuscles or other mark of cerebritis could be detected, although the healthy brain was everywhere broken down. The basilar artery was carefully searched for fibrinous clots, and its structure was inspected for osseous or other degeneration, but nothing of the kind existed.

The softening of the brain could not have been cadaveric, and must, in the absence of other local signs of alteration, be attributed to inanition—a want of proper nutrition, due to the condition of the blood produced by the combined influences of prolonged lactation, recurring pregnancy and labour, in a patient deprived of the usual comforts of the sick-room.

Dr. Levick regarded the case as a remarkable instance of great change of structure in the substance of the brain during life, without loss of motion or impairment of intellect; and he thought it worthy of note, also, as a striking illustration of the injurious effect of excessive lactation, especially under circumstances of health and position which must invariably aggravate the mischief.

Sept. 10. Maggots in the Meatus Auditorius removed by means of Tannic Acid in Glycerin.—Dr. R. P. THOMAS mentioned a case, recently under his care, of a young lady, from whose ear he had removed a large number of maggots. He said, while the lady was standing under a tree a small fly (probably the common maggot-fly) darted into her right ear, and remained there, in spite of all efforts at extrusion, for fifteen or twenty minutes. He presumed that it sought this cavity as a place of deposit for its eggs. Eight hours later a sensation of motion was experienced in the ear; at first faint, but afterwards more distinct, and increasing in violence until her powers of endurance were taxed to the utmost. Forty-six hours after the oviposit was made his attendance was requested. Her agony, from the motion and roaring sound in the ear, was indescribable. The external meatus proved to be unusually narrow and deep, and thereby rendered an

examination with the otoscope unsatisfactory. Having detected a tremulous jelly-like mass on the tympanic membrane, he attempted its removal with an ear-scoop, but failed. A resort was then made to injections of strong soapsuds, and in the course of a few minutes a maggot crawled out far enough to be visible, and thus to determine the nature of the case. Their removal by mechanical means, without injury to the delicate membrane, was impossible, and chemical solutions strong enough to affect their vitality were deemed too hazardous to the same structure. Sweet oil or almond oil will generally float out a bug or other small insect that may stray accidentally into the meatus; but their efficiency in removing parasites that have the power of attaching themselves to animal membranes having been deemed more questionable, he resolved, finally, upon trying a solution of twenty grains of tannin in an ounce of pure glycerin. The reason for its employment was twofold, viz: first, exclusion of the air from their breathing pores by the glycerin; and, second, the probability that the irritant effect of the tannin upon the delicate surfaces of the parasites would cause such a degree of activity as would bring them within reach of the forceps. The result confirmed the anticipation, and twenty-three maggots, each three-eighths of an inch in length, were removed by a slender forceps in the course of a few minutes.

The relief was immediate and complete; in two or three hours the cerebral excitement was allayed, and the following day the patient was entirely free of all uneasiness.

Several months have elapsed without the manifestation of any injury to the ear or the sense of hearing from the accident.

Oct. 3. Peritonitis with Disease of the Solitary and Agminated Glands in an Infant.—Dr. J. C. MORRIS exhibited a specimen of disease of the solitary and agminated glands, taken from a child eight and a half months of age who had died of peritonitis. He described the symptoms presented during life as follows:—

On last Monday week (Sept. 24) she was seized with a chill, followed by fever, with slight diarrhœa. There was some tenderness of the epigastrium, but no distension of the abdomen. The fever showed some tendency to regular remissions and exacerbations, so as to make me doubt whether I had a case of bilious fever, or of “infantile remittent” to deal with. The course of treatment pursued was the administration of mild laxatives and alkaline diaphoretics. The disease, however, did not yield, and very early on Friday morning she had a severe chill, followed by great distension of the belly and tenderness over the region of the liver. Calomel in small doses, and afterwards turpentine, were freely given, with wine whey, but she sank gradually, and expired during the next night, having vomited black grumous matter and thin blood shortly before.

Dr. PACKARD having made the autopsy, the following abstract of his account of it was then given by Dr. Morris:—

Post-mortem twelve hours after death.—Not much emaciation; no rigor mortis; hypostatic congestion. Lungs healthy, with some hypostatic congestion; heart soft; its tissue granular under the microscope. A large quantity of rather thin, healthy pus flowed out when the abdomen was opened. Omentum much congested. Patches of yellow lymph all over upper surface of liver and opposed portion of diaphragm, and over the spleen. Gall-bladder filled with dark, viscid bile. Stomach healthy, pale, except one or two slightly injected spots near the greater extremity: a patch of diffused redness in the duodenum. About eighteen inches from the commencement of

the jejunum, there was a patch of redness with several erosions. Peyer's patches seemed somewhat thickened, and stained deep grayish-brown. Here and there in the small intestines were patches of a yellow deposit. All along the large intestine the mucous membrane was studded with small dark round spots, very much like the deposit in the patches already mentioned; they corresponded to the solitary glands, and made the membrane resemble a leopard's skin. No other change in the large intestine.

Suspected Fracture of the Base of the Skull, attended with peculiar pathological phenomena.—Dr. R. P. THOMAS read the following report:—

Michael —, aged 27, was admitted into the surgical ward of the Episcopal Hospital, July 27th, 1860. While in a state of partial intoxication, he pitched head-long down one of the chutes on the Richmond coal wharves, a distance of eight or ten feet, and fell upon the right malar and temporal bones. He was taken immediately to the hospital. Upon examination the ramus of the right lower jaw was found broken about midway between the angle and the condyle. There was severe contusion of the right antero-lateral part of the head, but no fracture of the bone in that region, and but little laceration of the soft parts. Blood flowed freely from the nostrils and both ears. There had been vomiting prior to admission. The patient was almost unconscious; the face covered with a clammy sweat; the pulse 90, feeble; and the skin cold. Both pupils dilated, though unequally; the right eye was open, the left closed.

Diagnosis.—Concussion of the brain, with a probable fracture of the base of the skull towards the left side from the contrecoup. To sustain the sinking powers, small portions of milk punch were cautiously administered from time to time for the first few hours. Reaction took place; the pulse became fuller and stronger, and the skin warm, but the intellect remained obtuse. When the reaction was established, a mixture containing one-fourth of a grain of sulphate of morphia, and three grains of extract of conium was administered every four hours with a view of controlling the brain, and preventing the development of excitement, or delirium.

This plan, with beef tea as nourishment, was continued for three days, all alcoholic stimulus being avoided. At the expiration of the period named, the patient had the control of his sphincters, and could answer questions rationally, but could neither open nor close his eyes. The oozing of blood from the ears had nearly ceased, and the dilatation of the pupils was not so great. Their inequality, however, was still manifest. Improvement in all the symptoms occurred; the fracture of the lower jaw united, and at the end of six weeks he was permitted to walk about the ward. At the present time his appetite is excellent; he sleeps all night; is entirely free from pain; and his general condition is good.

During the progress of convalescence, some singular phenomena have been manifested, which afford a pathological confirmation of the correctness of the usual anatomical descriptions of the distribution of the nerves of the face and the eyeball.

The blow that fractured the ramus on the right side, paralyzed the portio dura nerve where it passed over the jaw, as evidenced by the drawing of the mouth to the left when he laughs, by the flat and expressionless aspect of the right cheek, and his inability to close the right eyelid, which is supplied by this nerve. The right eyelid never has been closed since the time of the accident. The injury to the portio dura must have been external, because the parts supplied by its branches, arising posterior to the ramus of the jaw, are not paralyzed. It is not probable that any internal injury involv-

ing the nerves of the right side has occurred, since they all perform their functions. The mobility of the tongue and of the eyeball on this side is perfect.

On the left side, the reverse phenomena are observed. The portio dura and the sixth nerve remain sound, while the third and fourth nerves are paralyzed. Here the injury is internal, but its outward manifestations are wonderfully distinct. I stated the patient could not close his right eyelid; I will now add he was unable to open the left. This latter fact involves two nerves, namely, the portio dura, which closes the lid by acting through the orbicularis muscle, and a branch of the third nerve which goes to the levator palpebræ muscle to open the lid by drawing it back. That branch fails in its duty, as do, also, the other branches of the third. For instance, if the patient be requested to look with the left eye upwards, inwards, or downwards, or upward and inward, or downward and outward, or to converge the axes of the two eyes, he cannot, or rather could not comply—the eyeball remained perfectly stationary; thus showing that both the third and the fourth nerves of the left side have been, and to a considerable extent still are paralyzed, since the former of these nerves supplies the superior, inferior, and internal straight muscles, and the inferior oblique, and the latter goes to the superior oblique.

If, on the other hand, he be requested to look at some object at a distance to the left, both eyes immediately converge upon it, thereby proving the left external straight muscle, and the sixth nerve which supplies it, to be perfectly sound.

The pupils were stated to have been widely and unequally dilated. At present they are nearly equal, though the left is still sluggish in its motions. The focal distance is widely different in the two eyes, being about five inches for the right, and nearly two feet for the left. Of course the patient sees two images, the outline of one being made more distinct than the other. He has difficulty in reading fine print.

There is dulness of hearing on both sides to a degree requiring the voice to be raised to a pitch above that of ordinary conversation. This is chiefly owing to the rupture of the tympanic membrane in each ear. On the right side, the anterior-inferior segment of the membrane is torn loose, and the handle of the malleus is directed outwards towards the auditory canal, instead of being inclined inward to the cavity of the tympanum. On the left, the superior segment is ruptured, the malleus remaining in position. There is now no discharge from either ear, a small pledget of cotton being kept in each, for the double purpose of protecting the cavity from atmospheric changes, and of serving in a slight degree as a substitute for the natural membrane.

The remaining cerebral nerves do not appear to have been injured, as no evidence of disturbed function exists.

During the past three weeks there has been a perceptible increase in the motions of the left eyeball and lid, and hence there is some probability of the functions of the third and fourth nerves being eventually restored. The right portio dura nerve seems to be permanently injured, as the muscles supplied by its temporo-facial branches do not perceptibly contract under the stimulus of a galvanic current.

Medical Note on the more familiar Flies; from Entomological Sources.
—Dr. B. H. COATES had been induced, by remarks made at a previous meeting in regard to the larvæ deposited by flies, which infest the sick room and hospital ward, to make some special references to entomological authori-

ties upon the subject, and read the following abstract as the result of his inquiries :—

It appears that the species of animals more or less nearly resembling the domestic fly, are so numerous that a recent writer has described no less than 1700 resident in Europe alone. The order is more frequently called Myidæ, than by any other name. Three groups of genera most commonly interfere with our comfort. The first, composed of Trachinia, with its allied genera, are most remarkably characterized by the rapidity of their flight; the second, Sarcophaga, and its allies, by sucking a variety of objects, and by a fondness for putrid flesh; and these include the most common domestic fly, Sarcophaga, formerly *Musca domestica*; and the third group, formed of Stomoxys and the genera which resemble it, by piercing the object which they suck, thus extracting, in many instances, blood from the human subject.

These classifications are formed on the study of the perfect insect, the well-developed parts of which admit of minute subdivision and discrimination; while nice distinction seems impossible with the larvæ, which much resemble each other. All of them undergo their changes within a shell formed out of their own external skin; and they produce no web.

The larvæ of the Trachinidæ pierce animal bodies, enter quite into their interior, and live there. The Sarcophagidæ are viviparous, the parent depositing no egg but living maggots. They are deposited on the surfaces of bodies, and never enter them. The parent, in general, prefers putrid matters for their nidus. The larvæ of the Stomoxydæ, in general, insert an appendix of the forepart of the body into the animal matter to which they adhere, and remain attached by it.

These notes are compiled from Mr. Westwood's *Introduction to Entomology*; and it was thought not improper to present them for the use of members of the College, in connection with the injuries inflicted by these animals on the human body.

Case of Fracture of the Thigh successfully treated by New Apparatus.—Dr. HENRY HARTSHORNE stated that he had, recently, through the kindness of Dr. R. P. Thomas, one of the surgeons to the Episcopal Hospital of this city, had an opportunity to make a full trial of the apparatus described and exhibited by him to the College about two years since;¹ the most essential novelty of which consisted in the principle of *supra-pelvic counter-extension*. The details of the case were, briefly, as follows:—

John Curry, a carter, aged 49, was admitted to the hospital, July 17, 1860, with a transverse fracture of the lower third of the femur. When the fragments were coaptated, there was, of course, no shortening; but displacement easily occurred, producing, on account of the muscularity of the patient, decided projection at the seat of fracture. The apparatus of Dr. Hartshorne, above alluded to, was applied the day after his admission. It is composed of two long outer splints, each of which is widened at its upper end sufficiently to allow of a bar or cross-piece being placed from one to the other *over the umbilicus* of the patient, who lies between the two splints. The *counter-extension* is made from this cross-bar. At their lower ends the splints are much narrower, and are fastened to a foot-board for the purpose of fixity and steadiness. Extension is made by means of a rotating cross-bar or windlass (between the splints, below the foot), to which *adhesive straps* connect the extremity. An attempt was made, during the first few days of the treatment of this case, to use adhesive plaster for coun-

¹ See Am. Journal of Med. Sciences, October, 1858, p. 395.

ter-extension. But, notwithstanding the utmost care, the straps became creased, twisted, and rolled into such shapes as to become very uncomfortable to the patient, and they were consequently abandoned. The perineum was then smoothly and accurately covered with pieces of *kid* spread with *soap-plaster*, and the common stuffed perineal band was applied. This answered the purpose *without discomfort* and *without excoriation* during the whole of the rest of the treatment. The *middle splint*, which forms a part of the original apparatus, was dispensed with in this case. Coaptation was maintained by the strips of Scultetus, and the limb was secured to the long splint upon its outside by a broad bandage; a junk-bag being interposed between the splint and the limb. A flat junk-bag or cushion was also placed under the injured limb, reaching from *just below* the tuberosity of the ischium to *just above* the heel.

Notwithstanding the prepossession of the patient against the apparatus, from his having heard that it was new, he soon became quite reconciled to it; and upon its removal, September 8, there was found to be absolutely *no shortening*, and *no deformity* whatever. The small amount of callus thrown out could only be detected by *palpation*.

The entire success of the treatment, even of this single case, is sufficient to demonstrate the availability of the plan of *supra-pelvic counter-extension*, which, so far as Dr. H. H. was aware, had been first applied in this apparatus. No objection or difficulty of any kind presented itself in the management of this case.

It was evident, however, that the apparatus could be simplified by making the counter-extending cross-bar *fixed* instead of revolving, and by modifying the foot-board into a simple joining piece for the splints.

In this form, or modified in any manner which may be suggested, he could not avoid the opinion that the principle of supra-pelvic counter-extension is deserving of more extended trial. It may be urged, at least, on its behalf—

1. That it is, *surgically*, more *correct* than the *oblique* counter-extension of Desault's apparatus, being *directly in the line of the limb*.

2. That, as the pressure of the counter-extending band is chiefly upon the *tuberosity of the ischium*, the tendency to the production of excoriation is much less than with oblique counter-extension.

3. That such an apparatus admits of greater *stability* and *security* than those ordinarily in use.

4. That in its simplest form it is not more difficult of construction, while it is decidedly more convenient in its application than Desault's apparatus, and requires less subsequent modification or arrangement.

Lastly, he repeated his conviction of the advantage (as compared with *adhesive plaster*) for counter-extension of the protective or *supplementary tegument of soap-plaster*, with the ordinary perineal band.

Employment of the Colpeurynter for Dilating the Os Uteri during Labour.—DR. KEATING read the following paper on this subject: I propose to lay before the College a novel employment of the colpeurynter, the use of which for obstetrical purposes was first suggested by Prof. Braun, of Vienna. Although this instrument has crept into practical use so as now to be of almost universal application, still it has not as yet been appreciated by many as thoroughly as it should be. The proposed application of the instrument is, I believe, entirely original with myself, and as the purposes which it subserved in the case I am about to relate were of the gravest character and the results most successful, I deem it just to suggest its

use under similar circumstances, as most likely to benefit a class of obstetrical cases for which, heretofore, we have had but the slenderest resources.

Mrs. P. became pregnant with her third child. Her first labour, I learn from other sources, was most severe and protracted, necessitating the prolonged application of the forceps, and the final extraction of a dead child. As the result of this severe labour, an extensive laceration occurred through the os uteri and the vagina; as a consequence, the os uteri and vagina were united in bands of cicatricial tissue. She again became pregnant, labour supervened at eight months, and at the expiration of twelve hours, a dead child was expelled naturally. Lacerations again occurred in the cicatrized portions of the uterus and vagina, and, as the result, such a complete occlusion and atresia followed in the vagina, that when first called to see her in consultation with Dr. Packard, in the month of June, 1859, the vagina was found only an inch and a half in length, excessively contracted in its circumference, and terminating in a cul-de-sac of cicatricial tissue in which there seemed to be a small opening of the size of a pin's head, through which the menses oozed guttatim. The latter seemed in fact to accumulate at times in a pouch behind the above-mentioned cul-de-sac. I was consulted on account of the irregular oozing of blood, the constant pain in the vagina, a severe dragging pain in the rectum during defecation and for some time after, and a continual nausea which so depressed and debilitated the patient as to incapacitate her for her ordinary domestic duties. We endeavoured by means of large metallic tubes of different sizes to dilate the vagina, and commenced at the same time with graduated cereoles to increase the small opening through which blood irregularly oozed. Finding that the vagina was exceedingly contracted and highly sensitive, we determined to allay its sensibility, and attempt gradual dilatations with the colpeurynter, at first expanded with air, afterwards with water. Some conception of the state of the parts may be formed when it is stated that even with the most gentle and gradual application of this unirritating instrument, the pain was so intense that I was forced to resort to anæsthesia. After repeated employment of the colpeurynter, I discovered that although the sensibility of the vagina was somewhat allayed, still no impression had been made upon the mass of abnormal tissue which formed a barrier between the ostium vaginae and os uteri. I then resorted to gradual dilatations of the small opening in the vagina by means of the most accurately graduated steel sounds; the opening was so small and so intimately connected with the tissues in direct contiguity with the bladder, that its dilatation was slow and painful. I soon found that there were other contractions and adhesions in the vagina, and in fine was obliged to cut and dilate four successive and distinct bands before I could reach that portion of the vagina in which I conceived the os uteri to be imbedded. Having reached the os, I found that it had also been dreadfully lacerated, and that the whole anterior lip had sloughed off, leaving a bevelled edge, which was continuous with the vagina, thus destroying entirely the anterior cul-de-sac formed by the mucous membrane of the vagina as it is reflected over the os and cervix. I now resorted to gradual dilatations of the opening leading into the uterus, and finally succeeded in introducing Simpson's sound into the organ, thus proving that this opening was really the mouth of the uterus. The canal of the vagina was then dilated by means of the colpeurynter, and the os uteri kept open by means of sponge tents.

Mrs. P.'s health now began to improve, her nausea ceased, her menstruation became regular, and she gave every indication of returning health. Some months after, her menstruation ceased, and she proved to be pregnant.

About the third month of pregnancy, a slight discharge of blood occurred, and we feared a miscarriage; by rest and appropriate means the threatenings ceased, but a per vaginam examination at this period revealed the distressing fact that under pregnancy the os uteri had entirely closed. There was, however, a slight dimple in the cicatricial tissue, in which we supposed the orifice of the womb to be placed, so minute, however, and so imbedded in inodular tissue, that it was impossible to predicate with any certainty its position. I examined Mrs. P. twice during her pregnancy, and found the condition of the os uteri unchanged. The child seemed active, and the abdominal tumour normal in size and position. I was summoned with Dr. Packard to see Mrs. P., on Wednesday, the 24th ult., at 9 P. M. Without the advent of any pain, the membranes had undoubtedly ruptured, and the liquor amnii was oozing away. According to her calculation she was six months and three weeks pregnant. Although the liquor amnii had been dribbling away for near two hours when we saw her, the form and position of the abdominal tumour remained unchanged; the fœtus was very active in its movements, and its heart was distinctly heard towards the left iliac region. A per vaginam examination revealed a perfectly occluded os uteri, but in its place a distinct indentation surrounded by a projection of dense cicatricial tissue; no part of the fœtus had engaged in the superior strait, and labour had evidently not as yet supervened.

The extensive lacerations from previous labours, the amount of cicatricial tissue surrounding and entering into the os uteri itself, its apparent occlusion with labour threatening, were circumstances which caused us great uneasiness in the case, and determined us to call in Prof. Meigs. This distinguished obstetrician coincided in our views and considered that although there was no positive certainty, still the probabilities were that the opening of the os uteri through which the liquor amnii oozed was in the indentation of abnormal tissue to which reference has already been made. Mrs. P. was in a nervous, irritable state, and to calm her we prescribed an enema of forty drops of laudanum. The next day the waters continued dribbling away, but the abdominal tumour did not seem to diminish, and the vitality of the fœtus was unimpaired. Mrs. P. remained much in the same condition until Friday, at 2 P. M., when regular uterine contractions set in. These pains evinced all the characters of true labour-pains, but after their regular continuation for four hours, it was found that they had made no impression upon the os. Prof. Meigs having gone out of town, Dr. Keller was called in consultation, and it was agreed that I should introduce the colpeurynter into the vagina, and thus assist in dilating the vagina and os, by at first gradually expanding it with air, and then with water. This instrument was immediately applied, and its use persevered in the whole afternoon and evening. From examinations which I made during the afternoon, I soon became convinced that the colpeurynter was assisting in dilating the os, and about 9 P. M. discovering with my finger a small opening in the mass of cicatricial tissue about the size of the head of a small tack, I immediately commenced the insertion of well graduated metallic sounds, and soon dilated the opening to such a degree that I could introduce the end of my little finger, and diagnosticate a cephalic presentation. The pains continuing frequent and forcible, I assisted the dilatations with my fingers.

Drs. Keller, Packard, and myself met at 10 P. M. We all agreed that the labour had made some progress. We now noticed, however, that the head could not engage fairly in the circle of the os uteri, but was caught in a sling-like projection of the posterior lip; the anterior lip, as before men-

tioned, having sloughed off after a previous labour, left the os bevelled off very much in the condition of a pig's snout. Although we endeavoured to keep down the resistance of this portion of the os uteri, and pressed it back during the accession of pain, still it was evident that owing to the resistance of the tissue of which it was composed, it did not yield. The labour seemed stationary. Mrs. P. having now been suffering from strong pains for at least eight hours, with the waters evacuated, it was apparent that unless some artificial assistance could be given to her, much difficulty would be experienced. In the first place, the labour would be a very painful and tedious one, owing to the condition of the os and surrounding tissues, the absence of a bag of waters to act as an entering wedge and assist the dilatations; and owing, also, to the bevelled condition of the os uteri preventing the head from engaging correctly. In the second place, the condensation of the uterus from active contractions in the absence of liquor amnii, must very soon tell upon a delicate foetus of six months and three weeks, and endanger its vitality. In fact, its movements were already becoming very infrequent and feeble, and I had not been able to hear the pulsations of the heart since 8 P. M. I was most anxious then to facilitate the dilatations of the os, and thus hasten the delivery of the foetus, and increase its chances of living. At 11 P. M. finding that the os remained about the size of half a dollar, I proposed to my colleagues to allow me to introduce the colpeurynter into the uterus, between the head of the child and os uteri, and gradually distend it with water, the idea being that in so doing I might, by keeping the colpeurynter distended in the absence of pain, prevent the complete condensation of the uterus, and thus protect the child, while at the same time during the contractions of the uterus, the colpeurynter being of a spherical shape might be pushed down in front of the head, and engaging within the os uteri, act as a more equal and certain dilator than the head, which, owing to the bevelled condition of the os, had a constant tendency to engage, with the occipito-frontal diameter instead of the occipito-bregmatic, and to push the posterior lip down before it, producing the sling-like condition just referred to, and resisting the descent of the child. This application of colpeurynter was immediately attended with the greatest success, the pains which had lulled and almost ceased, immediately revived, and I believe that I express the unanimous opinion of my colleagues in stating that the action of the instrument far exceeded our anticipations. During each contraction, it assumed a shape, condition and action so entirely analogous to the bag of waters that it could readily be mistaken for it. The dilatations of the os uteri rapidly progressed, and the movements of the foetus became again more frequent and active, so much so that this fact was noticed and dwelt upon by the patient herself.

About 12 P. M., we withdrew the colpeurynter; the os was then sufficiently dilated, but the descent of the foetus was retarded by a hand and foot having engaged with the head. Having caught hold of the presenting foot, Dr. Keller secured it with a fillet. I then performed version, and delivered the head in the first position of the vertex at 1.15 P. M.. The foetus at first gave but slight signs of life, but by means of heat and slight stimulation it gradually revived, cried, and was finally as vigorous as could be expected for six months and a half. It lived till the next day at 2 P. M. Placental delivery was natural, and the mother convalesced rapidly.

I believe that there can be no doubt that the rapidity of the delivery and safety of the child can be attributed to the introduction of the colpeurynter, and I suggest its similar application in all cases where the waters having

been evacuated early, the dilatations of the os are consequently slow and the labour prolonged.

It seems to me that under some circumstances an amendment might be made to this method, by first throwing into the womb a quantity of tepid water and preventing its escape by closing the os uteri with a colpeurynter distended with water or air after its introduction. In a case of partial placenta prævia presentation, which occurred to me some months since, I introduced the colpeurynter into the os uteri, and after distending it with water succeeded in arresting the hemorrhage, and in promoting rapid dilatations of the os uteri. It has occurred to me that in breech presentations it would also prove a most valuable auxiliary in causing full dilatations of the os uteri, previous to the descent of the breech or feet, and also in promoting the dilatations of the canal and perineum previous to the descent of the head, thereby producing a rapid delivery, and increasing the chances of the vitality of the fœtus. It has been customary with me, in the last two years, especially in cases of first labour where I discern the existence of great resistance from the soft parts and perineum, to prepare the canal for the descent of the fœtus by colpeurynter of the vagina and perineum, whilst the os uteri is dilating. I think that I have often in this manner shortened the labour by at least an hour without running any risks. On the contrary, by saving the individual so much pain, nervous excitement, and the engorgement of sensitive tissues by prolonged pressure, I have in many instances, and especially during particular seasons, prevented those morbid conditions which are apt to give us dangerous sequelæ. As a promoter of uterine contraction in lingering labours from a premature evacuation of the waters, I consider that colpeurynter is far superior and far safer than the administration of ergot, which is the great remedy of the timid and the ignorant. In all these applications of the colpeurynter I am in the habit of regulating its distension according to the feelings of the patient herself, commencing at first with air, and ending with water, although in many cases the air itself is sufficient. I enjoin upon the patient to increase or diminish the quantity according to her own feelings, thus placing it entirely under her command.

In conclusion, I beg leave to state, that this little, unpretending instrument is destined to revolutionize many of the operations of midwifery. Every day convinces me more and more of its extreme importance, and I shall take occasion before long to place before this learned body the cases, in detail, in which I have successfully applied it. In the present instance I have no hesitation in asserting that, 1st. By colpeurynter we assisted the dilatations of the os, so that we could introduce a sound and finally our fingers. 2d. It promoted uterine contractions when they were about to lull. 3d. by colpeurynter of the uterus itself we substituted an artificial for the natural bag of waters, thus facilitating dilatations, hastening delivery, and relieving the patient from a prolonged labour. 4th. By the continued presence and distension of the colpeurynter within the uterus, we prevented its complete condensation, protected the fœtus, and maintained its vitality. 5th. By colpeurynter of the vagina we prepared the canal and perineum for the descent of the fœtus, and thus hastened its delivery. 6th. As a final result, if there be any truth in the assertion of Simpson that the dangers for the parturient woman are in proportion to the duration of labour, by facilitating and shortening the delivery of our patient she was proportionally saved from those dangerous sequelæ so apt to occur after painful and tedious labours, especially where, as in this case, there had to be lacerations of old cicatrices connected with important organs.

ART. IX.—*Summary of the Proceedings of the Pathological Society of Philadelphia.*

1860. May 9. *Separation of the first from the second Bone of the Sternum, with Rupture of the Costal Cartilages.*—Dr. REED presented a specimen taken from a German labourer, who was brought in an insensible condition to the Pennsylvania Hospital, at one o'clock in the afternoon of the 28th of last April. Little information concerning him could be gained from those who brought him, except that he had been kicked by a mule in a drove-yard. He was examined by two of the attending surgeons. A stellar fracture of the outer plate of the frontal sinus was found, from which several pieces of bone were extracted. The conjunctiva of both eyes was intensely congested, but the pupils appeared normal, and contracted readily under the stimulus of light. There were several scalp wounds, and the right external ear was laid open. There was also a fracture of the right clavicle. The wounds of the scalp and external ear were closed by means of lead-wire sutures, and dressed with collodion and adhesive strips; and as the pulse was feeble, and the patient much prostrated, brandy and aromatic spirits of ammonia were ordered to be administered internally.

At 7 P. M. the patient was evidently losing ground; his belly was tympanitic, extremities cold, and his breathing had become exceedingly laboured. A second examination, made at this time, disclosed what seemed to be a fracture or dislocation of the first on the second bone of the sternum, together with rupture of several of the costal cartilages. No signs of this injury were apparent on the first examination, the apposition of the parts was perfect, and they did not yield under pressure, nor was there any peculiarity of respiration to lead us to a suspicion of anything amiss. When, however, the tympanites increased to such an extent as to interfere with the action of the diaphragm, the effort of respiration was entirely sustained by the thoracic walls, and displacement ensued. On each inspiration the separated portions of the sternum could be felt to grate upon each other. The right side expanded freely on inspiration, while the left appeared rather to be drawn in, giving the thorax a lop-sided appearance. The respiratory murmur was almost imperceptible on auscultation over the left lung. A turpentine injection was administered, which reduced the abdominal tension, and afforded a corresponding relief to the respiratory functions. His urine was drawn off by a catheter. The dislocation was reduced, and held in position by a straight splint placed over the sternum and fastened down by broad strips of adhesive plaster. The patient became exceedingly restless, requiring to be held in bed. He was ordered a tablespoonful of brandy, with half a drachm of aromatic spirits of ammonia every hour.

At 1 A. M., the patient having vomited, lime-water was given to him with his brandy. Some time after this he fell asleep, and when seen at 4 A. M. he seemed easier. He remained in about the same condition until 10 o'clock in the morning of the 29th, when he began to sink, and died at 5 P. M. of the same day.

Autopsy.—The first and second bones of the sternum were found to be separated from each other, and the cartilages of the second, third, fourth, and fifth ribs on the right side were fractured, so also were those of the

second, third, and fourth ribs of the left side. The pleural cavities were filled with blood. The lungs appeared healthy; that of the left side, however, being imperfectly expanded. The clavicle was fractured obliquely across its middle third. The brain was congested, the sinuses and vessels being distended with clotted blood. The spleen was ruptured, and was surrounded by masses of clotted blood. The heart, liver, kidneys, bladder, and other viscera, were healthy.

May 23. Anæmia from prolonged Lactation, causing Softening of the Brain, and Death.—Dr. LIVEZY communicated the following case: Ellen Fox, æt. 29; born in Ireland; married; admitted to the Pennsylvania Hospital, May 10, 1860, suffering from anæmia. She had had three children in rapid succession, there being an interval of but sixteen months between the births of the last two, the elder of which she had nursed until within two or three months of the birth of her last child, so that for several months gestation and lactation had been carried on at the same time. At the date of her admission she complained of general debility, and a thumping sensation in the left side of her head. An anæmic murmur could be heard very distinctly along the jugular veins. She was pale and emaciated, with a frequent but feeble pulse. She was ordered tonics, with good diet, under which treatment she appeared to improve. A few days after, she complained of sick stomach, occurring in the morning, and believed herself again pregnant. On the evening of the 18th inst. she said she felt better, and had been walking out in the yard. She slept well the fore part of that night, but about 3 o'clock the next morning she had a slight convulsion. When I saw her, she was better. I told her to remain in bed next day. She took food, but appeared inclined to sleep. In the evening she was about the same; had had no more convulsions. I was called to see her about 12 o'clock; she was then sensible, but suffering from spasmodic action of the muscles of the arms and forearms, the right being most affected, and when one was in motion the other was quiet. The pupils were natural. Ordered her an injection of mist. assafœtida, and to have mustard applied along the spine. After that she seemed better, and slept about two hours. In the morning she appeared sinking. She died at about 3 o'clock that afternoon.

Autopsy, twenty hours after death.—Rigor mortis well marked. Lungs congested, with minute tubercles scattered through them; there were some tubercular deposits also on the pleura. Heart very small, with concentric hypertrophy, and enlargement of the columnæ carneæ. Brain slightly congested, and very soft, particularly on the left side: Under the microscope, its minute structure was seen broken up. The large intestine was very much thickened, especially opposite the attachment of the mesocolon. The thickening consisted chiefly of hypertrophied connective tissue beneath the peritoneal coat of the bowel, and involving its fibrous coat.

Sept. 12. Medullary Sarcoma of the Uterus.—This specimen, together with the following report of the case, was presented, through Dr. Gross, by Dr. ROBT. BURNS, of Frankford.

Miss S. M.—, a tall, thin woman, æt. 46 years, born in Delaware County, Pa., began to menstruate at an early age, and continued regular until the change of life, about eighteen months ago. Her habits were active, and she had generally enjoyed good health. For seven or eight years previous to her death she was affected with what was denominated a cancer of the breast: This had been treated by escharotics, and remained

during her life an open ulcer, of inconsiderable size, the gland being entirely destroyed. She came under my care in August, 1859, at that time suffering from uterine hemorrhage, accompanied with pains, such as usually occur in cases of abortion. On examination, the os uteri was found circular and open; and the uterus enlarged so as to be easily felt above the os pubis by placing the left hand over the hypogastric region and raising the organ by the right finger. The discharge was considerable, and only temporarily relieved by remedies, always recurring in the course of two weeks and accompanied by uterine pains. On examination per vaginam, clots of blood were found within the dilated os uteri; on removing these, something more solid was found to occupy the neck of the uterus, to determine the nature of which I made a specular examination, in the presence of a physician of this city. On removing the coagulated blood, a tumour was seen protruding from the dilated os uteri. Taking this to be a medullary sarcoma, I removed as much as possible of it by ligature. This was followed by marked improvement; the hemorrhage ceased, the patient improved so as to walk about, the appetite was good, and sanguification went on rapidly. In the course of a few weeks, however, the hemorrhage returned with renewed violence, notwithstanding the constant use of astringent injections both into the vagina and cavity of the uterus. Into this last-named organ I injected a solution of creasote, hoping to make an impression upon the growth of the fungus, and destroy the fetor from decomposed blood; but, in consequence of its producing considerable pain, I desisted from its use. The hemorrhage and the expulsive pains were only relieved by breaking up with the fingers the presenting medullary mass. This gave immediate relief, the neck and os uteri contracting in a normal and healthy manner; but, as stated above, there was a repetition of a similar state in the course of two weeks. Consequently she became anæmic, and extremely exhausted, and gradually sank, notwithstanding every effort at support by stimulants, tonics, and nutritious diet. About three weeks before her death the hemorrhage ceased, and some peritoneal inflammation, accompanied by tympanites, set in. This was succeeded by cerebral disturbance, followed by coma, which continued until her death, July 30, 1860.

Post-mortem examination, thirty-six hours after death, Aug. 1, assisted by Dr. F.—The liver, omentum, intestines, and all the viscera, were very anæmic, the liver almost white, and very hard and dry. The uterus was about thrice its normal size. On the left side of the fundus there was a tumour of a somewhat fibrous character, about the size and shape of a pigeon's egg, with very limited attachment to the uterus. On the right side of the fundus was another, of the same size and shape, differing from the former in being congested with blood, soft, and easily detached. The ligaments, Fallopian tube, and ovarium on the left side, were normal; the right ovarium was absorbed. In contact with the fimbriated extremity of the Fallopian tube of the right side was a mass of medullary substance, similar in appearance to brain. This occupied the *cul-de-sac* on the right side of the uterus, and amounted in quantity to as much as might be grasped within the hand. On making a longitudinal section of the uterus, the greater portion of its cavity was found filled by a nodulated medullary mass. Not thinking of the preservation of the specimen at the time, this was detached with the scalpel, to ascertain, if possible, the source of the hemorrhage. Apparently this had come from many enlarged veins which ramified copiously throughout its substance. The incised walls of the uterus were firm, and, with the exception of being hypertrophied, were normal in

appearance. The neck and os uteri were free from disease; the latter was considerably dilated. The intestines in contact with the medullary mass above mentioned were discoloured, and of a gangrenous appearance, but no destruction of their walls was observable. An inflammatory blush extended from this point some eight or ten inches along the intestine. Several small polypi were found on different parts of the uterine appendages, and one upon the liver; the rest of the viscera presented nothing unusual. The thoracic cavity was not examined.

Cancer of the Liver.—Dr. CHAS. C. LEE communicated the following case: Richard Johnson, æt. 47, born in Denmark, was admitted to the Philadelphia Hospital, July 24, 1860, with well-marked hypertrophy of the liver. Though a thin man, and rather under the medium size, his abdomen measured thirty-four inches in circumference. According to the patient's account, the abdomen began to swell only five weeks before his admission, increasing rapidly in size and tension. He had never suffered from remittent fever or hepatic disease of any kind, nor could any hereditary taint be traced in his family. This story was thought improbable, but he repeated it several times, and was especially positive as to the date of his abdominal swelling. The outline of the liver was carefully traced, and its surface painted with strong tincture of iodine, and the patient was treated with mercurials and Lugol's solution, in combination with extract of taraxacum; notwithstanding which the liver steadily increased in size, no nodules being perceptible. On the 1st of August the patient's powers began to fail. Stimulants and tonics were exhibited, but he was so evidently sinking that no hopes were entertained for his recovery. He suffered no pain whatever, and became slightly jaundiced only one day before his death, which occurred on the night of the 18th of August, apparently from sheer exhaustion.

Autopsy, eighteen hours after death.—The body was greatly emaciated, and the rigor mortis but slightly marked. The heart was soft and very fatty, its right side filled with yellow fibrinous clots, firm, and intimately blended with the fleshy columns, apparently of *ante-mortem* formation. The lungs were both slightly œdematous, and showed evidences of an old pleuro-pneumonia on the right side, but contained no tubercles; scarcely any hydrothorax existed, but there were about two fluidounces of serum in the pericardium. The kidneys were normal in size and structure. The brain was not examined, as no cerebral symptoms had existed. Nearly three pints of limpid serous effusion were found within the peritoneum, but nothing abnormal in the stomach or intestines. The liver was enormously enlarged, completely covering the stomach, and extending deeply into the left hypochondriac region. It weighed fourteen pounds, and measured sixteen inches and a half across the under surface from right to left; the right lobe was twelve inches long and five inches thick. The proper hepatic structure was in great measure displaced by circumscribed deposits of medullary cancerous tissue of a yellowish-white colour, marbling the surface in a beautiful manner, and rising into nodules in every direction, but so soft as to be easily compressible, and imperceptible through the abdominal walls. The microscope revealed in this structure no fibrils, but numerous caudate and multiform cells, evidently of the cancerous type, intermingled with a few hepatic cells, the former greatly predominating in number.

Abscess of the Liver opening into the Right Lung.—Dr. MITCHELL, in presenting this specimen, gave the following account of the case: J. S.,

proof reader and agent, æt. 37 years, was born in Philadelphia, but had lived in the West many years. About two years ago he had a cough of several months' duration, and spit once a little blood. In October, 1859, Mr. S., then residing in Nashville, was attacked with general feebleness and depression of spirits, with pains in the bones, and frequent flushes of heat—the feelings which are usually described as “a cold.” During the fall and winter he lost flesh and colour. A trifling cough pursued him, and now and then a return of the general symptoms above described. There were no chills, malarial signs, or dysentery. About the beginning of March, Mr. S. was suddenly seized with fever, and violent pain in the nape of the neck and under the right shoulder-blade. After some domestic treatment, a physician was called in. He readily diagnosed the case as one of acute hepatitis, and treated it accordingly. Mr. S. was afoot in three weeks, but had a relapse owing to imprudence. In this second attack he had more pain in the region of the liver, and less in the back. He so far gained ground as to be able to travel to New York, which he reached about June 13th. On his way from that place to Connecticut, his hacking cough, which had been hitherto of trifling moment, increased suddenly, so that within twenty-four hours he expectorated at least three-fourths of a pint of pus. Mr. S. was astonished, but continued his journey, presuming that the new symptoms were due to bronchitis from sudden exposure. He went to the north, as I have said, still continuing to cast off from half a pint to a pint of pus daily. Returning southward, he reached Philadelphia on July 13th, 1860. On July 23d, Mr. S. sent for me. He told me his history, and especially insisted upon the fact of his constant exposure to the exhalations from a large drain and water-closets which were close to his office in Nashville. Every effort to abate the nuisance had failed, and the windows on the side towards the drain had finally been closed up. Nevertheless epidemics of dysentery and diarrhoea were of frequent occurrence among the hands. So great was the odour from the sources alluded to, that at least one person had left the office and a fair salary rather than endure the nuisance. These are the only important facts not already stated.

Mr. S. was spare and a little sallow, but not jaundiced. He was well able to go up and down stairs without aid. His tongue was clean, his appetite excellent, his digestive powers unimpaired, and there neither was nor had been any vomiting.

The cough was intermittent, being very violent, and attended with profuse expectoration of bloody pus for some hours, and then ceasing only to be renewed again within a day or even a less time. The abdomen was enlarged on the right side by the swollen liver which extended below the umbilicus and across the epigastric space. There was little or no rigidity of the right rectus muscle, but there was a very painful spot at the upper line of the right iliac fossa. Above the liver, dulness was continuous with a flat-sounding region of the lung. This involved a third of the lung in front, and curved upwards on the side and back so as to reach the scapular spine. At the lower point of the shoulder-blade there were the usual indications of a cavity. Moist rales were heard only in the right chest at first, but at a later period were also audible in the other lung, though to a less amount. We could detect no evidence of tubercle, but from the history of other cases we conjectured its existence.

The patient was treated with tonics and stimulants, and for a time pronounced well. On the 28th August, I left him in charge of Dr. Kane. He was then losing daily one to one and a half pints of mingled blood and

pus. He complained of the shoulder pain only when the abscesses filled up, and this pain was made easy upon his coughing freely. The iliac pain was intense towards the last, and was much relieved by the use of a bandage which Dr. Kane applied.

I have only to add that the pus was often putrid, and that it contained no bile. Mr. S. died from exhaustion on the 29th August, 1860.

When I first saw him, I diagnosed the existence of an abscess communicating with the lung. This abscess I supposed to involve the right lobe of the liver. Dr. J. F. Meigs, who saw him frequently, coincided in that view. The abscess first opened into the lung during his travel to Connecticut.

The following detail of the *post-mortem* conditions was furnished by Dr. Kane who examined the body, Drs. Meigs and Packard kindly assisting him.

Autopsy, twenty-nine hours after death.—The body, which had been kept in ice, was perfectly rigid and much emaciated. The anterior portion of the thorax was perfectly clear and resonant under percussion as low down as the superior margin of the sixth rib; from this point to nearly the level of the umbilicus both sides emitted a dull flat sound on being percussed.

The pericardium was perfectly healthy, and contained no more than the normal amount of fluid.

The heart was of the usual size, and showed no evidences of valvular disease, but its muscular fibres were much softened, and firm white clots were found in the auricles and ventricles of both sides, entangled in, and closely adherent to, the *cordæ tendineæ* and *columnæ carneæ*.

The inferior lobe of the left lung was studded with miliary tubercles. It was much congested, and of a deep red colour. The upper lobe, though congested, was less so than the lower, and contained no tubercles. The upper lobe of the right lung contained several small cretified tubercles, but was otherwise healthy. The lower lobe was completely riddled by an anfractuons vomica. What remained of its substance was much softened and of a dirty brown colour. The pleura around this portion was much thickened and immediately above, its two surfaces were closely adherent, thus forming a circumscribed empyema connected with the abscess of the lung.

The liver was almost double its normal size, its left lobe extending so far into the left hypochondriac region as to press strongly against the spleen. A rough measurement made before removing the organ from the abdomen, gave ten inches as the vertical diameter of the right lobe, nine inches as that of the left, and eleven inches as the transverse diameter of the entire viscus at its central portion. The upper left angle of the left lobe was the site of an abscess about as large as an ordinary hen's egg, which bulged outwards so as to press strongly against the diaphragm, and was filled with a thick, homogeneous, yellow pus. The right lobe was firmly adherent to the right wall of the abdomen and to the diaphragm, but especially to the right abdominal wall. An abscess as large as a Sicily orange occupied the upper portion of this lobe. It was filled with thick, whitish pus, and did not communicate either with the other abscesses in the liver or with the lung. A third abscess, nearly as large as a hen's egg, existed in the lower portion of the right lobe of the liver. This abscess communicated, by a large opening in its posterior wall, with the gall-bladder, which was firmly agglutinated to the liver, and was much distended with thick, greenish pus. The walls of the gall-bladder were much thickened. The cystic duct was

entirely occluded by the pressure of an enlarged gland. The hepatic duct was unimpeded, as was also the common duct, which we traced to its opening into the duodenum.

We had considerable difficulty in detecting the opening of communication between the liver and the lung, which was not as might have been expected above, from one of the large abscesses pressing against the concavity of the diaphragm, but by a small canalicular opening connected with the abscess in the lower portion of the right lobe which pierced the liver low down at about the junction of its right lateral and posterior surfaces, and allowed the pus to escape. This, being circumscribed by lymph, had burrowed upwards, and perforated the diaphragm at its attachment to the ribs anteriorly, thus opening into the circumscribed empyema in connection with the anterior surface of the right lung. There was considerable general peritonitis, especially in the ileo-cæcal region; but the stomach and intestines appeared healthy. The spleen was normal. The kidneys were enlarged and pale. The brain was not examined.

Dr. Morehouse said this case afforded an example of hepatic abscess occurring in association with tubercular deposit in the lungs. He thought such associations were not accidental, but illustrative of a relationship between suppurative inflammation of the liver and those systemic conditions favouring degeneration of tissue, and more prominently that denominated tubercle. Eighteen months ago he had exhibited to the Society a large abscess of the liver developed in a tubercular patient; since then he had learned the history of and observed a number of cases with especial reference to this point, and, from their evidence, was led to believe that these cases of hepatic abscess, coming on insidiously in persons from about forty to sixty years of age, are in the majority of instance associated with the tubercular diathesis. The history obtained from such cases is usually serious pulmonary disease in early life, cessation of cough and return to comparative health, persistence of dyspeptic symptoms for years, recently, disturbance of the bowels more or less severe, and lastly a new and fixed pain or tenderness in the right side, and it may be as the abscess develops and the health depreciates, recurrence of pulmonary trouble. Examinations after death generally exhibit lesions in some part of the abdominal mucous membrane, and these, no doubt, furnish the proximate cause of abscess, and its localization in the liver. Dr. M. did not wish to enter into the theory of production of these forms of hepatic abscess, but merely to call attention to their association with a dyscrasia, and to indicate the necessity for an early and correct interpretation of the oftentimes vague symptoms of hepatic irritation occurring in these cases, and when the diagnosis is established, the more guarded use of such active antiphlogistic measures as the treatment of hepatitis in a healthy person would justify.

Dr. Gross remarked that he was disinclined to believe that any special connection existed between the tubercular diathesis and suppurative hepatitis. He said that he had seen quite a number of cases of this disease among the boatmen on the Mississippi and Ohio Rivers, and that although he had seldom made post-mortem examinations in these cases, and never with a view to determining the presence or absence of tubercles in the lungs; yet the frequency of entire recovery, taken in connection with the fact of the patients being as a class strong, robust men, seemed to him to militate strongly against their being in any way connected with a constitutional taint.

Gangrene of the Lungs.—Dr. A. D. HALL exhibited specimens of this disease taken from a man fifty years of age, who came under notice Sept. 10th, 1860. The patient's occupation was that of a nurse, and he had been employed in a large hospital for several years. His habits were those of a confirmed spirit-drinker, and he was never entirely free from the influence of liquor during the past year. Two months previously to his final illness, he was seen by Dr. Hall for a surgical disease, and then presented no indications of a chest affection. When called to him, the sunken, and livid countenance, with a quick, feeble pulse, and cold clammy skin, held out but little hope; he continued to sink, and died in the course of twenty-four hours. During this period the symptoms were an incessant cough, with a puriform, exceedingly offensive expectoration, accompanied by a peculiar fetor of the breath. This was so great as to prevent auscultation and percussion of the patient's chest, and rendered it difficult to remain any length of time by the bedside.

On *post-mortem* examination a gangrenous cavity, nearly the size of a man's fist, was found in the anterior portion of the upper lobe of the right lung. The cavity communicated with the bronchial tubes, and emitted a fetid odour, similar to that of the breath and sputa during life. It was filled with a greenish mass of a sloughy character, together with a liquid apparently composed of the same material in a more fluid state. Isolated masses of crude tubercle occupied the apex of this lung. At the summit of the left lung there was found the depressed cicatrix of some former deposit. Beyond these alterations of tissue, the lungs were merely congested, and floated in water.

Remarks.—Gangrene of the lungs occurring during the course of pneumonia, pleuritis, especially where the constitution has been enfeebled by privation or disease, is not an affection of very great rarity. That it seldom occurs in connection with tubercular disease of the lung, may be inferred from the absence of reference to it by the more prominent authorities. In the elaborate work of Louis', no reference is made of it, and Walshe dismisses it in a few short and unsatisfactory sentences.

The great increase of fatty deposit, described by Rokitansky and Huss, as occurring in the bodies of old drunkards, held good in this case, in which the subcutaneous tissue, the exterior of the heart, the mesentery, and omentum were loaded with fat. The liver was not very markedly fatty, though rather large. The kidneys were somewhat enlarged, anæmic in appearance, with free fat deposited in their pelvis; but with the exception of a dense, fatty envelop, several inches in thickness, surrounding them, there was little else to remark. There was no approach to cirrhosis or granulated condition of either organ. More alteration of texture was to be expected from the well known and long continued habits of the patient.

The occurrence of fatty deposit in connection with tubercular disease is also worthy of notice; the supposition would naturally arise, that the wasting effect of tuberculosis should overbalance that of the fat producing influence of the alcohol in the economy. Another exception to the general rule, is mentioned by Handfield Jones, in which "a female, though dying of bronchitis with tubercular cavities in her lungs, was excessively fat."¹ The tubercular disease was also an exception in the rapidity of its progress.

¹ Brit. and For. Med.-Chir. Rev., July, 1853, p. 34.

In drunkards "tuberculosis runs an eminently chronic course."¹ But in this instance such was not the case. The disease advanced rapidly.

In all the cases published by Dr. Stokes, the patients were habitual drunkards, and the same is the fact with regard to those observed by Dr. Copland.

What influence a life of intemperance, with its train of fatty and fibroid degenerations, has in originating, or inducing a gangrenous action in some existing disease, would be an interesting subject for inquiry.

Severe Burn.—Dr. BRINTON exhibited to the Society a series of specimens removed from the body of a woman who had died recently at St. Joseph's Hospital. Dr. Brinton gave the following history of the case: About two weeks since the clothes of the person had taken fire, and almost the entire lower half of her body had been scorched. The eschar was superficial, involving only the integuments. When the patient was admitted into the wards, she was suffering from extreme prostration, but reacted readily under the administration of stimulants. She did not complain of much suffering, and appeared to progress favourably, gaining strength, until the ninth day after the accident. After this period diarrhœa set in, which resisted all treatment, and the woman died on the thirteenth day after the reception of the burn. During the continuance of this diarrhœa, but little pain was produced by pressure upon the abdomen. The respiration, however, during the last forty-eight hours of her life, was feeble and frequent.

Autopsy, fourteen hours after death. Abdominal Viscera.—The stomach was found to be inflamed, especially at its greater extremity; the duodenum was also inflamed, more particularly at its lower portion; the valvulæ conniventes here presenting a scarlet villous-like appearance. No ulcers, however, could be detected in the duodenum. The same inflammatory characteristics were observed at different points throughout the course of the jejunum and ileum. In this latter portion of the intestine, the inflammation was most marked in the neighbourhood of, and surrounding Peyer's patches. The glands themselves seemed slightly swollen, but were not inflamed. The large intestine was inflamed in the vicinity of the ilio-cæcal valve. The liver, when examined, was found to be fatty; the pancreas was normal, and the spleen was filled with blood, and was extremely friable. The right kidney was in the same condition, whilst the left was nearly healthy in appearance. The supra-renal capsules were not affected.

Thoracic Viscera.—The external surface of the heart was healthy; the mitral valves were inflamed, and the valves of the aorta were congested in the highest degree, presenting a beautiful vivid pink appearance. The same colouring was also found in the ascending part of the arch of the aorta. The coats of this vessel were studded over with atheromatous patches, as far as its bifurcation, at which point ossification was almost complete. The lungs were greatly congested with black blood, and the bronchial tubes were filled with frothy mucus.

Dr. HARTSHORNE remarked that it might often be questionable whether the internal congestion found in cases of external burns was due to the injury, or to the previous life and habits of the recipient, and cited Dr. Wilks as having expressed the opinion, that inflammation of the duodenum and jejunum is quite as frequently a coincidence as a consequence in these cases.

¹ Rokitansky, vol. i. p. 296.

Dr. Hoyt thought that there could be little room for doubt, that in the case reported by Dr. Brinton, the internal congestion had occurred after the external injury, since it was not until the tenth day after her admission to the hospital that any evidences of intestinal disturbance manifested themselves.

Dr. HARLAN presented a specimen of extra capsular fracture of the neck of the femur.

Diphtheria.—Dr. READ presented a larynx and trachea together with some portions of the larger bronchi, and read the following history:—

The patient from whom these specimens were taken, was a German tailor, aged 24 years, who walked into the hospital on the evening of Thursday. He stated that he had been suffering from sore throat ever since the Friday previous, and that although he had taken a variety of medicines, and had his throat frequently touched with a solution of nitrate of silver, he was getting no better, and had entirely lost his appetite. When I first saw him at 5½ P. M., he looked haggard and pale; his throat, which was externally œdematous, presented a peculiar tippet-shaped appearance, and his pulse was weak, small, and about 124. The tonsils and half arches were highly inflamed, pouring out pus and mucus mixed with blood from ulcerations on their surfaces. The uvula was extremely œdematous, and filled up the space between the tonsils. The tongue was parched and dark-brown. Though his breathing was accompanied by a marked bronchial stridor, there was no general dyspnoea, and his articulation, though indistinct, was perfectly intelligible. The patient was freely stimulated, and counter-irritants were applied to his throat externally.

At 9 P. M. he seemed easier, his pulse remaining unaltered. At this time an opiate was administered.

I saw him again half-past twelve o'clock, he was then sleeping in a recumbent posture, his skin was moist and respiration croupal; pulse increasing in rapidity, varying from 130 to 140. I ordered an increase of stimulus.

At 2 A. M. I was called to see the patient, who died just as I reached the ward. The body presented none of the appearances usually found when death is caused by asphyxia, nor were the symptoms during life other than those of intense prostration.

Autopsy, sixteen hours after death.—The heart, lungs, and abdominal viscera were healthy. The tonsils and half arches were the seats of excavated ulcerations. The upper portion of the larynx and the epiglottis were coated by a dense semi-organized fibrous membrane, resembling the exudation membrane of croup, which was continued downwards covering the vocal chords, and lining the trachea and bronchi even to the minute ramifications of the bronchial tubes. This exudation was closely adherent to the mucous surfaces, of which it formed perfect casts on being peeled off.

Tuberculosis.—Dr. PACKARD reported the following case: Dora Jackson, coloured, æt. about 11 months, had suffered from bronchitis last spring, but the principal symptom of her fatal illness was diarrhoea. As no physician had attended her, an autopsy was necessary in order to the giving of a certificate, and with Dr. Dunton's assistance I examined the body.

Thorax.—Right lung healthy, as was also the heart. The left pleura was universally adherent, and studded with deposits as big as a pin's head, evidently tuberculous. The upper lobe of the lung on this side was congested, but crepitant; its lower lobe was completely infiltrated with miliary tubercle, and at the posterior portion presented an abscess about large enough

to contain a chestnut, communicating with the pleura, and full of concrete yellow pus.

Abdomen.—*Liver* apparently healthy, except a few firm yellow deposits of very small size. *Gall-bladder* small, pale, and quite empty. *Spleen* studded on its outer surface and throughout its structure with miliary tubercles. *Kidneys* healthy; supra-renal capsules of normal size, but without grumous contents. *Mesenteric glands* a good deal enlarged.

Some small round spots of ulceration, about three in all, were observed in the patches of Peyer; and the colon bore traces of slight inflammation.

Empyema with Consecutive Pneumothorax without Perforation of the Pleura.—DR. WM. KELLER reported the following case: John Schneider, 10 months of age, had been affected for several weeks with diarrhœa, a hacking cough, and fever. When I saw him on the 19th of August, 1860, he was already greatly reduced in strength and flesh; his skin was freely perspiring, and his pulse between 130 and 140. The head had an unusually large parietal diameter, and the fontanelles were almost closed. The examination of the chest showed a dulness over the whole left side with entire absence of respiratory murmur. The right side was normal. The abdomen was swelled and tympanitic. I proposed to the mother, a healthy looking person, to wean the child, on the ground that her milk might be an exciting cause of the disease. This she willingly agreed to do, as her two older children had been very weak during lactation, and still showed a marked tendency towards rachitis. Having no hopes for the recovery of the little patient, I saw it every two or three days, without perceiving much change in the symptoms. The percussion which I never neglected to perform showed the same dulness over the entire left side as late as the 17th of September. On the 19th, a clear hollow sound was detected over its upper surface anteriorly. Respiration had nowhere returned, which I considered an unmistakable evidence of pneumothorax. On the right side the percussion was normal, and the respiratory murmur, though accompanied by mucous rales, was audible throughout. The spasmodic cough, which had previously been on the increase, had diminished during the last six days, though there was more difficulty in breathing. The appetite, which had been ravenous till now, had diminished, so that the child would scarcely eat anything. The diarrhœa increased from four to eight passages in twenty-four hours. The discharges were of a slimy appearance.

As the family were moving to another part of the city I did not see the child again until the 24th, when he was in a dying condition. The hollow sound was still perceptible on percussion over the upper portion of the left side. The cough had entirely ceased since two days.

The *post-mortem* examination was performed twenty-eight hours after death by Dr. Démme, who had once seen the case with me before pneumothorax had occurred. The body was very much emaciated. No communication from the outside could be perceived. In opening the left side of the chest a distinct hissing from the escape of the compressed air was heard. The heart was pressed towards the right side. The cavity of the left side of the chest was half empty, the other half contained about six ounces of thick white pus. The lung was pressed against the spinal column. The pleura seemed to be very much thickened, partly covered with a thin layer of exudation. The tissue of the lung was normal, but contained not a trace

of air. The upper lobe of the right lung was pale, and emphysematous at its edges. The two lower lobes were gorged with blood and serum, particularly at their posterior portions. The smaller bronchi of the right lung contained a thick catarrhal secretion. The heart, liver, spleen, kidneys, and bowels were extremely anæmic. The stomach contained a quantity of coagulated milk and liquid food, its fundus was changed to a gelatinous mass yielding to the least touch.

Cases of pneumothorax without perforation of the pleura are extremely rare, and are even doubted by many authors, so that in examining such a case some doubts might be felt about its real nature. Might there not have been an occult opening, or might not a previously existing opening have been recently closed; are questions to which I can only reply that a careful examination failed to detect traces of any opening whatever.

The literature on this subject is quite extensive. J. P. Frank and Laennec teach that pneumothorax may exist without perforation of the pleura. Prof. Rokitsky says, that in rare cases a certain quantity of air may be generated by a simple pleuritic inflammation. Dr. Williams, in the library of medicine, mentions two cases where the lungs were partially compressed by pseudo-membranes in such a manner that they could not expand during the resorption of the liquid exudation, so that the vacant space had to be filled by air. Dr. Graeves, an advocate of the formation of pneumothorax by the exhalation of air, argues, page 71, in the second volume of his *Clinical Lectures*: "Where there has been long-continued loss of blood from any cause the blood contains an unusual quantity of air, for nature, by absorbing air in such cases, makes an effort to keep the vascular system sufficiently full, and this air may be secreted into any part of the body."

There is no doubt that if, in the case above reported, the missing quantity of exudation was absorbed by the capillaries of the pleura (which is almost the only possible manner of accounting for its disappearance), the air which supplied its place had to be secreted by the same capillaries since an empty space cannot exist in the human system.

It is worthy of note, however, that the hydræmic blood of the patient probably contained less gas than that of a healthy person.

The relative capacity for the absorption of gases in normal blood and pure water, is—

	Blood.	Water.
Carbonic acid	150.	106.
Oxygen	11.	6.5
Nitrogen	6.5	4.2

From this it is evident that the absorbing power of the blood must diminish in direct proportion to the increase of water above the normal standard.

Rupture of the Aorta communicating with the Œsophagus.—This specimen, with the accompanying report of the case, was presented by Dr. C. S. BISHOP through Dr. WOODWARD.

Friday morning, September 7th, was called to see Mr. T. F., aged about fifty years, a native of England, a shoemaker, about 5½ feet in height, and tolerably fat, weighing probably 170 or 180 pounds, of the nervo-lymphatic temperament, with a constitutionally feeble pulse, of temperate and industrious habits. He complained of a severe pain in his chest, at a point corresponding to about the middle of the sternum. He told me that at about 11 o'clock on the previous morning he had taken a hearty lunch and drank

a glass of lager beer, immediately after swallowing which he was seized with the pain in question. He managed to get back to the shop, and was there taken with a "spasm" which lasted some time, and he was unable to get home until near midnight. When I saw him next morning, about 10 o'clock, he was sitting up. Being unable by auscultation and percussion or any rational signs to detect anything amiss in his chest, I ordered him some simple remedies to relieve the dyspepsia and constipation to which he was subject, combined with sufficient hyoscyamus to quiet the pain, and also directed a sinapism placed over the seat of uneasiness. I heard nothing more from him until the following Wednesday evening, when I met him in the street, and learned that he had been at work steadily since the day after I had seen him; but that the pain was quite bad at times, and he was never entirely free from it.

The next evening (Thursday, 13th) I was sent for. He had been at work until near 5 o'clock that afternoon, when he felt so much worse that he was obliged to go home. Found him in bed suffering very much from the same pain. Examined his chest, but could find nothing either in lungs or heart, or elsewhere, to throw any light on the case. I gave him an anodyne mixture, with a hop poultice to his chest, and as he had been costive for three days, directed a dose of cathartic pills containing an anodyne to be taken at bedtime. He rested passably, but with unpleasant dreams, and when I saw him at 11 o'clock next morning he was somewhat easier. He now described the sensation in his chest as "like two short pieces of bar iron with jagged ends which were being turned in a cavity too narrow for their length, and he could feel the ends tearing the sides of the cavity. Finding this to correspond in some degree with the heart's action, I examined that region carefully, but, owing to the feeble action of the organ and the thickness of the parietes, could discover nothing. He had had a small passage from his bowels which he described as being almost black; it had been removed before my call. About 5 o'clock P. M., word was sent me that he was "vomiting blood." Went to him immediately, and found a couple of cloths, into which he had been spitting, pretty well filled with dark clotted blood, but not more in quantity than we often see in epistaxis. The bleeding had apparently ceased, but he complained of nausea, which I presumed was from blood he had swallowed. As he complained of being thirsty, I directed him to have a teacupful of hot chamomile tea, which settled his stomach promptly. While the tea was preparing he vomited about half a teacupful of dark, clotted blood.

At 10 o'clock P. M., found him weak, but able to get out of bed; his mind was clear, and his skin cool (which it had been all along); his pulse was softer and more frequent than previously; the pain was not so severe, but extended more through to the back. He expressed the belief that if he could have a good night's rest he would be better. He slept comfortably for a couple of hours under the influence of morphia, but woke at about 12½ o'clock, and while complaining of a return of pain, was seized with a convulsion, and died in a few minutes.

The *autopsy* was made thirty-eight hours after death, with the assistance of Dr. Woodward. We found the lungs healthy. On severing the ascending vena cava for the purpose of removing the thoracic viscera, no blood escaped from it, being empty. The pericardium contained but little fluid. The heart was heavily loaded with fat, pale, and perhaps rather undersized. The aorta slightly aneurismal, and, as was afterwards shown by the microscope, somewhat affected with atheromatous degeneration. On examining

the cellular tissue between the aorta and œsophagus we noticed an ecchymosed and putrescent spot. On opening the aorta opposite this spot we detected at a point about two inches beyond the origin of the left subclavian artery, a dark orifice, a line or two in diameter. On applying a probe to it, we found it passed through the cellular tissue into the œsophagus. He had evidently bled to death from a rupture of the aorta into the œsophagus; and as he had not passed blood enough from either the mouth or by stool to produce a fatal result, it must have accumulated in the stomach and bowels, which were accordingly found to be filled with coagulated blood. The disease may have had its origin in the areolar tissue, and thence invaded the aorta and œsophagus, or an œsophageal ulcer may have given rise to the mischief; or, again, the rupture of a minute aneurism may have occurred at the origin of one of the œsophageal vessels, thus causing the pain felt at the time of the attack, and subsequently the effused blood may have followed the track of the vessel to the œsophagus. This last seems to me the most probable hypothesis, since the whole aorta was clearly in a diseased condition, whereas the areolar tissue and œsophagus appeared healthy.

Oct. 10. Epidemic Pleuro-Pneumonia.—Dr. WOODWARD exhibited a portion of healthy cow's lung, and a portion of the diseased lung of a cow killed in the advanced stage of epidemic pleuro-pneumonia. The morbid specimen had been obtained by Dr. J. N. Evans, of Hatborough, Montgomery County, and was brought before the Society by his permission. It presented appearances which closely resembled those of the human lung in the same stage of disease (red hepatization, verging on gray), except that on section it was seen to be intersected by bright yellow reticulations, which marked out irregular polygonal spaces. These were thus explained: The lobules of the cow's lung are quite distinct, and are separated from each other by loose areolar tissue. This interlobular tissue, infiltrated with yellow croupous lymph, constituted the yellow reticulations. In its minute anatomy, each lobule corresponded in all essential conditions with those of the human lung affected by pneumonia. Dr. Woodward mentioned that he had seen in both the healthy and the diseased lung the spores of fungi. These spores, however, could not be regarded as indicative of the etiology of the disease, as he had seen similar spores in the sputa of human patients labouring under various diseases, and in human lungs presenting the most dissimilar pathological conditions. It was not possible, in the present state of knowledge, to determine the species to which these spores belonged.

Dr. J. DARRACH said that he had examined the respiratory organs of a cow which had died at a more advanced period of the disease than the one from which the specimen just exhibited had been taken. The lungs were coloured yellow. The smaller bronchial tubes were filled with fibrinous casts, and the larger ones lined with tubular false membrane as high up as the tracheal bifurcation. The pleura also were coated with exudation. With regard to the fungi spores observed by Dr. Woodward, Dr. Darrach remarked that the growth of fungi in a fluid did not necessarily indicate the presence of sugar. He instanced the growth of the penicillum in urine in which no sugar could be demonstrated; but said that he was disposed to believe that there was a species of fermentation going on in this fluid at the time of the growth of the fungus, connected with the formation of lactic acid. He fully agreed with Dr. Woodward with regard to the difficulty of recognizing the various species of fungi in their spore state.

Dr. KELLER observed that contact with atmospheric air was a necessary

condition for the germination of these fungi, and that they were developed on the free surfaces of the smaller bronchial tubes, in the air-cells, or in cavities communicating with the bronchi.

Fibro-Plastic Tumours of the Ear-Lobes.—Dr. STILLÉ presented the specimens, and related the following particulars of the case:—

About four years ago a young lady, fourteen years of age, had her ears bored by a jeweller, who inserted in each orifice a very light gold ring. The wounds were somewhat inflamed, but did not require any special treatment. Within a year afterwards the posterior edge of each orifice began to be surrounded with a slight swelling. The rings were removed, and were not worn again. The swellings gradually increased, and during the last six months more rapidly than before, until they became as large as hazelnuts, and, being somewhat oval in shape, measured about three-quarters of an inch in their long diameter. They were attached to the lobes by necks of a quarter of an inch in thickness, and at the point of attachment the skin covering them was puckered. On the right side a small tumour, about the size of a coriander-seed, projected from the anterior face of the lobe. The larger tumours were covered with skin of a natural texture, but somewhat livid in colour. They were elastic to the touch, not painful, and but slightly sensitive when handled.

The tumours were removed by Drs. Hartshorne and Hewson, by making an incision around their pedicle, and traversing the lobe. On one of them being incised, the cut surface was found to be smooth and shining, of a pale pinkish colour, and fibrous bands were visible, diverging from its point of attachment to the circumference. The mass of the tumour was formed by an opaque elastic substance of an albuminous appearance.

Tumours like these have been described by Mr. Paget, who refers to several examples of them observed by himself, by Bruch, Venzetta, and Messrs. Holberton and Barrow.¹ Mr. Wilde has also reported a case of the sort,² and two are furnished by Mr. Hilton.³ Dr. Gross states that he has met with four cases of the affection, all of them occurring in female negroes.⁴ Dr. Norris has referred to four cases, three of which occurred in his own practice, and one of which he has figured.⁵ In these cases, also, the patients were negroes. Besides these, I am not aware that the tumours in question have been treated of by any writers, either on pathological anatomy or diseases of the ear, with the exception of Portal, who speaks of the remarkable thickness of the lobe in some females who wear very heavy ear-rings;⁶ and Elsaesser, who, writing in 1828, states that eighteen years before he had seen an encysted tumour as large as a walnut on the under surface of the ear-lobe of a scrofulous child ten years old, which was occasioned by boring the ear.⁷

I am told that one or two other cases have occurred in this city, but have not learned the particulars of them.

Dr. Gross said that these cicatricial tumours of the ear-lobes bore a close resemblance to keloid tumours. He regarded them as belonging to the class of simple recurrent fibroid tumours. They are always, he said, the result of an injury, and their tendency is always to return.

¹ Surgical Pathology, Am. ed., p. 408.

² Toynbee, On the Ear, p. 23.

³ Lancet, March, 1860, p. 294.

⁴ Surgery, ii. 431.

⁵ Trans. of Coll. of Physicians, July, 1850, p. 93.

⁶ Anat. Med., iv. 451.

⁷ Hufeland's Journ., lxxvii. 99.

Dr. HARRIS drew attention to the fact that the tegumentary covering of these tumours appeared to grow *pari passu* with the mass, and was always perfectly loose and flaccid, and said that he had seen two cases similar to the ones described by Dr. Stillé. In one of these the tumour of the ear-lobe was so large that the patient had to support it by a bag. A chain of similar tumours extended down the back. The second case, which was one of those mentioned by Dr. Stillé, fell under his observation in the Pennsylvania Hospital, where Dr. Norris removed a mass as large as a hen's egg from the ear-lobe of a negroess.

Dr. E. HARTSHORNE remarked, with regard to the second case spoken of by Dr. Harris, that the tumour had returned, and was again seen by Dr. Norris, in company with himself, after a lapse of five years.

Oct. 24. Method of Replacing the Skullcap after Post-mortem Examination so as to prevent Disfigurement.—Dr. PACKARD read the following communication :—

It is often difficult, after making a post-mortem examination of the brain, to replace the skullcap so that no disfigurement of the forehead or face shall ensue. The usual plan of sawing the cranium so as to make the incision form an angle in the median line before and behind, a salient angle in the lower part of the skull corresponding with an entering angle in the calvarium, is often ineffectual; the bony cap slipping backwards so as to cause a most hideous ridge across the forehead. And if, to prevent this, we cut away a transverse strip of the scalp, the skin of the face is apt to be so put on the stretch as to distort the features unpleasantly. Wiring the divided bones together, besides being troublesome, and requiring great accuracy, does not afford the necessary degree of firmness.

A very simple means of accomplishing the object is represented in the annexed wood-cut. A punch, made of any convenient metal, is employed



to drive one or more double-pointed pins into the diploic structure at suitable points, as for instance just above the external angular processes of the frontal bone; the calvarium is then fitted on, and forced down so that the points of the pins which are sticking up become engaged in its diploë. The central cylindrical portion of the pin may be roughened with a file in order to increase the strength of its hold. If necessary, a way may be prepared for the pin by boring the diploë with an awl.

This plan, which I have found perfectly satisfactory, is very inexpensive, the punch with a dozen pins costing, according to the estimate of Mr. Gemrig, of this city, about one dollar. Many an autopsy is objected to, to the disadvantage of science, because in some former case there was left a disfigurement of the body; and I trust that the simple contrivance I have mentioned may aid in obviating such difficulties.

FISKE FUND PRIZE ESSAY.

ART. X.—*Diphtheria ; its Nature and Treatment, with an Account of the History of its Prevalence in various Countries.* By DANIEL DENISON SLADE, M. D., of Boston, Mass. The Dissertation to which the Fiske Fund Prize was awarded, July 11, 1860.¹ (Published by request of the Rhode Island Medical Society.)

No diseases of late years have awakened more attention both among the profession and the public, generally, than those which have been classed more or less correctly under the term diphtheritic. Nor is this to be wondered at when we consider the distressing nature of the symptoms, and the terrible fatality with which the epidemics of malignant sore throat have so generally been attended.

Diphtheria is a synonyme of the word Diphtherite,² originally used by M. Bretonneau in his treatise on this subject, which appeared in 1826, and which is chiefly made up of his own observations on the epidemics of malignant sore throat which prevailed at Tours and in its neighborhood in 1818, and again in 1825 and 1826.

The following are the specific characters of diphtherite, according to M. Bretonneau :—³

“At the beginning of the disease there is perceived a circumscribed redness, which is covered with semi-transparent coagulated mucus. This first layer, thin, supple and porous, may be still elevated by portions of unaltered mucous membrane in such a manner as to form vesicles. Often in a few hours the red patches visibly extend step by step, through continuity or contact in the manner of a liquid which is poured out on a plane surface, or which runs by striæ into one channel. The concretion becomes opaque, white, and thick ; it assumes a membranous consistence. At this period it is easily detached, and adheres to the mucous membrane only by very delicate prolongations of a concrete matter, which penetrates into the muciparous follicles. The surface which it covers is usually of a slightly red tint, dotted with a deeper red ; this tint is more vivid at the periphery of the patches. If the false membrane be detached and leave exposed the mucous surface, the redness which appeared subdued under the concretion reappears, blood transudes through the deep red points, the concre-

¹ The Trustees of the Fiske Fund, at the annual meeting of the Rhode Island Medical Society, held in Newport, July 11, 1860, announced that the premium of one hundred dollars offered by them on the subject of “Diphtheria, its nature and treatment, with an account of the history of its prevalence in different countries,” had been awarded to the author of the dissertation bearing the motto—

“*Felix qui potuit rerum cognoscere causas.*”

And upon breaking the seal of the accompanying packet, they learned that the successful competitor was Daniel Denison Slade, M. D., of Boston, Mass.

JAMES H. ELDRIDGE, M. D., East Greenwich,

CHARLES W. PARSONS, M. D., Providence,

HENRY E. TURNER, M. D., Newport,

Trustees.

S. AUG. ARNOLD, M. D., Providence, *Secretary of the Fiske Fund.*

² Διφθέρια and Διφθερίτις have both the same signification, the prepared skin of an animal Διφθερίτης and Διφθερίας both signify that which is covered with skin.

³ *Traité de la Diphthérie*, Paris, 1826, p. 49.

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tion reappears, and becomes more and more adherent upon the points first invaded; it often acquires a thickness of several lines, and passes from a yellowish white to a grayish and to a black colour. At the same time the blood transudes with more facility, and constitutes those *stilticidia* which have been generally remarked by authors.

"Now, the alteration of the organic surfaces is more apparent than at the beginning; often portions of the concrete matter are effused into the substance itself of the mucous membrane; there is observed also a slight erosion, and sometimes ecchymosis in points, which by their situation are exposed to friction, or from which the avulsion of the false membranes has been attempted. It is especially about this time that the concretions which have become putrid give out infectious odour. If the concretions are circumscribed, the œdematous swelling of the cellular tissue immediately around makes the former appear depressed, and, judging from this appearance only, we might be tempted to believe that we had under observation a foul ulcer with considerable loss of substance.

"If, on the contrary, they are extended over considerable surface, they become partially detached, and hang in shreds more or less putrefied, and simulate the last stage of sphacelus; but when we open the body of those who, several days sick, have succumbed to tracheal diphtheritis, we shall find in the air-passages all the shades of this inflammation from its first degree, as shown in the portions just invaded, up to that which has by its deceptive appearance led us for a moment to dread the supervention of gangrene."

Diphtheria, according to M. Bretonneau, is a specific disease. Its specific character consists anatomically in the formation of a false membrane of definite structure—pathologically, that it has the power of reproducing itself. "Nothing is diphtheria that has not a pellicular exudation, no such exudation is diphtherical which is not capable of acting as a virus or contagion."

These were the views expressed in the treatise of M. Bretonneau, and to these he still adheres, with some modifications, in a paper published in the *Archives Générales de Médecine*, 1855.

Under the term diphtheria, Bretonneau, however, has connected several affections, which in the prevailing nosology are separated from each other by wide intervals. This point will demand of us especial consideration.

How far his description of diphtheria is to be considered a faithful representation, how far it is to be taken as a universal type of the disease, are questions to be answered only by a careful comparison of the accounts of the epidemics of "sore throat" which have invaded various portions of the world, at longer and shorter intervals, particularly during the last two centuries. On making such comparison it will be found that they exhibit marked differences in their characteristic symptoms and dangers, having been frequently regarded as different diseases. We shall, however, not only be satisfied of their identity, a fact so well established by Bretonneau, but also of the common character by which this identity may be recognized, viz., the existence of the exudation of false membrane.

As regards the special virulence of the diphtheritic exudation which constitutes an important feature in M. Bretonneau's views of diphtheria, it will be seen as we proceed, that so far from inoculation being the only mode of propagation, there is not sufficient reason to suppose that there is a concrete virus; that epidemics of a rapidly fatal character have occurred, where the exudation has been extremely limited and where death has been brought about solely by the constitutional disturbance.

History.—There is reason to suppose that we can trace back the history of this affection to a period almost cotemporary with Homer. Whether such be the case or not, certain it is that ten centuries after, we find distinct descriptions of a form of malignant sore throat in the writings of Aretæus,

under the name of Egyptian or Syrian ulcer. This prevailed in the two countries, more especially among children. It was characterized by the appearance of ulceration in the throat, by fetid breath, and sometimes by great dyspnoea.

Macrobius speaks of a similar epidemic at Rome, A.D. 380, at which time sacrifices were offered up to a certain Goddess—"ut populus Romanus, morbo, qui angina dicitur, promisso voto, sit liberatus."

A fatal epidemic of sore throat occurred in Holland, in 1337. Hecker,¹ in his account of the "Sweating sickness" of England, in 1517, says that—

"In January of that year, there appeared in Holland another disease which, from its dangerous and inexplicable symptoms, spread fear and horror around. It was a malignant and infectious inflammation of the throat, so rapid in its course that unless assistance was procured within eight hours, the patient was past all hope of recovery before the close of the day. Sudden pains in the throat, and violent oppression of the chest, especially in the region of the heart, threatened suffocation, and at length actually produced it. During the paroxysms the muscles of the throat and chest were seized with violent spasms, and there were but short intervals of alleviation, before a repetition of such seizures terminated in death. Unattended by any premonitory symptoms, the disease began with a severe catarrhal affection of the chest, which speedily advanced to inflammation of the air-passages.

"The physicians had recourse to venesection and purgatives. Moreover, the employment of detergent gargles, whereby the extension of the affection to the lungs was prevented, as also of demulcent pectoral remedies, was decidedly beneficial. . . . Most of those affected were taken ill at the same time; and eleven days of suffering and misery had scarcely elapsed, when not another case occurred. It spread, however, no doubt, beyond Holland, for in the same year we find it in Basle, where, within eight months, it destroyed about 2000 people, and its symptoms would seem to have been more strongly marked.

"Respecting the intermediate countries, which it is highly probable that the disease passed through from Holland before it reached Basle, we unfortunately have no information. The tongue and gullet were white, as if covered with mould; the patient had an aversion to food and drink, and suffered from malignant fever, accompanied with continued headache and delirium. Here, in addition to an internal method of cure not detailed, the cleansing of the mouth was perceived to be an essential part of the treatment; the viscous white coating was removed every two hours, and the tongue and fauces were afterwards smeared with honey of roses."

Again, in 1557, a similar epidemic occurred in Holland, which proved very fatal, and which spread to other parts of Europe. It has been described by M. Forest. He says:—

"It was not so rapid in its course as in 1517, but began with a slight fever, like a common catarrh, and showed its great malignity only by degrees. Sudden fits of suffocation then came on, and the pain of the chest was so distressing that the patients imagined that they must die in the paroxysm. The complaint was increased still more by a tight convulsive cough. Death did not take place till the ninth or fourteenth day. The painful affection of the stomach was, in this epidemic, very distinctly marked, so that a sense of pressure at the precordia, accompanied by continued acid eructations, continued to exist even after a succession of six or seven fits of fever; and convalescents were troubled with dyspepsia, debility, and hypochondriasis."

In 1576, there was a very malignant form of the disease prevalent in Paris. In fact, from the end of the 16th century, we find that epidemics of angina have shown themselves almost constantly to a greater or less extent, in some portions of the old or new world. In the beginning of

¹ Hecker's Epidemics, p. 224.

the 17th century, an epidemic of angina occurred in Spain, which received the name of "Garotillo," because those who were attacked by it, perished as if strangled by a cord. This has been described by Mercatus, Villareal, Nunez, and by others. In 1618, the same disease appeared at Naples, which the inhabitants termed "male de canna," disease of the trachea. It raged here to a greater or less extent for twenty years, and has been described by several writers, among whom we may mention Nola, Carnevale, Symbati, Zacutus Lusitanus, and Marcus Aurelius Severinus. Carnevale, in particular, has given us full data of this epidemic in his treatise entitled, "*De Epidemico Strangulatione Affectu.*" The children were first attacked, the disease afterwards spreading among the population generally, and proving very fatal. The disease commenced by a mild inflammation of the throat; soon the affected parts presented a whitish exudation; the breath became fetid; deglutition impossible; the respiration embarrassed, and the patient died of suffocation. This writer also gives us the different appearances which the pharynx presented in this epidemic; he also speaks of the extension of the disease to the trachea, œsophagus, pituitary membrane—of the diagnosis, prognosis, and the topical remedies, all of which are quite in accordance with modern views.

In 1632, Alaymus published a treatise upon "Syrian Ulcers." He prefers this term, he says, inasmuch as it applies to all forms of the disease, which he describes in a similar manner with Carnevale. No writer of this age, however, speaks of cutaneous diphtheria in connection with the other symptoms which they describe, although most of them particularly notice the extension of the disease to the air-passages.

From the middle of the seventeenth century up to 1740, we find but little mention made of the prevalence of malignant angina. But very shortly after this, in 1743, the disease made its appearance in Paris, where it prevailed until 1748, and has been described by Malouin and Chomel. At about the same time a similar epidemic appeared both in England and at Cremona, accounts of which are recorded by Fothergill, Starr, and Ghisi.

In England these epidemics proved very destructive. The epidemic described by Fothergill is, without doubt, closely allied with scarlatina. He says:—

It generally comes on with giddiness and chills, which are soon followed by great heat; these states alternate for a few hours, until, at length, the heat becomes constant and intense. Then follows acute pain in the head, heat and soreness, rather than pain of throat, stiffness of the neck, commonly great sickness or purging, or the two combined. The face soon after looks red and swelled, the eyes inflamed and watery, as in measles, restlessness, anxiety and faintness. If the mouth and throat be examined soon after the first attack, the uvula and tonsils are found swelled; and these parts together with the velum palati and pharynx appear of a florid red colour, which is most marked on posterior edge of palate in the angles above the tonsils, and upon the tonsils themselves. Instead of redness, a broad spot or patch of an irregular form and of pale white, is sometimes seen surrounded with florid red, which whiteness appears like that of the gums immediately after being pressed with the fingers, or as if matter ready to be discharged was contained beneath. Generally on the second day of the disease, the face, neck, breast and hands, are of a deep erysipelatous colour, with a sensible tumefaction. A great number of small pimples of a colour more intense than that which surrounds them appear on the arms and other parts. (In a note, he says, the eruption and redness have not so regularly accompanied the disease during the latter part of this winter, 1754, as they did last year. In some cases they did not appear at all, in others not till the third or fourth day).

The appearances in the fauces continue the same, except that the white

places become more ash-coloured; and it is now found that what might be taken for the superficial covering of a suppurated tumour is really a slough concealing an ulcer. Instead of the slough in mild cases, a superficial ulcer of an irregular form appears in one or more parts, scarce to be distinguished from the sound, but by the irregularity of surface which it occasions. Towards night, heat and restlessness increase, and a peculiar kind of delirium frequently comes on. The pulse is generally very quick; in some, hard and small; in some, soft and full. The tongue is generally moist, and not often found. In some it is covered with a thick white fur; and these generally complain of soreness about the root of the tongue.

Fothergill also speaks of an acrid discharge from the nose, and remarks that there is sometimes epistaxis at the commencement of the attack. He describes faintness as a common symptom, also diarrhoea at the outset. He is very positive about the separation of sloughs which leave ulcers. Although he does not mention dropsy as a sequela, yet he evidently had entertained the notion of the disease being allied to scarlatina; but, he remarks, it differs from the sore throat and scarlet fever described in Edinburgh in 1733.

Ghisi, after having given a detailed description of the epidemic of sore throat which commenced to show itself at Cremona in 1747, remarks that the disease proved fatal by suffocation, even in those cases where the attention of the patient had not been called to the condition of the throat. This absence of all difficulty in deglutition has been constantly observed, however, according to M. Bretonneau, in all the epidemics of malignant angina, particularly in those of Tours. Ghisi describes cases which appear to be primary and not secondary to scarlatina. He especially indicates the peculiarity of the pseudo-membranous concretion which lines the air-passages.

In 1747, M. Arnault, of Orleans, mentions cases of malignant sore throat which carried off the patient in twenty-four hours. At the autopsies of two children dead from this disease the mucous membrane of the trachea was found detached for the extent of several inches. It was of the consistence of parchment, and of a white colour.

In 1768, Marteau de Grandvilliers published descriptions of cases of gangrenous angina, which he had observed for many years in Picardy. These observations, according to M. Bretonneau, would have contributed essentially towards a right understanding of the several mooted points, had not the writer confounded scarlatinal with diphtheritic angina.

Huxham¹ describes an epidemic in 1757, prevailing in England, which was also closely allied with scarlatina. He says:—

“Most commonly the angina came on before the exanthem, but many times the eruption appeared before the sore throat, and was sometimes very considerable, though there was little or no pain in the fauces; on the contrary, a very severe angina seized some patients that had no manner of eruption; and yet even in these cases a very great itching and desquamation sometimes ensued, but this was chiefly in grown persons, very rarely in children.”

The eruption was sometimes pustular, sometimes erysipelatous. He alludes to some cases in which there were signs of croup, but the symptoms were not well marked; the peculiar cry and suffocation were wanting.

He remarks that “in all sorts of fevers about this time there was a surprising disposition to eruptions of some kind or other, to soreness of throat, and apnoea.”

In Huxham's description of the epidemic, his attention was chiefly directed to the condition of the fauces, and he does not at all seem to

¹ Huxham, Dissertation on the Malignant Ulcerous Sore Throat. London, 1757.

appreciate the tendency of the disease to extend to the air-passages. Yet, by his own statements, some of his cases must have terminated by laryngeal symptoms.

"Not only," says he, "were the nostrils, fauces, &c., affected, but the windpipe itself was sometimes much corroded, and pieces of its internal membrane were spit up."

Dr. Starr, of Liskeard, published a paper in the *Philosophical Transactions*, upon the malignant ulcerous sore throat epidemic which appeared in that place in 1749. In this paper, besides other details of the epidemic, he gives the full data of a case in which the false membrane, commencing in the fauces, extended to the larynx. He particularly dwells upon the physical properties of the exudation, its adherence to the subjacent surface, its frequent detachment and reproduction. In fact, he gives a complete picture of Bretonneau's diphtheria.

In 1761, Rosen gives an account of an epidemic which prevailed in Sweden. Dr. Samuel Bard¹ published a dissertation upon the nature, causes, and treatment of suffocative angina, as it appeared in New York in 1771. To this writer we shall have occasion to revert more particularly when we come to trace the history of diphtheria in our own country.

From this period the disease and the writings which the subject had called forth, seem to have been laid aside, and almost lost sight of, when, in 1826, the treatise of Bretonneau made its appearance. This we may truly consider to have been the first connected and practical research upon the nature of the affection. Since this, the disease has become firmly established in France, and it would seem, judging from the experience of the last few years, that it has also prevailed, to a greater or less extent, in England and in our own land. It has been described by many French writers, among whom we may mention Guersant, Isambert, Chomel, Andral, Rilliet, Barthez, Trousseau, and Bouchut.

It is by a careful study of the most characteristic and important researches into these epidemics that we are enabled to gain at least a partial insight into the nature of the disease, and to contrast the present with the earlier accounts of its character.

The epidemic at Tours, in 1818—1821, so vividly described by Bretonneau, first broke out in the barracks amongst the soldiers, and thence spread to the surrounding quarters. Among the military the gums were the portions most frequently attacked, the air-passages were rarely affected. As it spread into the city the larynx, however, was the portion which the disease selected, while the gums were preserved; children being, in most cases, the victims.

Those who were thus attacked rarely complained much at the outset of the attack, although deglutition was slightly interfered with. On examination of the throat it was found to be somewhat inflamed; shortly a yellow-grayish patch could be seen upon the tonsils, which spread rapidly over the soft palate, the mouth, and the pharynx; the cervical and submaxillary glands became swollen and inflamed. The outward appearance of the patient, the leaden aspect, the dulness of the eye, the uncertain step, bore evidence of the severe character of the disease, while the hoarseness of the cough, the change in the tone of the voice, the extremely fetid breath, and the grayish-black exudation upon the pharynx, were speedily followed by suffocating dyspnoea and by death.²

¹ *Researches on the Nature, Causes, and Treatment of Suffocative Angina, &c.* By Samuel Bard, M. D. New York.

² See cases reported by Bretonneau in his *Treatise*, 1826.

From Tours the epidemic spread to two small hamlets, La Fevriere and Chansonson, to which places it was for a time confined.

"From this time it continued to traverse the departments of France, passing mainly from the southern littoral districts towards the centre. It did not seem possible to ascribe its visitations to any particular climatic or meteorological conditions; for historical documents show that while it raged with terrible violence amongst the towns and hamlets of the Loiret, remarkable for their salubrity and the advantages of their geographical position, it passed over the villages of Sologne, seated amidst marshes; while elsewhere it seemed to select marshy and ill-drained districts, and to spare those which were in a better sanitary condition. Again, while in the year 1825, a year remarkable for its extreme dryness, the communes north of Orleans were laid waste by diphtheria, it made as many victims in the damp and warm year 1828, in the country south of Orleans."¹

In this year Trousseau saw thirteen individuals die out of seventeen in one farm, all attacked with diphtheria.

In 1841 an epidemic occurred in the Children's Hospital at Paris, which has been described by M. Becquerel.² In this many of the children were attacked with sore throat, sometimes false membranes being produced, and at others sloughs and gangrene, the one running into the other. The pharynx, larynx, and blistered surfaces were the parts attacked. There was a want of coagulability in the blood in all the cases, and pulmonary apoplexy often accompanied the maladies.

In the *Archives Générales de Médecine*, M. Empis³ gives a most valuable paper upon an epidemic of diphtheria which occurred at the Hôpital Nachez, in 1848. Both the mucous and cutaneous surfaces were attacked, in many cases conjointly.

There was a very virulent epidemic in Paris in 1855, attacking the rich and poor indiscriminately; carrying off adults, but expending itself more particularly upon children.

From the early part of 1855 to March, 1857, a very virulent epidemic prevailed in Boulogne, during which there died 366 persons, of whom many were English. A greater portion of those who were carried off were under ten years of age.

M. Lemoine has described an epidemic at Nièvre, in which the air-passages generally escaped.

In the department of the Haute Marne, the diphtheria had a decided predilection for the nasal fossæ, the larynx generally escaping. This epidemic was described by M. Jobert. M. Lespian has given an account of an epidemic which occurred among the military at Avignon, in the autumn of 1853, and in which the false membrane usually spread to the air-passages. Of 1796 soldiers, 195 were attacked; and of 22 children belonging to the regiment, 4.

In the cases secondary to other diseases nearly all died, while in the primary cases only 6 per cent. died.

Such is a concise history of the epidemics of malignant angina which have been observed in Europe, and more especially in France, during the present century. Before giving an account of the history of diphtheria in England, let us compare more closely the experience of French practitioners during the epidemics of the last few years with the observations of Bretonneau. We shall confine ourselves to a few of the most important points.

In his "*Traité de la Diphthérie*," Bretonneau says little of the consti-

¹ Report of the Lancet Sanitary Commission, 1859.

² Gazette Médicale de Paris, 1843.

³ March, 1850.

tutional symptoms which accompany diphtheria, probably because he did not attribute to them anything more than a secondary importance. He says :—

“ At the onset of diphtheria, the organic functions and those which belong to the life of relation, are so little disturbed that children who are already dangerously affected by malignant angina, generally retain their habitual appetite, and continue their play. . . . The disease only becomes mortal when the membranous layers which line the interior of the air-passages, form, by their accumulation, or by their adherence, a mechanical obstacle to respiration. . . .

. . . If a topical treatment modifies the diphtheritic inflammation, the return to health follows immediately on the cessation of the local disease.”¹

In a recent paper in the “*Archives Générales de Médecine*,” to which we have before alluded, Bretonneau has somewhat modified his notion that diphtheria is essentially a local disease. For, in the recent epidemics in France, the disease has come on so insidiously, and hastened to a fatal termination in a manner not to be explained by such a theory. Still maintaining the opinion that the constitutional state of diphtheria is secondary, and incapable of existing independently of the local changes, he assumes that whenever the disease takes on a suddenly fatal form, whenever the constitutional seem to precede the local symptoms, an explanation is to be found “not in the antecedence of a morbid diathesis, but in the secret development of diphtheria in the nostrils.” And this assumption seems to be founded solely upon the fact that in some cases coryza and glandular swellings have preceded the graver symptoms.

Although we cannot by any means agree with the distinguished observer in views which are so much at variance with modern experience, we must do him the justice to say that the characters of the disease, as observed by him in 1826, were undoubtedly as he has described them, but that during the last few years, the disease has assumed new forms and been attended with new dangers. Trousseau has most distinctly admitted this change of type of diphtheria, in the *Gazette des Hôpitaux*, 1855 :—

“There is a form of diphtheria to which, for seven or eight years past, innumerable victims have succumbed, which differs so completely from all others in the general aspect of its symptoms, that one would be tempted to establish a line of demarcation; but in directing our attention to its mode of invasion and etiology, we have no difficulty in recognizing conformity and even identity; the difference being that the diphtheritic disease assumes a character of exceptional gravity, and kills at once by the constitutional affection without the participation of the larynx. Usually the sorethroat seems to be the first symptom; but sometimes it is preceded by a coryza of great severity, as if the pituitary membrane had been attacked before the fauces.”

“There is also swelling of the lymphatic ganglia of the neck, which is sometimes so enormous as to extend beyond the jaw.

“Join to this acute pain in the head, extremely intense fever (excessive frequency of the pulse), and you will have the signs of the onset of the worst forms of diphtheria. Some hours after you will observe false membrane on the uvula and velum; the discharge from the nose becomes fetid, and if you open the nares with an ear speculum, false membranes are observed on the septum and turbinated bones. The patient does not sleep, and is in a state of extreme agitation; the breathing is stertorous and snoring. . . .

“After thirty-six or forty-eight hours, the features assume a livid pallor, delirium follows, and the unfortunate patient dies with all the appearance of anæmia, and in a state of somnolent tranquillity which strongly contrasts with the agitation that distinguishes the agony of croup. It is impossible to describe the horrible prostration, the powerless exhaustion, the frequent faintings, in one of which the thread of life is often severed.”

¹ Addition supplémentaire au Traité de la Diphthérie.

Again, in the course of a report read before the Imperial Academy of Medicine, on the 2d November, 1858, M. Trousseau makes the following remarks:—

“Those of us who for twenty-five years have followed the epidemics of diphtheria which have stricken the capital, may satisfy ourselves that the disease has not only extended itself considerably, particularly during the last twelve or thirteen years, but has assumed a much graver form. Up to about 1846, diphtheria scarcely appeared in the epidemic form, and the cases of it which were observed in Paris presented all the characters so well described by Bretonneau in his treatise, and so clearly pointed out by Guersant in the *Dictionnaire de Médecine*, where this excellent practitioner confirms in every particular what the illustrious physician of Tours had seen. . . .

“The diphtherite described by Bretonneau generally commenced in the pharynx, and there remained the longer in proportion to the youth of the child, giving rise usually to but little fever, scarcely in any way affecting the rest of the economy, and was propagated to the larynx, thus constituting croup. But within the last ten years, in place of this affection, comparatively of little severity, there has appeared another, in which hitherto all the resources of art have been almost unavailing.

“The pharynx, it is true, is most commonly first attacked, but in a little time the disease extends to the nares, to the nasal duct, and sometimes to the internal surface of the eyelids; and at the same time ataxo-adyamic symptoms become manifest, the pulse becomes very frequent, the cervical glands greatly enlarge, and frequently forty-eight hours after the attack, the patient dies, *without the larynx having been sufficiently affected to suggest the idea of croup*. It seems as though a poison had been introduced into the system, by which the latter had been intimately and rapidly modified.”

So also in the account given by M. Isambert¹ of the epidemic in Paris in 1856 we find, under the head of malignant diphtheritic angina, the following observations:—

“We retain the old name of malignant angina to designate that specific form in which the patient succumbs to a profound adynamia, to a general intoxication, and in nowise to the occlusion of the larynx. For in cases of this description tracheotomy not only does not save, but it does not even temporarily relieve the patient. This form of angina seems to have escaped the notice of M. Bretonneau, and as we cannot suppose that a man of his powers of observation could overlook a type of the disease so well marked, we must admit that it did not present itself in those epidemics, in the midst of which the eminent physician wrote his *Traité de la Diphthérie*. This form, then, appears to be a new one, although without doubt it is to this that many of the descriptions of the malignant or gangrenous anginas of the early epidemics apply.”

Having described the anatomical lesions, the enormous tumefaction of the cervical glands, and the other local changes, he goes on to say:—

“These local disorders, so grave in character, are accompanied by a general state not less serious: burning fever, extreme restlessness, insupportable headache, depriving the patient of all sleep, are present; shortly typhoid symptoms, the most complete adynamia, declare themselves; the fever appears to diminish towards the end, the pulse becomes small, and the patient falls into a condition of somnolent tranquillity, which announces the termination.”

Did we deem it necessary, in order to prove that the constitutional symptoms of diphtheria have been not only present, but have often assumed a primary importance during the epidemics of the last few years, we could refer to many other papers published by French practitioners of eminence. We shall, however, have occasion to observe the importance of these symptoms when we study the history of the English epidemics.

¹ Arch. Gén. de Médecine, 1857.

Again, one of the points most particularly insisted upon by M. Bretonneau is the absence of all relation between diphtheria and gangrene of the fauces. He even considers it characteristic of the affection that the mucous membrane remains unaltered throughout. He says that malignant angina is unaccompanied with any sloughing, and a contrary opinion could only arise from deceptive appearances, for in none of the cases at Tours, even when malignant angina had assumed the most repulsive aspect, could anything be discovered which resembled a gangrenous lesion.

In this opinion he certainly seems to be supported by historical testimony, especially as regards some of the epidemics of the last century.

"The results of the analysis of historical testimony do not differ in any respect from those which my own direct observations furnish me."¹

But in others of the recent French epidemics, in which researches were conducted with a special view to a solution of this point, gangrene has occurred as the expected termination of all the most malignant cases, and not as a mere accident. In the epidemic at Paris in 1841, described by M. Becquerel, and to which we have already referred, gangrenous sore throat prevailed at the same time with cases which presented the true characters of diphtheria. The two forms of disease were not to be distinguished as respects their origin, the local affection not being preceded by any constitutional symptoms. The fauces, too, in all cases, at first presented appearances purely diphtheritic. In those which in their progress took on the gangrenous aspect, the exudation became friable, and soon separated from the mucous surface. At first this was usually entire, but exhibited the appearance of a limited eschar, and, on being thrown off, left a deep excavation. The constitutional symptoms preceding death were the same as those which usually accompany gangrene—diminution of temperature, a rapid and almost imperceptible pulse, great restlessness, frequent vomiting, involuntary stools, &c. These cases were generally fatal.

From the fact that many of these cases were examined after death, there is no reason to suppose that there could be any mistake as to the actual presence of gangrene. In 15 cases examined, there was gangrene affecting the tonsils exclusively in 9, and in the remaining 6 the pillars of the velum and pharynx. In the tonsil the gangrene was either in the centre or near the surface. In either case the resulting cavity was irregular in form, filled with a thin fetid fluid, and was surrounded by softening of the submucous tissue, which was to a greater or less extent converted into greenish-gray detritus. The disintegration evidently always commenced beneath the mucous membrane, which, at first merely swollen and rugose, gradually took on a gangrenous appearance and colour, and finally terminated in an eschar. When this separated, the cavity was left exposed.

The history of this epidemic clearly shows that although the gangrenous form of diphtheria differs from the purely membranous in various ways, yet it occurs under the same epidemic influence.

M. Isambert, in his account of the Paris epidemics of 1855 and '56, distinguishes both forms of diphtheria, the one tending to a fatal result by extension to the larynx, the other, which he calls *angina maligna diphtheritica*, assuming a totally different character.

"It is particularly to this form," he says, "that are to be referred those confluent exudations of a dirty gray or black colour, giving out a gangrenous odour. . . . Several times we have observed undoubted loss of substance beneath the exudation."²

¹ *Traité de la Diphthérie*, p. 13.

² *Archives Générales*, 1857.

Again :—

"In this disease the membranous exudation, soon after its appearance, softens, and assumes a dirty gray or blackish colour, the uncovered mucous surface is livid, the adenetic swelling is enormous, and affects not only the glands themselves, but the cellular tissue, the skin often sloughing from extensive tension."

Death is preceded by gradually increasing prostration, but not accompanied by any nervous symptoms more marked than those described by M. Becquerel in the account of his epidemic.

M. Duche gives a description of the diphtherite which has proved so fatal for the last few years in the department of L'Yonne.

"The principal features of this epidemic (1858) are cephalalgia, fever more or less intense, pain in the fauces. Upon examining the mouth, the tonsils are found swollen and red, and on the surface of one—sometimes on both at the same time—there is a white patch of variable dimensions. These patches quickly enlarge, reach the velum palati and uvula, which latter, at times, becomes enormously enlarged; later, they invade the posterior wall of the pharynx, and descend gradually into the larynx and bronchia, and even into the cesophagus and digestive organs.

"The first period, which may be called pharyngeal, is characterized by a painful sensation, and the ejection from the mouth of abundant sputa, mixed with blood and false membrane. The invasion of the larynx is marked by all the signs of croup, and asphyxia rapidly terminates the scene of agony. On the contrary, when the larynx escapes, then there is an apparent calm, which deceives the most experienced eyes. Then there is a little vomiting of glairy matter, great thirst, absence of pain, but, soon, complete prostration; pulse insensible; absence of urine during four or five days, and death by syncope.

"It is generally easy, by aid of curved forceps, to seize and tear away the membranous exudations, when they cover only the tonsils, uvula, or pharynx. The mucous membrane, thus denuded, is livid and bloody; and in spite of the most energetic cauterizations, a few hours suffice for the reappearance of new morbid formations like the first. Gangrene of the pharynx often terminates the disease in a sudden manner, and we are warned of this fatal issue by the fetor of the breath, and of substances ejected from the mouth."

According to Bretonneau, diphtheria also includes croup. He says—"Croup is but the extreme degree of malignant angina." Now, it would certainly seem very evident, to those of us who have derived our ideas of the word croup, from Dr. Francis Home's description of this affection,¹ or from the graphic lecture on Cynanche trachealis, by Dr. Watson, that Bretonneau uses the word in a very different sense.

It is well known that Dr. Home first introduced the term croup into medical literature in 1765, and to him is due the honour of first defining the characters of a disease which had been in part described by the most ancient authors. He first drew attention to the fact, that the formation of a false membrane in the trachea and larynx is essential to the disease, and constitutes the source of danger.

Dr. Home's description of croup was not only accepted by most of the physicians of England, but also by many in Europe. His views were especially supported by the writings of Cheyne, Cullen, and others, but still more particularly by the report of the commissioners of the famous *concours* instituted by Napoleon. The ideas of these writers were, in brief,

"That croup is an acute inflammation of the mucous membrane of the air-passages, distinguished from others by the rapidity of its progress; by the existence of concrete exudation in the larynx, and by the fact that it principally attacks children under ten years of age. They regard cold and moisture as its

¹ Inquiry into the Nature, Cause, and Cure of the Croup—Edinboro', 1765.

main causes, and support this inference by all that is known as to the seasons during which the disease is most apt to occur, and the climates in which it is most prevalent; and they hold that it is its habit, to select for its invasion, single individuals in large populations, without communicating itself to the rest—in other words, that it is apt to be sporadic, not epidemic.”

Dr. Watson, in his lectures, says :—

“Some analogy with that disease (croup) it certainly has, but the points of difference are stronger and more essential. It resembles croup, inasmuch as it leads to the production of an adventitious membrane upon a mucous surface. It differs in the position of that membrane, which is seldom formed in the trachea. The affection of the windpipe, when it occurs at all, is secondary, so that the term ‘cynanche trachealis’ would be quite inappropriate.”

In an admirable lecture on Diphtheria, by Dr. Ranking, and published in the *Lancet*,¹ we find the following remarks :—

“The great distinctive mark between diphtherite and croup, properly so called, is to be found in the locality chiefly affected. In both, it is true, a main feature is the presence of an exudation; but in the one disease, it commences in the fauces, and only reaches the windpipe by extension, and in a certain number of cases; in the other, that of true croup: it commences in the larynx and trachea, and does not necessarily affect the soft parts above the glottis at all. As a consequence of this, a marked difference is also found in the symptoms of the two diseases. In diphtheria the uneasiness is first referred to the parts subservient to deglutition; in croup, on the contrary, the earliest symptom is that of stridulous voice and breathing—a symptom which, in the former, indicates the final development of diseased action.”

Dr. Hauner,² director of the children’s hospital at Munich, concludes a paper upon this subject with the following aphorisms :—

“1. True croup (laryngeal croup) is a disease proper to childhood, and its cause is chiefly to be sought in the organization (the period of development) of the larynx at this period of life. 2. The anatomy and physiology of the larynx sufficiently explain the nature of croup. 3. It cannot be shown that croup is connected with any peculiarity of the blood crisis. 4. True croup always commences in the larynx, and often passes downwards to the trachea, &c., but it never passes upwards. 5. Laryngeal croup is characterized by a pseudo-membrane of more or less extent. 6. Laryngeal croup is to be carefully distinguished from diphtheritic croup, the latter always depending upon a peculiar blood crisis, as seen in other organs of enfeebled individuals. 7. Diphtheritic croup is almost always secondary, and is not essentially different from croup in and after acute exanthemata. 8. The diphtheritic form begins, as a general rule, in the fauces, uvula, tonsils, &c., and extends hence downwards. It is very rare for it to commence in the larynx or trachea, &c.”

It is well known that Dr. West, in his work upon diseases of children, has considered diphtheritis as a form of croup. In the last edition of his work, however, he has seen fit to somewhat modify his previous views. In speaking of croup and diphtheria, he says :—

“Of these two diseases, the one is almost always idiopathic, the other is often secondary; the one attacks persons in perfect health, is sthenic in its character, acute in its course, and usually proves amenable to antiphlogistic treatment. The other attacks by preference those who are out of health, or who are surrounded by unfavourable hygienic conditions, and is remarkable for the asthenic character of the symptoms which attend it. The one selects its victims almost exclusively from among children; is incapable of being diffused by contagion; is governed in its prevalence by influence of season, temperature, and climate, but rarely becomes, in the usual acceptation of the term, an epidemic. While the

other attacks adults as well as children, is propagated by contagion, and, though it occasionally occurs in a sporadic form, is susceptible of wide-spread epidemic influence.

“Different, however, as are the two diseases, there are yet between them points of similarity no less striking, and the diagnostic difficulties are still further enhanced by the occasional simultaneous prevalence of both affections.

“It has, indeed, been suggested by M. Isambert, in a recent valuable paper, that the condition of the subjacent mucous membrane furnishes a ground of distinction between the affections; and that while in diphtheria the surface beneath the exudation is often ulcerated, no such erosion of the mucous membrane is met with in true croup. This is not, however, according to my observation, for ulceration of the mucous membrane has come under my notice in primary croup, though less frequently than in cases of the diphtheritic kind.

“Whatever differences exist between croup and diphtheria, must be sought elsewhere than in the pathological changes observable in the respiratory organs; and the affinities of the latter disease are seen to be to the class of blood diseases, rather than to that of purely local inflammation to which croup belongs.”

With Bretonneau, nearly all French writers regard croup and diphtheria as identical. In justification of this view, so little consonant with our own ideas, we may remark that in France, true croup is commonly introduced by a diphtheritic affection of the fauces, and that sometimes it appears to be contagious, which is not considered to be true of the sporadic disease as observed in England and in our own country. Moreover, in France, it differs by its asthenic character, and to some extent by the nature of the exudation, which is less tenacious.

In fine, the laryngeal diphtheritis of Bretonneau, and of other French authors, although closely resembling the disease described by Home, and known to us as croup in its anatomical characters, differs widely in its dynamical ones. Moreover, it is contagious and epidemic.

Bretonneau has also in a measure confounded scarlatina with diphtheria, under the term “Scarlatina Anginosa.”

The exact relation which exists between these two diseases has been a much debated question. By some persons the two affections, notwithstanding certain points of strong resemblance, are regarded as essentially different. By others, diphtheria is regarded as a form of scarlet fever, in which the throat affection is unaccompanied by the eruption which usually characterizes it.

We must admit that there are many circumstances which favour this latter opinion. For instance, not only do the two diseases prevail frequently at the same time in the same region, but even in the same family; some members being attacked by all the symptoms of true diphtheria, while others present the symptoms of common scarlatina. Then again in some instances, in those who have been attacked by diphtheria, a rash very similar to that of scarlatina has been observed. This rash may have been very partial, and may have remained but a few hours, but its characters have been thought sufficiently marked to leave no doubt as to its nature. Moreover, since the albuminous condition of the urine has been so frequently observed in cases of diphtheria, it may be thought that the analogy between the two diseases is drawn still closer.

These facts are certainly of great weight, but we shall see that there are other considerations still stronger that may be adduced in favour of the essential difference between the two diseases. For example, as regards the existence of a rash in diphtheria. This has certainly been occasionally noticed in some epidemics of diphtheria, but in the great majority it has not been observed at all. Whereas, in epidemics of scarlet fever its absence

is a rare exception, and occurs only in those cases of very malignant character which are marked by great cerebral disturbance, violent delirium, and by speedy death. In diphtheria, on the other hand, the intellect remains undisturbed until the very last.

Then, again, the rash is in many respects dissimilar from that seen in scarlatina. It is described as being for the most part, of a uniform erythematous redness, without the peculiar punctated appearance of the scarlet fever rash, appearing suddenly in patches, not deepening in intensity gradually, and not followed by any change in the other symptoms, nor by any increase in their severity.

As to the presence of albumen in the urine, there are certain points to be especially observed. When present, there is no diminution in the quantity of the secretion, neither is there any other particular change in its character. Moreover, the albumen seems often to disappear at a very early period of the disease.

"Its disappearance takes place suddenly, and though its presence is usually observed in cases where this disease is severe, yet there does not seem to be any necessary connection between the urine becoming non-albuminous, and the disease assuming a milder type."

Again, the sequelæ of the two diseases are widely different. For while, on the one hand, we have none of the formidable dropsical symptoms in the convalescence of diphtheria, which so often succeed to scarlatina, on the other, we *do* have a peculiar loss of nervous power, and temporary muscular paralysis which have no analogy to anything in the sequelæ of the latter disease.

Lastly, almost universal experience bears testimony to this fact, viz., that diphtheria does not protect from scarlet fever, nor, on the other hand, does scarlet fever form any defence against diphtheria. Of this, the following may serve as examples:—

Dr. Edward Ballard² reports the case.

"Three children in a family in my district (Islington) were attacked with diphtheria in August, 1858. Two of them died; the third, aged three years, recovered. I saw these children, and satisfied myself that there was no error in the diagnosis. In January, 1859, the child that recovered was attacked with scarlet fever, after playing about upon a carpet brought from a house where a fatal case of this disease had occurred. There was both the rash, and the usual throat affection, but no diphtheritic exudation; and the child died."

Dr. West gives the following case:—

"In a school in the neighbourhood of London, diphtheria broke out; many of the lads were affected by it, and one or two died. Several of those who were convalescent from the disease were sent to the sea coast for the more speedy recovery of their strength, and while there some were attacked by scarlet fever, and this also, in one or two cases, proved fatal.

Numerous cases similar to these might be cited, but the above will suffice. And, although, further and more accurate observations may hereafter tend to a different conclusion, we are decidedly of the opinion that the balance of evidence at the present time is in favour of the non-identity of scarlatina and diphtheria.

As regards the history of the earlier epidemics of "sore throat" in England, we have few reliable accounts, and even of the origin and progress of the late epidemics of diphtheria, our knowledge is far from being either ac-

¹ Dr. West, *Diseases of Childhood*, 1859.

² *Med. Times and Gazette*, July 23, 1859.

curate or satisfactory. We have already alluded to the description of the epidemics of throat disease by Fothergill and Huxham, about the middle of the last century, as also to the admirable paper of Dr. Starr. The first of these writers, as we have seen, speaks distinctly of sloughs in the fauces which leave ulcers. Huxham and Starr speak of the exudation extending to the air-passages. It is not a little remarkable, that the same neighbourhood in Cornwall (Liskeard and the other towns in which the epidemic of ulcerous sore throat described by Dr. Huxham, prevailed), has been subject during the last three years to a similar affection, and which closely resembles the disease described by M. Becquerel, inasmuch as the membranous exudation of unusual thickness is associated with softening and destruction of the submucous tissue. This epidemic has been well described by Mr. Thompson, of Launceston.¹

"About three years since, this neighbourhood was visited by an epidemic of this disease. The first cases occurred in the town; and no others then appeared for several months, when it again broke out in the district north of this place, where it prevailed for several months; whilst the south side was comparatively free from it. From the north it gradually spread until the whole line of country had been visited by it. There appeared to be no difference in the geological nature of the country, the level, or the aspect, in increasing the severity, or granting an immunity from the disease. The premonitory symptoms varied somewhat. A few retired to rest comparatively well, and awoke in the morning with the throat sore, and covered with white deposit. In the majority it was preceded by all the ordinary symptoms of pyrexia, of which headache was one of the most severe; followed in the course of a day or two by the usual throat symptoms. An extreme feeling of depression, not to be accounted for by the amount of mischief in the throat, was a characteristic symptom in each case. An external examination of the throat showed the tonsil generally to be swollen, hard, and tender to the touch; whilst sometimes the parotid gland participated in the swelling. Internally the tonsil was swollen, and either covered with the diphtheritic deposit which frequently extended over the pharynx, and sometimes into the nares and palate; or else it would be scooped out into an ulcer with raised violet-coloured edges; the floor exhibiting a dark ash-coloured slough. In some instances there would be no deposit or ulceration at first, but simply the tonsil painful and enlarged. These cases generally change for a state of ulceration, which began in several distinct spots, and gradually spread over the whole tonsil. In the most severe examples, the tonsil sometimes sloughed *en masse*. I saw one instance in which this account in an early stage of the disease, and where now (two years since it occurred) a cavity remains capable of containing a pigeon's egg, across the surface of which extends a small band of mucous membrane which did not slough at the same time, and gives great inconvenience from retaining the food impacted in the hollow during deglutition. I have seen no case in which I could detect the extension of the disease into the œsophagus; but in many it has entered into the air-passages, this being the most frequent and most fatal complication."

"It can scarcely fail to strike the reader that the affection under consideration would be just as correctly designated by the term 'sore throat with ulcers,' employed by Huxham and Fothergill, as by that of diphtheria, a fact which appears the more remarkable when we consider that the very towns in which Huxham's disease most prevailed in 1748-50, have been most severely visited during the last few years. Are these two epidemics, separated by an interval of more than a century, of the same nature? A careful comparison of their symptoms assures us that they are, and that Bretonneau in disclaiming all relationship between his diphtheria and the 'sore throat with ulcers' was mistaken."²

As we have before remarked, in the year 1765 Dr. Home published a small

¹ Brit. Med. Journal, June, 1858.

² Brit. and For. Med.-Chir. Review, Jan. 1860.

treatise¹ upon the disease of the larynx which had long been known, but the characters of which had never been clearly defined. To this he gave the name of croup, and upon this the modern doctrine of croup is based. Home's description is based upon the careful observation of twelve cases, in ten of which post-mortem examinations were made. He first pointed out that the formation of a false membrane is essential to the disease, and that its presence in the larynx is the source of danger. Others also published their observations upon this disease, among whom were Cheyne and Cullen. The affection described by these writers is essentially different from the croup of Bretonneau.

From this time, although we may gather a few scattering allusions to diphtheria from British medical literature, until its recent outbreak it was a disease practically unknown to even the most experienced of English practitioners, certainly, in the form in which it has recently presented itself.

The advent of the present epidemics of the disease first attracted public attention in England, in the autumn of 1857, a few cases having occurred for twelve months previously. It first appeared in the southeastern counties, especially in Kent, in the town of Canterbury. In Essex, particularly in the marshy districts, it prevailed extensively; thence it spread through all the eastern counties.

"The local name was 'throat fever.' It appeared after arriving at a certain stage to baffle medical skill, and something of a fungus nature showed itself in the throat. Croupal suffocations was one of its complications, which appears to eliminate 'putrid sore throat,' and those, therefore, who classify this Cornish epidemic with diphtheria are probably warranted in so doing."²

During the next summer months the disease spread northwards to Lincolnshire and Yorkshire. In the winter months of 1858 the southeastern counties still suffered. In parts of Essex the disease was almost universal.

"At Teignmouth, Mr. Lake observed cases of that severest form of diphtheric inflammation, in which the local manifestation of the disease is from the first overshadowed in importance by the constitutional symptoms. The blood-making powers were seriously compromised after the annihilation of the throat affection, the patient sinking then through general failure of the powers of life, without anything like typhoid symptoms, a distinction which it is very important to maintain, or being left in a state in which he is liable to be carried off by any prevalent disorder, or during convalescence continuing unusually weak and anæmic."

In Suffolk, and in some of the eastern counties, scarlatina prevailed in conjunction with diphtheria, so also in Nottinghamshire. In the north-western counties we find hooping-cough and diphtheria prevailing.

In fact the disease spread to almost all parts of England, appearing with much greater severity in some localities than in others. Dr. Hart has given in his report a very succinct account of its progress through the country.

If now we examine some of the various accounts of the recent epidemics in England, as they have appeared in various parts of the country, there will be seen to be a considerable amount of discrepancy, and, moreover, many of these accounts will be found to differ widely from Bretonneau's model. We select a few as they have appeared in the various journals of the day.

At a meeting of the Harveian Society,³ Dr. B. Sanderson said:—

¹ Inquiry into the Nature, Cause, and Cure of Croup, Edinb., 1765.

² On Diphtheria, by Ernest Hart, London, 1859.

³ Lancet, Oct. 1857.

"That the disease recently prevalent in England was identical with the malignant sore throat described by many authors, and that in a great number of instances scarlatina precedes it. It was attended with much fever and fetid breath, the fever sometimes of a typhoid character. The thickness and adhesiveness of the exudation was less marked than that occurring at Tours. In England exhaustion and fever destroyed the patient rather than asphyxia, which suddenly put an end to Bretonneau's patients. In true diphtheria there was no fever and no fetid breath; both these were remarked in this country. Finally, he believed croup and diphtheria identical, and that the disease in England was not diphtherite, but the pultaceous pharyngitis of the French."

Dr. Laycock,¹ of Edinburgh, in a clinical lecture, published May, 1858, regards diphtherite as a disease produced by a fungous growth—"oïdium albicans"—the same that is found in thrush. He says:—

"If the fungus multiply in a population at the same time that there is an epidemic of scarlatina or rubeola prevalent, that epidemic may be expected to take the diphtheritic form."

His remarks, however, appear to be founded only on one case, in which there was an aphthous affection of the mouth and throat.

Dr. Kingsford,² in a letter to the *Lancet*, thus speaks of the disease as it has come under his observation:—

"Diphtheria may be divided into the mild and severe forms.

"The mild form, which, for the sake of distinction, may be designated the diphtheritic sore throat, is ushered in by a variable amount of feverishness, loss of appetite, and at first only slight pain in swallowing; the tongue presents a thick, white, creamy coat, through which some of the papillæ are visible; the velum palati, uvula, and pharynx are of a bright red colour; the tonsils are much swollen, and of the same livid hue, and upon the inner side of one or both of them distinct white patches are seen, which in some instances resemble an exudation from the sulci of the tumid gland, but more frequently are flat and filmy in appearance, not confined to the tonsils alone, but spread over the uvula and posterior wall of the pharynx; both the exudation and the filmy deposit adhere tenaciously to the submucous surface, and cannot easily be scraped off. Ulcerative stomatitis not rarely precedes and accompanies this mild form of diphtheria—indeed, by some they are considered to be identical; the parotid and submaxillary glands are not much swollen, although one or two enlarged glandulæ concatenatæ may often be detected.

"The severe form, or genuine diphtheria, is always characterized by a high state of fever, hot, pungent skin, flushed countenance, congested lips, a rapid, feeble pulse, great difficulty in swallowing, and hurried respiration; the tongue is covered by a thick, dirty, yellowish-brown or sometimes slaty-coloured coat; the velum palati, uvula, and pharynx are of a deep, dark, erysipelatous redness; the tonsils usually enormously swollen, and of the same dark red colour, but instead of the white patches observed in the mild form a large ash-coloured membrane is spread over the inner side of one or both tonsils, and also upon the uvula and posterior wall of the pharynx. As the disease advances, the above symptoms increase in severity; the breathing becomes stertorous from mechanical obstruction; deglutition so painful that young children will refuse to swallow even liquids; the saliva dribbles from the mouth, and a foul acrid discharge often flows from the nares; the pulse becomes more rapid and feeble; the glands of the neck are now swollen and tender, and the voice is hoarse and indistinct; the patient restless, tosses about upon the bed, or else lies on his back in a semi-comatose state. These cases, when fatal, terminate either by rapid prostration of the vital powers, or by an affection simulating croup, from extension of the diphtheritic membrane into the air-passages; in both instances death is usually preceded by obstinate vomiting, probably the result of inflammation or irritation of the par vagum. . . .

¹ See Appendix A and B.

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² *Lancet*, Nov. 1858.

"In fatal cases, the *post-mortem* examination reveals the ash-coloured membrane spread over the pharynx, extending to the posterior nares and down the œsophagus; but when death is preceded by symptoms of croup, it is found also in the larynx and trachea. Upon detaching this membranous exudation, the submucous surface presents an ecchymosed appearance, but no distinct signs of ulceration."

Dr. Heslop, in a communication to the *Medical Times and Gazette*,¹ believes that, although so little known now, this disease was well understood and described by former British authors, especially Fothergill. It is a pestilence with well-marked features. It is contagious, though not highly so, and its ataxic phenomena are most striking—prostration, quite disproportionate to the amount of disease in the throat, coming on early, and remaining after all other indications of disease have passed away. In the worst cases a foul ulcerous condition of the fauces complicates the genuine membranous angina. The mode of death, as in other pestilences, is by asthenia, and frequently the event is sudden and unlooked for.

Dr. Heslop points out at length the differences between this affection and croup.

Dr. Whitehead, in the same journal, describing the disease, states that the symptoms are very similar to those of croup, but they come on suddenly, without the peculiar crowing, after what seems a slight sore throat. On examining the fauces then, they are found red and dry, the tonsils dripping with a thick, opaque, offensive matter. Sometimes there is also great external swelling of the throat.

Dr. Camps² believes that three distinct varieties of the disease, if not three distinct diseases, have prevailed. 1. Cases which have presented a precise resemblance to those described by Bretonneau. 2. Other cases presenting many of the characters of the Fothergill sore throat. 3. Cases consisting in the sore throat accompanying scarlatina, whether the eruption has been present or not. The type of diphtheria, properly so called, is essentially asthenic.

Dr. Pollock³ believed that Bretonneau had painted the disease too strongly. True diphtheria, so described, was not a prevalent disease, but many cases more or less approximated to it. All such arose from poisonous influences, and however different, they were yet identical. In the same family these throat affections may approximate to and diverge from the diphtheritic type, there being in some exudations, in others ulceration and excoriation.

Mr. Bottomley,⁴ of Croydon, remarks as follows:—

"It appears to me that at the commencement of the attack there is but a slight congestion of the mucous membrane of the pharynx, accompanied with slight constitutional disturbance; but in a few hours the membrane puts on a livid appearance, and runs rapidly into the gangrenous state; and that the false membrane is a deposit of layers of lymph in the early stage of the disease, which soon loses its vitality, and acts as an extraneous body, thereby preventing the parts from performing their natural functions, and, accompanying this change, great depression of the vital powers of the system takes place."

Mr. Thomas Smith, of Kent County, writes:—

"There are three forms in which the disease presents itself, viz: simple ash-coloured diphtheric membrane in patches, with very slight congestion of the surrounding parts, and without fetor; secondly, a deeper colour, and more widely spread membranous exudation, with fetid breath, and intense engorgement of dark hue; thirdly, the membrane with much tonsillitis, in a few cases resulting

¹ May 29, 1858.

² Med. Times and Gazette, March, 1853.

³ British Medical Journal, July, 1859.

⁴ Ibid.

in quinsy. But there has been a fourth and more formidable state of things to contend with, viz., an extension of the membrane, in either of the above forms, to the larynx and trachea. . . . Lately there has been more tonsillitis, and frequently superficial ulceration. There is a depression of the vital powers.

"In observing the progress of this epidemic, I have been instinctively led to reflect on the altered type of disease in general. I have myself no doubt of that alteration in the type of disease observed since the year 1832 in England."

Mr. Cammach,¹ of Bennington, remarks:—

"Diphtheria was epidemic in this district last year, in November and December, and has been so again since July. Diphtheria varies in extent from simple herpes of the lips or nose, which are covered with vesicles which burst, ulcerate, and heal in two or three days, to the most extensive inflammation and sloughing and ulceration of the cheek, the palate, and the pharynx; and more in children than in adults. It extends into the larynx and trachea, and kills by asphyxia. In the mildest form there is a tendency to ulceration beneath a white, loosely attached membrane. . . . In the worst cases its vesicular nature can be distinctly traced, for a few hours after its commencement, from the large patch within the cheek or upon the gum, which will slough like cancerum oris, to the more diffused bullæ upon the soft palate and pharynx."

Dr. Moncton, in a letter to the *Medical Times and Gazette*, June, 1857, says:—

"Diphtheria is a distinct disease, easily recognized, and not to be dreaded till such changes have occurred about the fauces and tonsils as it is impossible to overlook. A remote kinship there certainly is between it and scarlet fever, but identical they are not. . . . Though, as the diphtheritic membrane loosens and separates from the surface of the throat and tonsil, sloughing ulceration *may* ensue, I feel at present fully persuaded that diphtheria and cynanche maligna are not the same thing. . . . The constitutional symptoms, at first altogether slight, become very real as the disease advances. The main feature is prostration, not typhoid at all—no coma, no sensorial disturbance throughout, no sordes, no heavy lurid look; and in many cases the practitioner, if not warned by previous experience, or a careful observation of the pulse, is surprised to learn that the patient he left with clear countenance, cheerful manner, and little suffering, a few hours ago, has just gone off, while casually sitting upright, in a fatal syncope. . . . The practical fact is, however, this, that after the fourth or fifth day a diphtheritic patient becomes the subject of very real asthenia, not so much perceived by the patient as discovered by the lax pupil and feeble pulse, and that this state is the one which, about the eighth day, is too apt to terminate in death."

Dr. Copeman, in an essay recently published, on diphtheria, remarks:—

"On turning our attention to the features presented by the present epidemic, we shall find that, as a general rule, the constitutional symptoms bear but little proportion to the local mischief, and the danger chiefly to be feared is the extension of the false membrane into the larynx and trachea, so as to produce suffocation in the same way as in croup. . . .

"It is true that, on the first appearance of the epidemic, in several instances it knocked down its victims at once, showing itself as a poison too powerful to give time for the development of any decided symptoms, either constitutional or local. But this is a character common to almost all severe visitations of epidemic disease at their first onset, and, as I have said before, many of the patients who have since died from it have exhibited no very marked constitutional disturbance."

Thus it will be seen by these various accounts of the disease in Great Britain that not only was a distinct loss of substance in the fauces frequently observed, but that the great prostration and general constitutional disturbance did not fail to attract the attention of almost every practitioner.

¹ *Lancet*, Oct. 1858.

If the materials for a full and satisfactory account of the epidemics of sore throat which have prevailed in Great Britain are scanty, they are very much more so as regards our own country.

Dr. Douglas, of Boston, in the year 1736, seems to have published an account of the first appearance of a "sore throat distemper" in this country. This account is alluded to by Dr. Bard in his valuable paper. The epidemic which he describes was very malignant, and was attended with "erysipelatous appearances and highly putrid symptoms."

* In the first volume of *Medical Observations and Inquiries*, published in London in 1771, is an extract from a letter from Mr. Cadwallader Colden to Dr. Fothergill, concerning the *throat* distemper, dated—Coldenham, New York, October 1, 1753. He says:—

"The first appearance of the throat distemper was at Kingston, an inland town of New England, about 1735. It spread from thence, and spread gradually westward, so that it did not reach Hudson's river till nearly two years afterwards. It continued on the east side of Hudson's river before it passed to the westward, and appeared first in those places to which the people of New England resorted for trade, and in the places through which they travelled. It continued to move westwardly, till I believe it has at last spread over all the British Colonies on the Continent. Children and young people were only subject to it, with a few exceptions of some above twenty or thirty, and a very few old people who died of it. The poorer sort of people were more liable to have the disease than those who lived well with all the conveniences of life, and it has been more fatal in the country than in great towns.

"In some families it passed like a plague through all their children; in others, only one or two were seized with it. Ever since it came into the part of the country where I live (now about fourteen years), it frequently breaks out in different families and places without any previous observable cause, but does not spread as it did at first. It seems as if some seeds, or leaven, or secret cause remains wherever it goes. When the distemper becomes obvious, it has the common symptoms attending a fever, except that a nausea or vomiting is seldom observed to accompany it.

"It is attended with a moist putrid heat, the skin being seldom parched. The pulse is usually low, but frequent and irregular. The countenance dejected with lowness of spirits; no considerable thirst; the tongue much furred, and the furring sometimes extends all over the tonsils as far as the eye can reach. At other times, in the milder kind, the tonsils appear only swelled with white specks of about a quarter of an inch or half an inch in diameter, which are thrown off from time to time in tough, cream-coloured sloughs. Sometimes all the parts near the gullet or throat are much swelled both inwardly and outwardly so as to endanger suffocation, and frequently mortify; but most generally the swelling internally is not so much as to make swallowing difficult. Sometimes these swellings imposthume. The last complaint is commonly of an oppression or strictness in the upper part of the chest, with difficulty of breathing, and a deep, hollow, hoarse cough, ending in a livid, strangled-like countenance, which is soon followed by death. This disease is not often attended with that loss of strength that is usual in other fevers; so that many have not been confined to their beds, but have walked about the room till within an hour or two of their death; and it has often appeared no way dangerous to the attendants, till the sick were in their last agony. Some died on the fourth or fifth day; others on the fourteenth or fifteenth day, or even later. When this disease first appeared, it was treated with the usual evacuations in a common angina, and few escaped. In many families, who had a great many children, all died; no plague was more destructive."

As we have before remarked, Dr. Saml. Bard, in 1771, gave a very faithful description of an epidemic of sore throat, which prevailed in New York. As will be seen in the extracts which we give from his treatise (now become excessively rare), his opinions correspond with those of Bretonneau. He

recognizes the analogy between this disease and croup, as well as the manner in which it spreads from the throat to the larynx. He observed it sometimes as simple angina; sometimes as angina complicated with laryngitis, and occasionally as laryngitis alone.

In general the disease was confined to children under ten years of age, though some few grown persons, particularly women, had symptoms very similar to it. Most of the persons attacked were observed to droop before they were confined. Generally, the first symptoms were a slightly inflamed eye, livid countenance, with slight eruptions upon the face. At the same time, or very soon after, those who could speak complained of an uneasy sensation in the throat, but without much soreness or pain. Upon examination, the tonsils appeared swelled and highly inflamed, with a few white specks upon them, which, in some cases, increased so as to cover them all over with one general slough; this, however, although a frequent symptom, did not invariably attend the disease. The breath was not offensive, and deglutition but very little impeded.

These symptoms continued in some for five or six days without creating any alarm; in others, a difficulty of breathing came on within twenty-four hours, especially in the time of sleep, and was often suddenly increased to such an extent as to threaten immediate suffocation. Generally, it came on later, increased more gradually, and was not constant.

This stage of the disease was attended with a very great and sudden prostration of strength, a very peculiar hollow dry cough, and a remarkable change in the tone of the voice. In some the voice was almost entirely lost, and would continue very weak and low for several weeks after recovery. These symptoms continued for one, two, or three days, and greatly increased in those who died; a purging in several cases came on, the difficulty of breathing became more marked, and the patient died apparently of suffocation. This commonly happened before the end of the fourth or fifth day. One child, however, lived under these circumstances to the eighth day. Shortly before he died, his breath and expectoration were somewhat offensive; "but this was the only instance in which I could discover anything like a disagreeable smell, either from the breath or expectoration."

In some cases, instead of the difficulty in respiration, very troublesome ulcers appeared behind the ears.

"These began with a few red pimples, which soon ran together, itched violently, and discharged a great deal of very sharp ichor, so as to erode the neighbouring parts, and in a few days spread all over the back part of the ear, and down upon the neck."

In a few cases, swelling of the parotid and sublingual glands was noticed. Dr. Bard says:—

"I met with but two instances of anything like this complaint in adult persons. Both of these were women, and one of them had assisted in laying out two of the children that died of it. At first her symptoms resembled rather an inflammatory angina; but, about the third day, the tonsils appeared covered in some places with sloughs resembling those on the tonsils; her pulse was low and feeble; she had a moist skin, a dejection of spirits, and some degree of anxiety, though nothing like the difficult breathing of the children.

"The other was a soldier's wife, who, for some time before she perceived any complaint in her throat, laboured under a low fever. Her tonsils were swelled and inflamed, and covered with sloughs resembling those of the children; but her breath was more offensive, and she had no suffocation.

"I have had an opportunity of examining the nature and seat of this disease from dissection, in three instances. One was a child of three years old. Her

first complaint was an uneasiness in her throat. Upon examining it, the tonsils appeared swelled and inflamed, with large white sloughs upon them, the edges of which were remarkably more red than the other parts of the throat. She had no great soreness in her throat, and could swallow with little or no difficulty. She complained of a pain under her left breast; her pulse was quick, soft, and fluttering. The heat of the body was not very great, and her skin was moist; her face was swelled; she had a considerable prostration of strength, with a very great difficulty of breathing; a very remarkable hollow cough, and a peculiar change in the tone of her voice. She was exceedingly restless; was sensible, and when asked a question, would give a pertinent answer; but, otherways, she appeared dull and comatose. All these symptoms continued, or rather increased, until the third night, on which she had five or six loose stools, and died early in the morning.

“Upon examining the body—which was done on the afternoon of the day she died—I found the fauces, uvula, tonsils, and root of the tongue interspersed with sloughs, which still retained their whitish colour. Upon removing them, the parts underneath appeared rather pale than inflamed. I perceived no putrid smell from them, nor was the corpse in the least offensive. The œsophagus appeared as in a sound state. The epiglottis was a little inflamed on its external surface; and on the inner side, together with the inside of the whole larynx, was covered with the same tough white sloughs as the glands of the fauces. The whole trachea, from the larynx down to its division in the lungs, was lined with an inspissated mucus, in form of a membrane, remarkably tough and firm; which, when it came to the first subdivisions of the trachea, seemed to grow thin and disappear. It was so tough as to require no inconsiderable force to tear it, and came out whole from the trachea, which it left with much ease; and resembled, more than anything, both in thickness and appearance, a sheath of thin chamois leather. The inner membrane of the trachea was slightly inflamed; the lungs, too, appeared inflamed, as in peripneumonic cases, particularly the right lobe, on which there were many large livid spots, though neither rotten nor offensive; and the left lobe had small black spots on it, resembling those marks left under the skin by gunpowder. Upon cutting into any of the larger spots which appeared on the right lobe, a bloody sanies issued from them without frothing.”

Dr. Bard attributes the prevalence of the epidemic which he describes to a particular disposition of the air, or *miasmata sui generis*—

“Which more or less according to particular circumstances generate an acrimony in the humours and dispose them to putrefaction; and which have a singular tendency to attack the throat and trachea, affecting the mucous glands of these parts in such a way as to occasion them to secrete their natural mucus in greater quantities than is sufficient for the purposes of nature, and which in this particular species, when secreted, is really either of a tougher or more viscid consistence than natural, or is disposed to become so from rest and stagnation.”

The disease Dr. Bard considered of an infectious nature. In the treatment he advocated bleeding, according to circumstances, and the use of mercury, gargles, fomentations, &c., as local remedies.

We have devoted much space to the remarks of Dr. Bard. But his little treatise has always been considered as very accurate and truthful in its delineations, and as a valuable contribution to medical science. His observations are quoted by almost all writers on this subject since his day, and particularly by Bretonneau.

Since the epidemic described by Dr. Bard, we do not find any other of a similar character mentioned by writers, until, in 1831, Dr. Bell speaks of having witnessed this affection in an epidemic form in Philadelphia. For the last few years, however, as in England, diphtheria has been much more frequently met with, and in some portions of the United States, especially in California, very fatal epidemics have prevailed. The medical journals in the various parts of the Union contain numerous descriptions of the disease

as it has prevailed in certain sections. From a few of these we select extracts.

A terrible epidemic occurred at San Francisco, and in other towns of California, in the autumn of 1856. It had all the characters of pharyngeal diphtheria. Dr. J. V. Fougeaud¹ has published a monograph on this epidemic, in which he speaks of the mortality amongst children in several counties around the Bay of San Francisco as having "assumed an appalling character."

"Few children attacked by it recovered. The disease begins in a very insidious manner by a little engorgement or inflammation of the soft palate, pharynx and one of the tonsils. (The attack seldom commences on both at the same time, but soon extends to both if not arrested.) At this period of the malady, the patient complains but little, there is often no fever, or it is very moderate. The pain in the throat is much slighter than in the usual forms of common sore throat, so slight indeed, that the little patients go about playing as if nothing was the matter. In some exceptional cases, however, the fever and inflammation about the pharynx are considerable from the beginning. The characteristic signs of the affection soon follow this period of invasion. They consist in small portions (plaques) of white or yellowish lymph deposited on the soft palate, the tonsils, and the posterior part of the pharynx. The cervical and submaxillary gland becomes inflamed and swollen, and the pain in swallowing and opening the mouth is occasioned more by the engorged state of the glands than by the internal secretion of lymph. These deposits go on increasing in size more or less rapidly, and, in violent cases, in a few hours the whole cavity of the throat is covered by them. Generally one side is more affected than the other, and upon examination the glands corresponding with the parts affected will be found more swollen than those of the opposite side."

Dr. James Blake,² of Sacramento, in a memoir on this subject, says:—

"The first effect produced by the poison is evidently on the nervous system. Drowsiness, prostration, or oppression, are manifested by infants, or complained of by adults, and when the disease is prevailing this desire of children to sleep at other than their usual hours should awaken our suspicions. The pulse is accelerated from the first, but generally soft and typhoid, although in some cases it is for a few hours rather hard. The temperature of the skin is raised, although it is seldom harsh or dry, but frequently moist, or even covered with profuse perspiration. There is seldom any pain, rarely headache or backache. The tongue is usually coated, edges red, and papillæ prominent. The appetite may remain good, and the digestion unimpaired. If we examine the throat, we may, even within twelve hours after the occurrence of the first slight symptoms, find the tonsil covered with a grayish, pultaceous exudation, which rapidly extends upwards into the nostrils, and downwards towards the larynx; and again we might detect only a redness of the tonsil, and a small point of exudation two or three days after the commencement of the disease, and at a time when the symptoms of general prostration had become alarming.

"Again, cases present themselves in which the general symptoms and the anatomical lesions proceed *pari passu*; but in almost every case that I have seen. I have considered that death was the result rather of the action of the poison on the system, than from obstruction of the larynx. In from twelve to twenty-four hours after the formation of exudation on the tonsil, we shall generally find the cervical glands enlarged, and in protracted cases this enlargement may become so great as to afford a serious obstacle to deglutition and respiration. I have seen cases in which I think death was thus produced, when the patient might otherwise have rallied from the effect of the poison.

"The duration of the disease is very uncertain. I have seen it terminate fatally in four days from the first ascertainable departure from perfect health,

¹ Diphtheria: a Concise Historical and Critical Essay, &c. Sacramento, 1858.

² Pacific Med. and Surg. Journal, August, 1858.

and this in a strong, healthy child, and I have witnessed it run along for two or three weeks, and then terminate fatally. The cases that arise from contagion, and remain exposed to the original source of contagion, I believe, as a general rule, run a more rapid course than the sporadic cases; thus we frequently find two or three children in the same family dying within a day or two of each other, although the sporadic case might have had the disease some days before the others took it. This is probably owing to the continued absorption of the poison in a state of concentration."

In a communication to the *Boston Medical and Surgical Journal*, Dr. L. N. Beardsley, of Milford, Conn., writes that—

"This disease [diphtheria] appeared in an endemic form and with great mortality in this vicinity during the months of March and April last. It first made its appearance in Orange, an adjoining town (which is in an elevated situation, and is a remarkably healthy place, with a sparse population), and for a while was confined entirely to the scholars attending a select school in the village. . . .

"Fourteen cases out of fifteen, of those who were first attacked, proved fatal, in periods varying from six to twenty-four days.

"Most persons residing in the district where the disease first appeared sooner or later had some manifestation of the disease. The period of incubation varied from five to twenty days. The lymphatic glands were in many cases greatly enlarged.

"The first symptom of this disease—and it is one which we have never seen referred to by any writer on the subject—was *pain in the ear*. It was not only pathognomonic, but prominent, and almost invariably present, in every case that came under our observation, for a day or two before the patient made the least complaint in any other respect, and before the smallest point or concretion of lymphatic exudation could be discovered on the tonsils or elsewhere."

The tonsils were enlarged and inflamed, with small points of lymphatic exudation upon them, which gradually spread upwards into the nasal fossæ, and downwards into the larynx and trachea.

There was extreme prostration, depression of the nervous system, feeble pulse, &c., but in no case was there any mental disturbance. There was nothing peculiar in the treatment.

Dr. Beardsley's account is concise, and well drawn up.

In Albany, N. Y., diphtheria assumed an epidemic character in 1858, proving very destructive. Dr. Willard, of that city, in a paper read before the New York State Medical Society, states that it first appeared in April, 1858, although its greatest severity was in the autumn. In a population of about 60,000 there were 167 deaths. Of the whole number only three were adults, the remainder being children, mostly under twelve years of age. The deaths of females were about one-third more than of males. One portion of the city suffered more than another, but no satisfactory connection was traced between the disease and any local cause.

A few cases of diphtheria have also been observed in Boston, Providence, New Bedford, and in several other portions of New England, but there has been no prevailing epidemic of the disease in this section of the country, besides those we have mentioned.

There is reason to believe that the disease may become more firmly established with us, as has been the case in both France and England.

We have thus given some account of the history of the epidemics of "sore throat" which have prevailed in various portions of the world at different periods. On making a comparison, it will be found that all these epidemics possess certain characters in common, although presenting occasional features of difference. If we study them together, it will be also seen that they are closely connected by a bond of union which is to be found in the patho-

logical anatomy of the disease, and which consists in a peculiar exudation. This was clearly recognized by Bretonneau, and is in fact the dominant idea in his memoir upon the subject. Bretonneau was incorrect, however, as we have shown, in bringing together, under the term diphtherite, affections which are separated by wide intervals; he was wrong also in his conception as to the absence in the disease of all constitutional symptoms, as well as regarding the integrity of the subjacent mucous membrane on the removal of the exudation. At least, we can truly say that his views on these points do not coincide with the experience taught us within the last few years. So that, while we give M. Bretonneau the credit of having established these two leading facts—viz., that all the various forms of epidemic sore throat which have prevailed in different parts of the world are identical, and that the characteristic of this identity is the existence of the exudation—we must confess that his description is wanting in many points necessary to a faithful representation of the disease.

What, then, it may be asked, can be considered as a faithful description of the disease? In answer, we subjoin portions of an article by MM. Barthez and Rilliet, contained in their admirable *Traité des Maladies des Enfants*, as also the brief description by Dr. J. Copland in his dictionary.

“The angina described by authors under the name of gangrenous, pseudo-membranous, *couenneuse*, and to which M. Bretonneau has applied the term diphtherite, is a disease which principally attacks children, and the character of which has given rise to numerous discussions. It may occur as a primary disease (the true diphtherite of Bretonneau), and also as a secondary disease, supervening most commonly upon eruptive fevers.

“*Pathological Anatomy.*—The uvula, tonsils, and pharynx are covered by false membranes of greater or less thickness, of a yellow or yellowish-white colour, and sometimes gray. They exhale no fetid smell after death, and are generally very firmly adherent to the subjacent mucous membrane, especially in the pharynx and arch of the palate. The tonsils are rarely covered with a continuous layer, but spotted here and there with patches of various sizes, many of which penetrate into the lacunæ of these organs. In the pharynx the false membrane forms a large plate, a sort of yellow covering to the mucous membrane, sometimes continuous, sometimes disposed in broken or interrupted layers. The false membranes have sometimes a gray colour, which led for some time to the belief that they were the result of gangrene; but the gangrenous aspect of the pharynx is due to the putrid degeneration of the pellicular concretions themselves.

“The exudation of blood, which is not unusual in diphtheritic inflammation, completes the error. The false membrane, coloured by this fluid, successively assumes different tints, marks of its decomposition.

“M. Bretonneau maintains that the mucous membrane subjacent to the exudation for the most part preserves its usual consistence and appearance. ‘Slight ecchymosis, and a trifling amount of erosion upon the surface, in cases where the disease has been of long standing, constitute the chief alterations in the tissues.’ In some cases which have come under our observation we have witnessed much more serious lesions; but, on the other hand, we have not met with those lines of ecchymosis which are described as being always present in the pharynx and upon the velum palati. In two cases under our care the pharynx was deeply ulcerated.

“The tumefaction of the submaxillary glands is a lesion which M. Bretonneau considers as being almost constant. They attain a considerable size, but rarely suppurate.

“*Symptoms, &c.*—Diphtheria commonly sets in with slight febrile symptoms, the strength and appetite not being sensibly affected. The patient complains of a slight pain in the throat; no change in deglutition. Very shortly after the first attack a slight swelling of the tonsils is observed, and frequently a little exudation of false membrane. Soon whitish or yellowish-white spots are seen

on the tonsils, which extend to the larynx, velum palati, and pharynx. Sometimes these are limited to the tonsils and velum palati, when they often lose the white colour, and become of a dirty gray, giving out an extremely fetid odour; an abundance of saliva is at the same time running from the corners of the mouth. The glands of the neck gradually become enlarged.

"At the end of a certain time, according as the membrane is more or less adherent, it commences to separate, and is thrown off. Or, remaining adherent to the mucous surface, it gradually grows thinner, and thus disappears.

"During the course of the disease the appetite not unfrequently remains unimpaired. There is neither diarrhoea nor vomiting. If the disease terminates favourably, there remains only a slight redness about the throat. In the fatal cases the inflammation extends from the fauces to the air-passages, thus giving rise to croup. Occasionally the disease assumes a typhoid character, a condition which has not been observed by M. Bretonneau. When diphtheria runs through its course without complications, it generally lasts from six to nine days; if croup intervenes, it may prove fatal in one or two days."

Dr. Copland, in his dictionary, defines diphtheria as follows:—

"Soreness, pain, and heat in the throat, often increased on deglutition; redness, with an exudation of a buff or gray-coloured lymph in spots at an early stage, commencing either in the fauces, on the tonsils, or pharynx, and quickly extending to these, and often also to the larynx and œsophagus; the exudation becoming more continuous and firm, accompanied with fever, and appearing generally either epidemically or endemically."

Again, if we carefully examine the various epidemics of diphtheria, we shall be able to bring them together under two principal forms of the disease—the mild and the severe.

The mild form is usually preceded by more or less fever, by some loss of appetite, a slight difficulty in deglutition, with, perhaps, some discomfort about the fauces. The tongue presents a thick whitish coat. On examination, at the very outset of the disease, the velum palati, uvula, and pharynx are of a bright red colour. The tonsils are slightly swollen, and are of the same red hue. In a short time, generally from twelve to thirty-six hours after the attack, upon one tonsil, and sometimes upon both, are seen distinct white patches of exudation of false membrane. These soon extend over the uvula and posterior wall of the pharynx. The exudation adheres more or less firmly to the adjacent mucous surface, and cannot be easily removed. In a few cases the exudation remains confined to the tonsils, and neither grows black nor putrefies. The surrounding mucous membrane is swollen and projecting. The parotid and submaxillary glands are not much swollen. The duration of the mild form of the disease is from six to nine or ten days.

In the severe form, the disease is ushered in by intense headache, hot pungent skin, rapid feeble pulse; there is great difficulty in deglutition, and the respiration is much hurried. The tongue is covered with a thick, dirty brownish coat. On examination of the throat, the tonsils are found enormously swollen and covered with a thick ash-coloured membrane, which has also extended to the uvula and to the posterior walls of the pharynx, and not unfrequently gives out a fetid odour. Unless arrested by treatment, all the symptoms increase in severity, the respiration becomes much oppressed, there is a barking cough, and a change in the voice, which becomes hoarse and indistinct; the deglutition becomes so painful that children refuse to swallow even liquids; the saliva dribbles from the corners of the mouth, and an acrid discharge flows from the nares. The glands of the neck are greatly swollen and tender. The patient is restless to an extreme degree, tossing about and then sinking into a semi-comatose condition. These

cases when they prove fatal, as is the general rule, terminate either by rapid prostration of the vital powers or by an extension of the diphtheritic membrane into the air-passages.

Such, then, are the principal features of diphtheria. There are some points, however, as regards its nature, which require to be considered more in detail. First the characteristics of the false membrane itself claim our special attention.

As regards the physical appearances of the false membrane, if closely examined by the unaided sight, it has the character of a fibro-plastic membrane. In the larynx it presents a whiter colour than when it is situated in the fauces, and very much resembles the membrane thrown out in true croup, although it is softer and often soddened by the sanious matter which exudes from beneath and around it.

After very long and careful examination, it has been observed that the exudation was preceded by a sero-mucous transparent liquid, which, in some cases, is very abundant. This liquid once exuded soon takes on more density and a closer adherence to the surface which secretes it, and at certain points becomes a little less transparent, assuming a yellowish tinge. These points soon run together, coalesce, and thus form a very thin pellicle, which may be regarded as the commencement of the false membrane. In fact, this commencement of the false membrane is an act of coagulation, according to M. Empis, which takes place by a precipitation of fibrin independently of any agency of the living tissue. This is to be seen most distinctly in the air-passages, particularly in the larynx and trachea, in which the tubular cast is seldom ever adherent, and is commonly much smaller than the cavity it occupies; its external surface, therefore, being separated by a considerable interval from the mucous membrane.

That coagulation is not determined by the mucous membrane is, in fact, shown by the experience of M. Empis¹ in cases where tracheotomy has been performed upon children. He says—

“At the end of a few hours after the operation of tracheotomy, whatever care might be taken to clear the canula, the instrument was seen to be lined with a layer of whitish concretions, the thickness of which continually increased. These concretions were evidently only the result of the coagulation of the liquid by which the sides of the canula were constantly covered.”

The pellicle thus formed, which we said may be considered as the first degree of the false membrane, is thicker at the centre than at the circumference, and generally may be easily lifted up, although in very small pieces, owing to its friability. Beneath this superficial pellicle, according to M. Empis, there is still an exudation of sero-mucous matter which gradually coalesces with the pellicle already formed, thus producing a false membrane several lines in thickness, and adhering to the subjacent surface very closely.

In many cases the membrane thus formed appears to remain for some time stationary, and then sooner or later it takes on an increase in thickness as well as in extent of surface. The secretion of sanious fluid which embues and softens the concretions is also increased, becomes very dark coloured, and exhales a fetid odour similar to that of gangrene. This especially applies to the deeper portions of the fauces, to the vulva, and to the anterior parts of the vagina.

With regard to the cicatrization of the subjacent surface, and to the disappearance of the false membrane, M. Empis² says :—

¹ Arch. Gén. de Méd., Février, 1850.

² Arch. Gén. de Méd., 1850.

"We never see the membrane disappear all at once, leaving in its place a cicatrized surface, as is the case with an ordinary eschar, but it is by a gradual process that the pellicle diminishes in thickness, in proportion as the edges of the abraded surface cicatrize. If, however, we modify the secreting surface by an energetic local treatment, we can cause the complete disappearance of the membrane, leaving nothing beneath but a granulating surface of a healthy character.

As regards the particular seat of the exudation, it is sometimes situated upon the cutaneous surface, at other times upon the mucous, and not unfrequently upon both at once. Any portion of the external surface of the body may become the seat of a diphtheritic false membrane, the only condition essential being the absence of the epidermis, the skin thereby approximating to the condition of a mucous membrane. This cutaneous diphtheria has been much more prevalent in certain epidemics than in others, especially in France. In some the cutaneous affection has been so frequent as to become the prominent characteristic of the disease. Leech bites, blistered surfaces, excoriations of any part, various wounds, in the progress of an epidemic might become the seat of diphtheritic inflammation. Whatever may be the situation of the exudation, it has been incontestably proved that the diphtheritic affections of the skin are identical in their nature with those which are seated in the mucous membrane of the fauces and larynx. Nor is the external manifestation of the diphtheritic poison in any way less formidable than the faucial. In many cases reported by M. Trousseau, the symptoms of low typhoid were present; they often terminated fatally, or were followed by a long, tedious convalescence.

When a wound is attacked by diphtheritic inflammation, it becomes painful, fetid and discoloured serosity pours from it in abundance, and a gray soft coating soon covers it with a layer of increasing thickness; the edges swell and become violet. The wound remains often obstinately stationary for months; sometimes it spreads; then around it an erysipelatous blush is seen; pustules form, become confluent, burst and leave apparent a diphtheritic patch, which spreads even from the head to the loins.

A curious fact which has been observed as regards the seat of the diphtheritic exudation, is, that although it is found equally in the mouth, on the soft palate, the tonsils, the pharynx, the nasal fossæ, the larynx, trachea, and even in the bronchial tubes, on the conjunctiva, the vulva and anus, and upon the skin, it is not found upon those portions which are removed from the contact of the air; these seem refractory to the extension of the disease. M. Empis¹ remarks, that he never saw true diphtheria extend into the œsophagus, while, on the contrary, the exudation of certain aphthous affections show a great tendency to spread into the œsophagus, but never into the respiratory organs. The atmosphere would thus certainly seem to exert an influence in promoting diphtheritic inflammation. The same observations have been made by M. Isambert,² as well as by others.

In order to determine the value of the exudation as a characteristic of diphtheria, it is well known that M. Bretonneau made numerous experiments relating to the effects produced by the application of irritant substances to the mucous membrane. As the result of these experiments, he found that no substance was capable of producing similar effects excepting cantharides. He says:—

"The action of the oil of cantharides when applied to the surface of the tongue and lips is almost instantaneous. In less than thirty minutes the epi-

¹ Arch. Gén. de Méd.

² Ibid., 1857

dermis shrivels and becomes raised and detached. It is soon replaced by a concrete pellicle, at first thin and semi-transparent, which speedily becomes more opaque and thicker. Like the diphtheritic exudation, this membrane, which is at first slightly adherent, is detached and reproduced with great readiness. Within a period of six or seven days it may be several times renewed."

M. Bretonneau concludes, from these experiments, that the membrane of cantharides is anatomically identical with that of diphtheria, and is, therefore, forced to admit that diseases cannot be distinguished merely by their anatomical characters.¹

"The facts relating to the cantharidic inflammation do not in the slightest degree weaken the specificity of diphtheria; on the contrary, if we consider them in their true light and in their complete development, they prove it experimentally and demonstratively. However close may be the resemblance between the two forms of inflammation, they are distinguished by well-marked characters. The cantharidic inflammation is limited to the surfaces which have been subject to the inflaming action of the vesicant, and soon become extinct; while it is in the nature of the diphtheritic inflammation to extend and to persist."²

Under the microscope the false membrane of diphtheria exhibits the ordinary elements of such structures, although its characters would appear to vary somewhat. The elements usually detected are, chiefly, molecular particles, matted epithelium-cells of all kinds and shapes, pus, and blood-cells. These are arranged in layers, and united so as to form a membranous deposit.

M. Empis has particularly investigated the pathological anatomy of the disease by the aid of the microscope. Now, it is well known that some observers, among whom we may mention Vogel and Dr. Laycock, have associated with the disease the presence of a parasitic fungus which fixes itself on the mucous membrane of the fauces, and is thought to be the starting-point of the vascular condition of which we have spoken, which afterwards gives rise to the exudation. This parasitic fungus is the *oïdium albicans*.

M. Empis, after noticing the statement of Vogel, that the *oïdium albicans* is to be found in the pellicle of diphtheria, says:—

"This author has evidently confounded under the term diphtherite all the pseudo-membranous exudations, without examining into their nature or characters, for this parasite is not found in the true diphtheritic or fibrinous exudations, but only in those of muguet."

He then goes on to compare the diphtheritic exudation with other similar products, such as the buffy coat of the blood, the false membrane of pleurisy, the exudation of blistered surfaces, and that which occurs in the scarlatinal sore throat, and arrives at these conclusions: That it is easy to determine, by means of the microscope—1st, the pseudo-membranous exudations of muguet, which have nothing in common with other false membranes; 2d, the buffy coat of the blood; 3d, the false membrane of pleurisy; but that it is impossible to draw any distinction, founded on microscopic investigation, between the exudation of diphtheria and that of the blistered surface, or that which occurs in the angina of scarlatina.

As the development, then, of this parasitic growth takes place in a variety of other diseases, we must regard it as purely accidental, or at least secondary, and by no means as characteristic, or an exciting cause of the disease under consideration.

¹ *Traité de la Diphthérie*, p. 367.

² Appendix D.

The communication of Dr. Laycock also fails to inspire us with that confidence in his theory which it would perhaps have done, had the case been one of uncomplicated diphtheria.¹ Moreover, he himself also admits that this parasite is discoverable in the patches of aphthæ as well as in the secretions of the mouth in other diseases.

In an excellent communication to the *Medical Times and Gazette*,² Dr. Wilks says:—

“Opinions still vary as to the true nature of diphtheria, and therefore as to its connection with a parasite fungus (*oïdium albicans*). As on several occasions the white film on the throat has been found to consist of this fungus, it has been conjectured whether the malady is not one having a parasitic origin, and the belief has been rendered more probable from the fact that several new diseases have of late prevailed throughout the organic kingdom, both animal and vegetable, which are clearly traceable to parasites—for example, the *oïdium* of the vine. . . .

“My attention being directed to this matter, I took the opportunity to examine the films which occasionally form on the mouths of those sick with various diseases; and on submitting them to the test of the microscope, felt some surprise in witnessing, in all, fungous growths, which I have not been able to distinguish from that of diphtheria.”

After giving a brief history of several cases of different diseases, in all of which either a distinct pellicle or a thick secretion was present, Dr. Wilks says:—

“These facts are sufficient to show that a vegetable fungus may spring up on the buccal mucous membrane in various cases of disease, but requiring probably some previously morbid condition for a nidus. Is it not so in diphtherite? Is the disease, strictly speaking, a malignant sore throat, and the formation of a pellicle an accident, or is the latter an essential part of the affection?”

“In speaking of the parasitic growth found in the above-mentioned instances, we are aware of the objection which can be made—that the fungus of diphtheria is peculiar (supposing it always to be present), and that found in the mouth of other sick persons is in connection with aphthæ, and is another variety. In answer, I can only say that I failed to discover in the above cases any difference, and, moreover, the character of the pellicle, and its rapid extension over the whole mouth, throat, and tongue, was totally unlike ordinary aphthæ.”

Dr. Wade, of Birmingham, considers that there is present in some cases, in or near the exudation of diphtheria, a fungous growth—not, however, the *oïdium albicans*, but the *leptothrix buccalis*, such as is so commonly met with in the mouth and pharynx. Further investigations, however, have shown that the presence of either of these parasitical growths is to be considered as a mere accident, and not as an essential part of the affection.

In conclusion, we may say that the essential character of the diphtheritic exudations, and that which distinguishes them from other forms of exudation, is, “that they have the power of organization, and never become vascular. Hence they never concur in the reparation of tissue, but putrefy on the surface if they be not removed, existing always as a foreign body.”

In the next number of this journal we shall consider the treatment of this disease, and discuss some other points of interest.

¹ See Appendix A.

² October 2, 1858.

The conclusion of Dr. Slade's Prize Essay on DIPHTHERIA will be found as a Supplement to the present number, p. 301.

REVIEWS.

ART. XI.—*On Diseases peculiar to Women, including Displacements of the Uterus.* By HUGH L. HODGE, M. D., Professor of Obstetrics and Diseases of Women and Children in the University of Pennsylvania. "*Nullius addictus jurare in verba magistri.*" With original illustrations. 8vo. pp. 442. Philadelphia: Blanchard & Lea, 1860.

THIS contribution towards the elucidation of the pathology and treatment of some of the diseases peculiar to women, cannot fail to meet with a favourable reception from the medical profession. The character of the particular maladies of which the work before us treats; their frequency, variety, and obscurity; the amount of malaise and even of actual suffering by which they are invariably attended; their obstinacy, the difficulty with which they are overcome, and their disposition again and again to recur—these, taken in connection with the entire competency of the author to render a correct account of their nature, their causes, and their appropriate management—his ample experience, his matured judgment, and his perfect conscientiousness—invest this publication with an interest and value to which few of the medical treatises of a recent date can lay a stronger, if, perchance, an equal claim.

Dr. Hodge, in the preparation of the present work, disclaims any desire either to present "a summary of the labours of others, or to give a critical review of their opinions and practice." His only object is to record, while incidentally alluding to what others have advanced, his own opinions and practice in relation to a class of diseases which have always excited and must continue always to excite the interest of the medical profession; opinions and practice which have been the result mainly of clinical observations made during a laborious practice of many years, nearly thirty of which have been spent as a public teacher of obstetrics and the diseases of women and children. There is no work, perhaps, upon the subject discussed by Dr. Hodge, which approaches nearer to the character of a strictly original treatise than that before us.

The title affixed to the work scarcely conveys a correct idea of its scope. It does not treat of all the diseases which are peculiar to the female sex, but of those only which, the author believes, primarily, and for the most part throughout their entire course, to depend exclusively on simple nervous irritation. That is to say, on an irritation of the proper tissue of the nerves and nervous centres—the functions of organic life being involved in so trifling or secondary a manner that these latter demand little or no attention in either a pathological or practical point of view. These diseases Dr. Hodge attributes, in the great majority of cases at least, to uterine irritation—a state of morbid irritability of the womb and its appendages; and to give a systematic account of this state of the uterine organs, and the nervous disorders which are excited or kept up by it in the neighbouring and remote parts, is the object of the present treatise.

The work is divided into three parts. The first of these is devoted to a

very close investigation of the nature, general and local symptoms, concomitant phenomena, complications, progress, results, causes, diagnosis, and prognosis, of irritable uterus.

In consequence of the very loose manner in which most of the medical terms in common use are too often applied, and the confusion and obscurity thence resulting, Dr. H. has taken care in the very outset to define with precision the several technical terms employed by him.

The initial chapter of the first part of the treatise is devoted to a general examination into the nature of nervous irritation and its consequences. A careful study of this chapter will amply repay the time and attention expended in its accomplishment. The teachings of the author in respect to the pathological character and results of morbid nervous irritation are, it is true, somewhat different from those entertained by pathologists generally. They explain, nevertheless, we feel persuaded, far better the true character, causation, phenomena, progress, and terminations of the entire class of the strictly nervous diseases—diseases which so frequently perplex the physician by their frequency, by their multiplicity and variable character—the severity, often, and alarming appearance of the phenomena by which they are characterized, and by their obstinacy under all the ordinary plans of therapeutic management.

By the irritability of a tissue, Dr. H. understands, simply, “a capability of receiving impressions from surrounding agents, and thus producing its appropriate phenomena.” It is the capability of being acted upon. This impressible and sensitive property, in the normal state of the organism, differs in the different tissues—at the several stages of life, in the two sexes, and in different individuals of each sex. It is possessed by the female in an eminent degree. Women are essentially nervous. Much more so during the period of their menstrual life, from fifteen to forty-five, than they are either before it commences or after it has terminated. Their diseases during the larger portion of their existence are all of them more or less modified by this nervous temperament, while they are often of a purely neurotic character. There is, in many cases, a complete independence of nervous disease on all organic lesions. According to Dr. H., even when complications exist, calling for their appropriate treatment, the neurotic affection is not unfrequently of primary and essential importance, and requires the chief attention of the physician.

“The remark must be made,” we use the words of the author, “although apparently unnecessary, that these neurotic complaints are *physical*; they are, in every way, as much real diseases as those of organic life. They are irritations of the medullary matter of the brain, of the spinal cord, and of their radiations, the nerves; as truly as organic diseases, inflammations, fevers, &c., are irritations of the heart, arteries, capillaries, and organic cells.”

To avoid confusion, Dr. H. assumes that sensibility in the cerebro-spinal system is a synonyme with irritability of the medullary nervous tissue. Hence, nervous irritation may be manifested by sensation, internal or external, of whatever type or character; by muscular contraction, as in motion, or in subsultus tendinum, spasms, convulsions; and also by the disturbance of the mental powers, of “the senses,” and indirectly of the functions of organic life.

“Cerebral irritation,” Dr. H. remarks, “exists, say from moral causes—confusion of ideas, wandering of thoughts, disturbed sensations, spasmodic actions, even convulsions may ensue: this is true cerebral irritation; that is, irritation

of the cerebrum as an organ, as a medullary mass, the centre of the nervous system, and also as the instrument of the mind. To employ the same expression in cases of acute inflammation of the brain, is very common, but manifestly erroneous in theory and in practice. Cerebritis is an irritation of the organic life of the brain, of its bloodvessels, and other organic tissues, and not of medullary nervous matter. Sometimes, as after wounds of the brain, inflammation exists, with few or no evidences of functional disturbance; but generally the functions are disturbed: in which case there is, of course, cerebral or nervous irritation superadded to inflammatory or organic irritation of the tissues. In this case, the chief indication is to subdue the organic, which is the cause of the animal or nervous irritation."

As a consequence of simple nervous irritation of a tissue or organ, Dr. Hodge refers to the form of "simple congestion," as observed in blushing, flushes of heat, crying; in the erections of the penis, nipple, etc.; in the salivary and mammary glands; in the head and brain from excitements of a purely mental or moral character; in the uterus previous to menstruation; in fact, in all the organs, under their normal excitants, when new or increased demands are made on their powers.

"In all such instances," says Dr. H., "there is evidently excitation and a consequent active determination of blood to a part, not only without injury, but generally productive of good, in furnishing the material for the performance of the respective functions of the tissues. Such congestions usually disappear spontaneously, with or without increased secretions. In the erectile tissues, the excitation disappearing, the fulness of the vessels subsides rapidly. Generally, however, the secretion becomes abundant, as in the shedding of tears, the flow of saliva from the mouth, of milk from the breasts, of bile from the liver, of the menses from the uterus. We say, therefore, that such normal excitements and congestions are relieved partly by the disappearance of the excitants, but chiefly by a free secretion. All, however, is normal, physiological and healthy."

Founded on this normal condition of things—excitation and congestion of a physiological character—we have, also, not unfrequently an abnormal state—irritation and engorgement, but without inflammation and its consequences, ensuing. Of the latter form of congestion we have examples in most of those cases, so constantly referred to by authors under the name of apoplexy of the brain and lungs; engorgements or congestions of the lungs, liver, spleen, bowels, uterus, &c. Usually this form of simple, active congestion occurs suddenly, unexpectedly, but often it approaches slowly—insidiously. It sometimes disappears rapidly, but more frequently gradually diminishes as the irritation subsides.

"The consequences of this congestion," according to Dr. H., "are sometimes moderate, and may continue for a long time without serious mischief, but are often terrible and fatal; yet, in all cases they are very different from those of inflammation. This variety in the results depends mainly on the importance of the organ involved, as well as on the activity and intensity of the nervous irritation and congestion."

In cases of simple congestion of the brain, Dr. H. remarks, that there may ensue entire recovery from the complete apoplectic condition or the greater or less disturbance of the cerebral functions induced; sometimes, however, the recovery is tedious and imperfect, especially when the overdistended vessels have given way, and the blood in its escape from them into the medullary portion of the brain has caused a disruption of its fibres; occasionally, death then speedily ensues.

"In the *post-mortem* examination no evidences of inflammatory action are visible. Not unfrequently the anatomist reports all the tissues in a normal condition; generally, however, the veins are distended, and more or less of serous

fluid is found in the cavity of the arachnoid membrane and in the ventricles, but particularly in the subarachnoid space, and in the cellular tissue of the pia mater. Occasionally red globules are commingled with the serum, and, in bad cases, pure blood is effused on the surface, in the cavities, or into the substance of the brain."

Dr. H. sets down effusion as the natural consequence of simple congestion, with a corresponding interruption or suspension of the functions of the organ involved. When the congestion is slight, it may disappear without any, or, at least, trifling effusion, the organ returning to its normal condition on the subsidence of the irritation.

"Where secretory surfaces exist, as in the bronchial, hepatic, and uterine tissues, an abundant secretion," Dr. H. remarks, "of their natural fluids ensues, and as in normal excitements, to the relief of the engorged vessels. Hence, the free secretion of mucus in the lungs, or of bile in the liver, or of the menstrual fluid in the uterus, will relieve moderate congestions of the respective organs. In severe cases of engorgement pure blood is sometimes effused, as in hæmoptysis, hæmatemesis, hæmorrhagia uteri, and so in other exposed surfaces.

"When the distension of the vascular tissue of an organ is for a long time kept up, from the persistence of the cause, it sometimes becomes a stimulus to the organic actions, so that the nutritive functions are more actively developed; the organ grows in size, is hypertrophied, without any real alteration in the character of its tissues. Hence *hypertrophy* is an occasional consequent of chronic nervous congestion, and may remain permanent, but may disappear under favourable circumstances, when the cause is removed. Hence we have one cause of hypertrophy of the heart, spleen, liver, uterus, testes, &c., in which there is none of that effusion of lymph, that indurated condition or disorganized state of the tissues which are the consequences of phlogosis. Simple hypertrophy seldom interferes, materially, with the functions of the enlarged organ. The uterus, during gestation, may be considered as the physiological type of the uterus in a state of chronic congestion.

"Whether the degree of organic excitement may, in cases of acute simple congestion, ever become so intense as to excite *inflammation*, is, and must be, a disputable point, as so many other causes of irritation are often operative. My own opinion is, that simple congestion is never the cause of inflammation. Inflammation is not one of its sequæ."

Dr. Hodge points to the fact that the irritability of a part is almost always inversely as its strength. This, however, is only true of each tissue in reference to its own powers and actions, and not comparatively as it relates to other tissues. Thus, it cannot be affirmed of the cellular tissue that, because its organic life is comparatively feeble, it is therefore more excitable than the skin, with its abundant supply of bloodvessels and nerves.

It is said of a strong man in full health, that he has "no nerves;" that is, it requires powerful impressions to disturb his mental and physical susceptibilities. A woman, on the contrary, is "all nerve;" that is, eminently impressible, sensitive. Minor causes will disturb her mind and body; powerful impressions will produce great agitation of her mental and moral being, and of all the tissues under the domain of the cerebro-spinal system. Should, however, the strong man, from any cause, be rendered weak, debilitated, he also will become "nervous." His mind will be now excitable, and his body keenly susceptible to every impression. So, on the other hand, a course of life or of education adapted to approximate the constitution of the female to that of the robust male, will, in proportion to the degree of such approximation, diminish her irritability, render her less nervous, less sensitive, less impressible.

Dr. H. shows, and we think very conclusively, that the connection between the anæmia, by which, so commonly, general neurotic or hysterical

phenomena are accompanied, is not, as some have supposed, that of cause and effect—and that by curing the former, the latter will cease. They are both, evidently, a common consequence of the same cause—debility. Loss of power is attended by increased irritability—nervousness—as its consequence. When loss of power occurs in the brain and spinal marrow, organic life suffers, chiefly from the consequent deprivation of fresh air and exercise; hence, the want of appetite and weakened powers of digestion, of hæmatisation, of nutrition, and from these, of course, anæmia. If strength can be restored by iron, fresh air, exercise, &c., both the nervousness and the anæmia will be relieved by the removal of their common cause.

In the summary of general conclusions, deducible from the teachings in this introductory chapter to the section devoted to the consideration of irritable uterus and its consequences, Dr. Hodge remarks—

“The hysterical or the neurotic diseases of women, are therefore states of irritation of the cerebro-spinal nervous system, in whole or in part. They are very evanescent when the cause is transitory, as in nervous affections from moral causes, such as fear, anger, anxiety, joy, grief; or, from physical causes, as from indigestion, flatulence, &c., but very persistent in all cases where the cause remains operative, and will then often defy the best directed remedial agents for months and years. Hence the indomitable character of various nervous or neuralgic diseases; the cause is persistent, perhaps it has not been detected, or cannot be removed. Tonics, nutritious diet, exercise, travelling, as well as antispasmodics, narcotics, and stimuli, prove useless, or perhaps worse than useless, and at best but temporary palliations. The symptoms will return, often with increased vehemence. The location of the cause may be in any tissue or organ of the body. Wherever the irritability or sensibility of a part is disturbed, thence, as from a focus, may radiate nervous excitements. They are usually perceived in the nerves involved—first, at the point irritated, then, if sufficiently severe, in the sentient extremities of such nerves, or in the reverse direction towards their origin in the spinal marrow, or the brain; and thence again, by reflex influences, in any tissues of the economy, thus causing a disturbance of the functions of such tissues or organs, so far as these functions are dependent on nervous influences. In other words, ‘nervous irritations’ of the brain and spinal axis, and of their dependencies, may, and often do, arise from any local irritation. It is often of small consequence what may be the character of such local irritation, whether it be simply nervous or inflammatory, sympathetic nervous irritations may result in different and often in very distant portions of the animal economy.”

Dr. H. applies the term “irritable” to a morbid condition of the uterus, in which it is “more sensitive, more easily excited, than in a healthy state.” The abnormal condition is one which has reference alone to the nervous system—the animal life of the organ. It is a state, not of organic irritation, but of nervous irritation. In simple uncomplicated cases, the alteration of the sensibility is, in fact, the only indication of its existence. There is, necessarily, no turgescence of the uterus; no congestion, active or passive; no inflammation, and of course, no alteration of structure, even after the lapse of years. Congestions and inflammations, when detected, are complications, sometimes perhaps the result of the nervous excitement of the organ, but more generally of adventitious and secondary, or accidental causes. Occasionally, an irritability of the uterus is connected with inflammation of its tissues; generally, however, as a concomitant lesion, the two very seldom holding to each other the relation of cause and effect.

“An irritable uterus,” says Dr. H., “is that state of the nerves of the organ in which they are preternaturally susceptible to impressions. This is all. There may be, and often is, no perceptible disturbance of the organic life. It is merely a disease of the animal life; often, however, when a severe or continued irritant

disturbs this irritability, the irritation will be followed by congestion or engorgement. This is not inflammatory congestion, but that modification of congestion resulting from nervous excitements, as in menstruation, erections, &c. This congestion may be the cause of leucorrhœa, menorrhagia, and hæmorrhagia. Sometimes, when long persistent, and especially when no secretions or effusions ensue for its relief, congestion is followed by increased growth or development of the organ, that is, by hypertrophy, but never by inflammation, by induration, ulceration, or other results of inflammatory action. Hence, there is no permanent alteration of structure; even after the lapse of many years the recovery is perfect, and often sudden. Of course, inflammation is often coexistent with an irritable uterus, but it arises from other and extraneous causes."

A most excellent and closely analytical history of the symptoms of irritable uterus is given in the succeeding three chapters. The local symptoms are first examined. These consist of pain and various distressing sensations, aggravated by motion, sneezing, coughing, vomiting, and the natural efforts to empty the bladder, or in defecation, &c., and by menstruation, producing the simple uncomplicated or neuralgic form of dysmenorrhœa.

Vaginal examination is in general painful, in cases of irritable uterus, sometimes intensely so, more especially if by the finger the slightest pressure be made upon the uterus. The same is true, also, of examinations *per rectum*. By a vaginal examination, in all the uncomplicated cases of irritable uterus, however severe, and even though they may have lasted for years, it will be found that there is present none of the usual consequences of inflammation, nor any change of tissue. All the parts explored by the finger will feel perfectly natural as to softness, pliability, form, and size, as if no morbid state had existed. An ocular inspection, by means of the speculum, confirms these observations. With the exception of the pain and the temporary organic excitement and congestion, due to the increased irritation of the tissues consequent upon the use of the speculum, which sometimes occur, in very many cases the instrument reveals no turgescence of the mucous membrane, no swelling of the cervix uteri, no patulous condition of the os uteri, no increase of color, no granular or ulcerated condition of the neck or of the lips of the organ.

Irritation of the nerves of sensation and of motion may, remarks Dr. H., and often does exist without congestion; but, such is not always the case. Thus, in cases of irritable uterus, the neck of the organ will often be found swollen, the lips of the os divergent, evincing a tumefaction of the reflected membrane of the vagina as it becomes uterine. In some cases there is evidently a fulness or turgescence of the entire organ. It varies very much, even in the same patient, being greater during the catamenia, after a long walk, after coition, or, indeed, after any physical or moral excitement. When the patient has been long at rest, or free from excitement, the intumescence can scarcely be recognized. Similar tumefaction may often be detected at the orifice of the urethra, in its caruncle, in the arborescent vaginal surface below the urethra, and throughout this vulvo-uterine canal. By the speculum will be discovered a deeper colour of the tissues—a red or scarlet, sometimes purplish colour of the mucous membrane of the vulva, vagina, cervix, and especially of the os tinæ.

Engorgement may exist without the occurrence of organic irritation, and its consequent inflammation. When, however, it is very considerable or long continued, then, according to Dr. H., the organic actions sometimes become excited normally, but not abnormally irritated. This is manifested *first*, and most frequently, by an increase of the natural secretions, giving rise to simple functional leucorrhœa; *secondly*, in many cases where the congestion is considerable, and especially if it occur suddenly, by effusions of

blood, menorrhagia, hæmorrhagia uteri; and, *thirdly*, when neither secretion nor effusion occurs, or only partial relief is afforded by them, and the congestion continues, the uterus is more fully nourished, it becomes developed, enlarged, and hypertrophied.

Each of these consequences of "irritable uterus" are separately considered, and their entire independence of organic irritation, and consequent inflammation, very clearly pointed out.

Leucorrhœa, according to Dr. H., in cases of congestion from irritable uterus, is to be viewed as an imperfect menstruation. The fluid is not fully elaborated. It is a white menses, constituting the "*menstruatio alba*" of old authors, and should certainly be distinguished from all those divers discharges which are the result of inflammation, cancer, &c. Judging from his own observations, Dr. H. would infer that leucorrhœa is far more frequently uterine than vaginal, and functional than inflammatory, an opinion altogether at variance with that sanctioned by some of the most eminent of modern pathologists.

Besides the simple uncomplicated form of dysmenorrhœa which, according to Dr. H., occurs as a symptom or rather consequence of irritable uterus, he describes three other forms. First, the *congestive* form. This is merely an aggravation of the more moderate attacks of dysmenorrhœa. There is a greater degree of congestion, more intense nervous symptoms; severe sympathetic disturbances of the brain, spinal marrow, stomach, and bowels. These violent and apparently alarming phenomena are, however, usually of short duration, diminishing or ceasing ordinarily within twenty-four hours, sometimes within two or three hours. A free secretion from the cavity of the uterus, moderates at once the engorgement and the neuralgic pains.

Second, *mechanical dysmenorrhœa*. Caused by obstructions of the canal of the cervix uteri, as the presence of coagula, inspissated mucus, lymph, or of a membrane in the cavity of the body or neck of the uterus; by turgescence, or thickening of the lining membrane of the canal from inflammation; by strictures, or by flexion of the cervix uteri, which Dr. H. believes to be, probably, by far the most frequent cause of obstruction.

Third, *membranous dysmenorrhœa*. Caused by the formation of a deciduous membrane as in pregnancy, and its discharge, either entire, or in fragments at the menstrual period. Dr. H. denies that this membrane is the result of inflammatory action. It differs from lymph, he remarks, in all its characters, and would seem to be due to a high degree of nervous irritation, accompanied with congestion. A kind of hypertrophy of the lining membrane of the uterus, so that epithelial cells and scales are rapidly generated, and cast off occasionally in a membranous form. The extrusion of this membrane, aggravating the pain and spasm of the uterus, constitutes an example of mechanical dysmenorrhœa.

Dr. H. notices next, the local symptoms of irritable uterus, when complicated with inflammation of the womb—endometritis or metritis, partial or complete, or with tumours, &c.

"Judging from what he has seen, the conclusion seems to him to be inevitable that the great error in the practice of many is in not drawing the proper distinction between the varieties of uterine congestion; between those symptoms depending on nervous, and those on organic irritation; between an irritable and an inflamed tissue; in magnifying the importance of the latter, and paying little heed to the former and more common form of uterine trouble. Although more common and far more painful, yet it is less dangerous, very seldom fatal; and, ultimately, recoveries, especially after the grand climacteric of woman's life, may generally be anticipated.

"Inflammation, nevertheless, is often the cause of nervous irritation of the uterus: and in many cases, therefore, the latter will entirely disappear under the antiphlogistic treatment, and the recovery be complete. It is contended, however, that in a great number of even such cases, recovery will be facilitated by attending to the more purely nervous symptoms; that such nervous affections, in many cases, demand the chief attention; that in a very large number of supposed cases of inflammation, they demand primary and exclusive treatment; and, finally, that in many cases, after every evidence of inflammatory action has disappeared for weeks, months, or years, and where phlogosis had been the original complaint, the patient's urgent and distressing affections continue, and are often increased in severity from a neglect or mismanagement of *nervous irritation*."

Chapter sixth of Part I. is devoted to a consideration of the general symptoms of irritable uterus, the reflex influences of cerebral and spinal irritation. Under this head are included that extensive class of morbid nervous phenomena, known as the neurotic affections of the female sex, as exhibited, particularly, in the varying forms of hysteria: Phenomena, as various as are the morbid conditions into which the functions of the several portions of the human organism have been brought. Pains in the small of the back, in the cervical, dorsal, or sacral region; pain or distress at the top of the head, in the occipital or in the frontal regions; soreness of the scalp; languor, lassitude, and inertness; irritations along particular nerves, as the crural, obturator, and sciatic; neuralgic pains of the iliac, lumbar, and hypochondriac regions; hyperæsthesia of the skin. These symptoms are often confined to one side of the body. To these may be added cephalalgia, convulsions, catalepsy, delirium, sudden loss of consciousness and of muscular power; intellectual and moral disturbances, as, depression of mind, excitability and versatility of temper; anticipations of impending evil; hallucinations and spectral illusions; spasmodic affections of the œsophagus, larynx, lungs, heart, diaphragm, &c.; disturbances of the respiratory function, giving rise to asthma, cough, aphonia; palpitation, and irregular action of the heart; soreness, and partial or general hypertrophy of the mammæ; neuralgia of the abdomen; dyspepsia, flatulence, tympanites, constipation; inordinate secretion and discharge of urine, &c. In short, phenomena, the occurrence of which in different cases, or at different periods in the same case, indicate an affection purely nervous, dependent upon a purely local nervous irritation, but cause it, nevertheless, to simulate some of the most alarming and unmanageable organic affections to which the human organism is liable.

In considering the progress and results of irritable uterus, Dr. H. shows that, as the disease advances, the local disease, instead of remaining confined to the uterus, extends to the adjacent organs—the rectum, the vagina, and the bladder—involving in all cases, directly or indirectly, the sacral nerves. These often become so morbidly sensitive, that the least pressure upon them from a displaced uterus, or feculent matters in the rectum, will give intense pain, radiating towards their sentient extremities, or in the reverse direction, towards their centres or origins in the spinal marrow or brain.

The account given by Dr. H. of irritable rectum, irritable vulva and vagina, and irritable bladder and urethra, are very excellent. The condition of the rectum, vulva, vagina, bladder, and urethra, described by him as one of morbid irritability, whatever disputes may exist as to its true nature, causes, and relations, must have been repeatedly met with in practice by every practitioner. The sufferings to which it in general gives rise are of the most intense character, and are often intractable under all the ordinary plans of treatment.

In summing up his account of the progress and terminations of irritable uterus and its associated nervous affections, Dr. H. remarks:—

“Locally, a sensitive uterus may remain unchanged even for years at a time, and the irritability may eventually disappear, under the ever-varying changes in the nervous sensibility of the patient, as she gradually becomes less sensitive to local irritations. This more frequently occurs as the patient advances in years, especially when the menses cease to appear and the phenomena of age advance. To this period of life many unfortunates anxiously look for relief to their sufferings, and not unfrequently their hopes are realized. Yet in many cases I have known irritable diseases to continue, and sometimes actually to be generated, after this ‘change of life.’

“In mild cases remedial measures may greatly assist in blunting the morbid sensibility, so that the patients may become comparatively comfortable even while the cause remains more or less operative. The idea is exemplified by the fact, often mentioned, that a degree of local irritation is often easily tolerated by the strong and robust, which would torment the delicate and nervous. In more severe cases the suffering of the patient may continue for many years, unless the cause be removed, without any alteration of tissue, or any manifest enlargement or swelling. In other cases there is manifest enlargement. The organ is developed, hypertrophied; this is the result of congestion and increased nutrition, without any inflammation, and no proper alteration of tissue. Not unfrequently there are leucorrhœal and menorrhagic discharges, due to the congestion. There seems to be no evidence that alteration of structure, or any disorganization, ever results from mere nervous irritability of an organ, however long it may exist. It may be complicated, however, with inflammation; then disorganization may, of course, ensue: but this is accidental, and not essential.” “The natural tendencies of the complaint are eventually to perfect health. When the cause has been removed, recoveries are very rapid; so, also, as the nervous system becomes more insensible to irritations, any local irritability soon disappears. The same is true of the spinal and cerebral irritations, and their reflex influences. They disappear rapidly, the local disease being cured; or they may be diminished or destroyed in some cases, after the lapse of years, by the natural changes in the animal economy.

“The only exceptions to these remarks are those unfortunate individuals who, from want of fresh and pure air, of exercise, and good food, or from improper medical treatment, and mental or moral causes due to morbid hereditary predispositions (such as scrofula, tuberculosis, or other cachectic diseases), are liable to secondary complaints, which may, indeed, prove serious and destructive. So also those who, while they are suffering from irritable affections, have inflammatory or febrile diseases superadded to their old complaints. These complications, arising from their usual causes, are prone to their own terminations.”

We come next to a consideration of the predisposing and exciting causes of diseases of irritation. Among the first set of causes, Dr. H. enumerates the nervous temperament; which may be hereditary, or it may be acquired in consequence of bad physical education, inordinate mental excitements, luxurious indulgences, intemperance, various sources of debility, too early marriage, etc.; the rheumatic or gouty diathesis; the parturient state; over-lactation, &c.; displacements of the uterus, whether in the single or married woman; and the exhausting influences of a warm climate.

The predisposition to irritable affections laid by a strongly marked nervous temperament is strongly insisted upon by Dr. H. In those endowed with such a temperament, whether hereditary or acquired, severe and urgent symptoms are liable to result from trifling causes; while in such as have not this predisposition no decided suffering will be experienced even from well-marked local irritation. In vigorous and insusceptible women, displacements of the uterus, we are told, will often exist for a long time with apparent impunity. Females, Dr. H. remarks, may often recover, in consequence of their strength becoming restored, and their morbid irritability,

in this way, reduced, even while a local irritation of moderate character continues.

Among the exciting causes of irritable uterus, Dr. H. enumerates inflammatory congestion; parturition; over-lactation; excessive venereal enjoyment; mental and moral excitements; voluptuous imaginings, however excited; prolonged study; cold, especially the disturbance produced by it in the functions of the uterus, &c. Great and sudden muscular efforts, whether in walking, climbing, dancing, running, jumping, lifting, jolting on horseback or in a vehicle, sneezing, coughing, vomiting, straining at stool, &c. These physical efforts, according to Dr. H., are chiefly deleterious by causing a displacement of the uterus. This accident, however induced, and whether with or without a predisposition to nervous irritation, becomes, we are told, a most prolific cause of uterine irritability. Even when other causes are operative, the symptoms are often enhanced and kept up by uterine displacement; indeed, they will not unfrequently all disappear, even when other supposed mal-influences exist, provided the displacement be relieved.

Pressure upon the uterus, from improper articles or manner of dressing, or from internal causes, as the presence of tympanites, ascites, enlarged liver, spleen, ovaries, feculent accumulations in the bowels, etc., is set down by Dr. H. as a direct source of irritation of the pelvic viscera or of displacement of the womb.

All the chronic affections of the uterus not only excite the organic and animal actions of the organ, but are liable to cause its displacement, which will be followed by irritation, congestion, leucorrhœa, menorrhagia, hemorrhage, &c.

Obstructions, partial or complete, of the cervix uteri, Dr. H. enumerates as also among the exciting causes of irritation of the organ. These, by preventing the ready outlet of the mucoid or menstrual fluid from its cavity, give rise to irritation and painful contractions, the periodical recurrence of which will render the tissues of the organ preternaturally sensitive. Finally, the reflex influence of irritations or disease of other organs, as of the ovaries, bladder, clitoris, vulva, rectum, stomach, mammæ, &c., are enumerated likewise among the efficient exciting causes of irritable uterus.

In reply to the question, why nervous affections of the uterus are so much more common and distressing now than they would appear to have been formerly, Dr. H. remarks:—

“The nervous temperament of women of the present age has been greatly developed by the wonderful increase of the indulgences and luxuries of modern life. The physical education of the girl has been most carelessly and thoughtlessly disregarded; while every stimulus has been applied to procure a precocious development of the mind, the heart, and the passions. The organic life has been neglected, while the animal has been unduly and too rapidly excited. Another answer to the query is, that the tight dresses, the weight of garments, the braces, &c., to which girls are subjected, are more constantly resorted to now, and are of a more decided character than those employed by their ancestors.

“Perhaps it should be added that the greater frequency of uterine diseases is more apparent than real. Formerly these complaints passed under other names, as weakness of the general system, nervousness, rheumatism, gout, affections of the head, heart, lungs, stomach, liver, or even of the bowels. Patients were regarded as complaining, as addicted to secret vices, to intemperance in alcohol or opium. Even of late years, neuralgia, spasms, and especially that most convenient word, ‘spinal irritation,’ have received the credit of all the varieties of uterine affection. The uterus, as a source of spinal and cerebral irritation, of neuralgia, spasms, and convulsions, has been, and perhaps still is, too often ignored.”

"All ages are liable to irritable diseases, but the uterus is very seldom involved until the occurrence of puberty. * * * It is during the menstrual life of a woman that such complaints most generally occur. A strong predisposition is seen as soon as this most wonderful change begins in the economy; for then not only the ovaries and the uterus are most rapidly developed to their perfect organization, but analogous changes occur in all the tissues and organs of the body. It is when the brain, as the organ of the mental and moral being, as well as the source and centre of all nervous influences, attains the full development of its structure and its functions, that the peculiar irritability and sensibility of the woman are fully developed, and the predisposition for neurotic complaints becomes most marked. * * * When the menstrual life terminates, the ovaries and the uterus rapidly lose their influence, the organic actions often become more active, and the cerebro-spinal system loses its sensibility to a greater or less degree, so that the hysterical forms of complaint are far less frequent. Nevertheless, irritable diseases sometimes originate after the disappearance of the menses, and when they existed previously, are often continued, even severely, for years; still, it is a general truth that neurotic diseases disappear at fifty years of age."

In respect to the treatment of irritable uterus, the first indication laid down by Dr. H. is to remove or palliate the cause by which it has been produced. Without the removal of the cause, all other treatment he considers to be but palliative, in the large majority of cases; while, on the other hand, the removal of the cause, whenever it can be effected, is, in most instances, all that is required for the complete recovery of the patient; to enable her to pass promptly from extreme suffering and apparently most serious disease to perfect comfort and health. Unfortunately, the cause cannot always be detected, or, when detected, may be irremovable. In the first case the morbid phenomena admit of no relief; in the second, the furthest we can hope for is to be able to palliate the sufferings of the patient. Occasionally the causes are of a transitory character, and with the cessation of their action the patient becomes spontaneously relieved.

Uterine congestion, when it acts as an aggravating cause of existing irritability, Dr. H. recommends to be reduced by a few cups or leeches to the sacrum, hypogastrium, perineum, or labia, aided by laxatives, diaphoretics, and especially by warm hip-baths, fomentations, and poultices. When *acute inflammation* is the cause of irritability, as is sometimes the case, it is to be removed by the usual antiphlogistic treatment conjoined with perfect rest. *Chronic inflammation* is the most frequently found conjoined with irritable uterus. It has, hence, with its sequela, ulceration, been considered by some of the leading pathologists of the day the common, almost universal cause of nervous, neuralgic, and spasmodic affections. Dr. H. denounces this opinion as fundamentally erroneous, and the practice based upon it as decidedly mischievous. In the greater number of cases, he maintains, the nervous symptoms have an origin altogether independent of the presence of the inflammation. The existence of both the nervous irritation and the organic inflammatory irritation is, in his opinion, due to a common cause—displacement of the uterus. In all cases, however, of irritable uterus, he considers that chronic inflammation, when present, is to be viewed as a source of aggravation, and hence requires a suitable treatment to procure its abatement or removal. Such a treatment will not be a purely antiphlogistic one, but in the vast majority of cases the main dependence should be placed on rest and remedies adapted for an irritable rather than an inflamed organ. In these chronic cases, Dr. H. remarks, perseverance in a course of antiphlogistic remedies, whether local or general, will prove not merely nugatory, but highly detrimental, and even dangerous. In reference to the modern much-abused local plan pursued for the cure of

inflammations and ulcerations, real or imaginary, of the os and cervix uteri, he remarks:—

“Is it conceivable, on any acknowledged principles, or on any results of experience of the wise and prudent surgeon, that if a non-specific inflammation or ulceration—the cause being removed—will not disappear under the influence of mild and soothing measures, assisted by solutions of the nitrate of silver, and occasionally by the judicious and transient application of the solid nitrate, any good can be anticipated from repeated and prolonged burnings from the lunar caustic, or, if this fail, from the mineral acids, the acid nitrate of mercury, the chloride of zinc, the potassa pura, the potassa cum calce, or the actual cautery? Will not all and each of these articles produce a more intense inflammation, a deeper ulceration, than the original inflammation, however intense or however prolonged?”

“That patients will not only survive such practice, but occasionally recover, is by no means wonderful to the scientific and experienced accoucheur. The uterus itself is not a vital organ. All its inflammations, all its hypertrophies, its indurations, its tumours, when not specific, may be often tolerated for a long life, and may not prove the cause of death. The severe contusions, lacerations, and the consequent inflammations, ulcerations, and even gangrene, resulting in many cases of difficult labour, seldom destroy life. Even specific diseases, the phagedenic and cancerous ulcers, with their fungous growths, their profuse sanious, fetid and hemorrhagic discharges, often continue for months and years, before relief is found in the grave. It is, therefore, altogether credible that, however severe the inflammation, ulceration, and sloughing produced by caustics, nevertheless, as soon as the practitioner shall remit his so-called remedial applications, the inflammation will subside, the sloughs will separate, granulations form, and cicatrization ensue, and the patient be assured of her recovery. May not the question be, however, seriously and conscientiously propounded, whether in all cases of inflammation, with or without ulceration of the mucous membrane of the cervix uteri, all this treatment, even if successful eventually, is not only unnecessary, but positively an aggravation and prolongation of the sufferings, corporeal and mental. Every one must give their testimony from their own experience. The writer gives his positively. Such inflammations are as curable as those of the rectum, the mouth, or the eye, and by remedies as mild, as soothing, and as effectual in the one case as in the other. By the heroic plan more suffering is induced, more time is required, and more injury results to the general health from the greater suffering and prolonged confinement; very frequently, as intimated, no relief is afforded, and in some instances the recoveries, if they can be so named, are followed by partial occlusion of the os uteri.”

In cases where simple *hypertrophy* of the uterus is present, Dr. H. deprecates, in the strongest terms, the use of caustic applications. He recommends the quieting of irritation; the promotion of the secretions by the warm bath and fomentations; warm mucilaginous injections per vaginam; emollient ointments; applications of conium, belladonna, and other narcotics; mercury, iodine, and other resolvers.

In cases of *uterine induration*, Dr. H. maintains that all the local and general symptoms which ensue are due to displacement of the organ, and not, as generally supposed, to congestion and inflammation. Hence, it is not the antiphlogistic or the cauterizing treatment which is demanded, but suitable measures for removing and preventing displacement. The feeling of pressure, of soreness, and of pain may thus be dissipated, and the way prepared for the employment, with far more rational hope of success, of the usual resolvers—iodine, nitrate of silver, &c. The general conclusions of Dr. H. are:—

“1st. In all cases of acute inflammation, perfect rest in bed with appropriate antiphlogistic remedies should be exclusively relied upon.

“2d. In chronic inflammations, with nervous, neuralgic, and spasmodic symp-

toms, while constant attention should be paid to the phlogosis, the main business of the practitioner has respect to the nervous irritation. This usually has a distinct origin, and is, thus far, independent of the inflammation. The latter may be, and often is, an aggravating, but not the essential cause. It may be removed and the nervous irritation will continue in all intensity.

"3d. The persistence in the antiphlogistic treatment, especially by heroic remedies, powerful stimulating and caustic revulsions, is founded on a wrong principle and is calculated to increase, rather than to diminish the morbid irritability of the tissues.

"4th. By a careful attention to the various predisposing and exciting causes of inflammation, and by mild and soothing measures, more will be accomplished, than by a resort to powerful and destructive escharotics.

"5th. Hypertrophied and indurated enlargements of the uterus, are usually injurious merely from their size and weight. Hence patients at rest are often perfectly comfortable, but in motion, become tormented by the symptoms of displaced and irritable uterus.

"6th. And therefore, suitable pessaries, not caustics, are generally the proper means for palliation, and even for radical recovery."

When symptoms of irritable uterus are connected with *rheumatic, gouty, or other irritations of a metastatic character*, the proper treatment of these affections will be required. In chronic cases, however, we should bear in mind the fact that the symptoms referred to gout or rheumatism are in many cases the result of displacements or of some other irritating cause.

The predisposing influences of *gestation* are to be counteracted by perfect rest, the avoidance of all sources of irritation to the uterus, and the judicious resort to soothing and anodyne treatment.

The effect of *lactation* is to be carefully watched, and it is to be promptly interdicted when symptoms of exhaustion and nervous irritability become developed in the mother, even when she has a large supply of milk.

The laws of strict *temperance in eating and drinking*, and of *moderation in the indulgence of all the pleasures of sense*, must be strictly enforced to prevent irritable and nervous disease. The same remark applies also to excitement of the mind—*over-stimulation* of the mental powers, especially in the young, is set down by Dr. H. as a most efficient cause of nervous irritability and the host of sufferings incident to it.

The *morbid influence of cold* must be carefully counteracted by suitable clothing, &c. *Displacements of the uterus* must if possible be prevented, by guarding the patient against all sudden and inordinate muscular efforts, against all improper pressure upon the abdomen from without, and all sources of internal pressure and weight, as tympanites, ascites, feculent accumulations, mesenteric, ovarian, uterine, and other abdominal tumours. When present, displacements of the uterus are to be rectified and their return prevented.

The foregoing measures have for their object the prevention of debility and of morbid irritability; when this latter is actually present, the indication to be fulfilled is to diminish or destroy it. Taking for granted that the cause is no longer operative—that there is no inflammation; no rheumatic or gouty irritation; no ovarian, rectal, or vesical irritation; no sympathetic disturbance of the uterus from other organs; no displacements from simple relaxation or elongation of the ligaments, from hypertrophy or induration of the uterus, or from the presence of internal and external tumors; the morbidly sensitive condition of the patient which continues is to be moderated, according to Dr. H., by direct or indirect measures.

The former includes all those remedial agents which have a direct sedative or anodyne influence on the nervous system, particularly the antispas-

modic and narcotic medicines so useful as temporary means in assuaging pain and moderating disagreeable nervous sensations. They are only valuable, however, as palliatives, seldom as permanent remedies. In common with alcohol, which, when combined with hot water, is in many cases a most important adjuvant to our other antispasmodic remedies, their prolonged habitual use is attended with many serious dangers, among not the least to be dreaded of which is, the rendering the patient more nervous, and thus increasing her susceptibility to morbid impressions.

The *second*, or indirect mode of treatment, is, according to Dr. H., far more efficacious, although less immediately advantageous. It is based on the aphorism, that to impart strength and vital vigor to the organism is the surest means of rendering it less excitable and less irritable. And it is upon a judicious and well conducted course of hygiene that the physician is to depend in his efforts to improve the strength and vigor of his patients. He is to regulate their diet—to overcome costiveness by the use of simple enemata or some tonic laxative; by restricting them to pure water as a beverage; enforcing upon them the use of baths of a proper temperature for the purpose of cleanliness, and for maintaining the proper softness and pliability of the skin, with its natural secretions and exhalations. While the effect of the warm bath is considered by Dr. H. to be, in individuals of a peculiar temperament, invigorating, if not continued too long, yet, he regards it generally as a means only of temporary relief. Local warm baths are recommended by him as valuable remedies to equalize the circulation, soothe irritations, moderate soreness, pains and spasms, and promote secretions. Hence the good effects often experienced from the use of pediluvia, hip-baths, fomentations, poultices, and the injection of warm water or mucilages into the rectum and vagina.

After acute and urgent symptoms have been palliated, Dr. H. is in favour of tepid, cool, or cold bathing, according to the temperament of the individual. Cold, he thinks, really invigorates; it lessens or even destroys morbid excitability. It diminishes organic actions or excitements for the moment, but when reaction ensues, there is almost invariably an increase of strength and vigor and a consequent diminution of sensibility. The cold bath is employed by Dr. H. as either a plunge or shower bath, as a local bath, a douche, or sitz-bath. Injections of cold water into the vagina are also recommended, and, in some cases, he believes the system of packing, either of the entire body or locally of the pelvis, &c., to act beneficially.

The necessity of a proper alternation of exercise and rest, and of an adequate amount of both, are points strongly insisted upon by Dr. H. The exercise should, when the patient is very weak and readily exhausted, be at first gentle and strictly passive—in some cases it must be restricted to frictions of the surface, change of position, or standing for a short time by the assistance of the nurse, a chair, or crutches. Walking, sailing, riding in a vehicle or on horseback, etc., are all of them modes of exercise suited to the case of irritable females, according as they are best borne, etc. In respect to all of them, short and frequent efforts should be first made, always ceasing if great fatigue or much pain is experienced. The early part of the day is that best adapted for exercise.

Fresh and pure air, both in and out of doors, is an absolute requisite for the due sustenance of animal life, and equally as essential for the restoration of the animal organism to activity and vigor.

Tonics, Dr. H. enumerates as valuable adjuvants to the hygienic measures—not always by directly imparting strength, but rather, indirectly, by ex-

citing an appetite—increasing the activity of the digestive function, and promoting hæmatisis. He sets down the simple combination of “the proto-carbonate of iron, with the extract of gentian or of cinchona,” as one equally efficient with the apparently more scientific and complicated preparations in vogue with some practitioners.

In regard to medicinal agents in cases of irritable disease in the female, these, according to Dr. H., differ according as the cases are acute or chronic.

In acute cases, in addition to perfect rest, the loss of blood may be demanded to reduce general plethora or local congestion. It may be by venesection or by cups or leeches to the sacrum, abdomen, hypogastric or iliac regions, inside of the thighs or pudendum. To sinapisms, blisters, local irritants, and rubefacients, Dr. H. is opposed, believing that they more generally do rather harm than good. Narcotics are demanded in acute attacks of pain and spasm, either given by the mouth, or applied locally in the form of hot poultices, frictions with oil and laudanum, or camphor, the application of belladonna, aconite, and chloroform; vaginal injections of laudanum, morphia, and camphor, rectal anodyne enemata, etc.

In the chronic cases of irritable uterus, occasional paroxysms of suffering will require rest, fomentations, and anodynes. Loss of blood will rarely if ever be proper. In these cases it is that the general and local application of warm water will be found both palliative and soothing. The warm may be followed or even alternated with the application of cold water as a douche to the back, loins, and sacrum; by sponging to the abdomen, thighs, and pudenda; as a hip or sitz-bath; as an injection into the vagina or rectum, or by packing, particularly to the abdomen, or around the hips.

In respect to the efficacy of the direct application of narcotic substances to the neck and cavity of the uterus, Dr. H. gives no decided opinion. Their application to the vagina he has found to produce temporary relief. He prefers their introduction in the form of suppository with the butter of cocoa.

In pure cases of irritable uterus, Dr. H. has known much temporary benefit result from the application of lunar caustic to the cervix and os uteri, by its diminishing morbid sensitiveness, but seldom any permanent relief. On the principle of blunting the sensibility of tissues by mechanical means, bougies have been introduced into the neck and body of the uterus. In general this excites so much pain and irritation, even after repeated trials, as to oblige its relinquishment. In a few cases Dr. H. has known it to be successful.

The treatment of the complications of irritable disturbances is next considered. Dysmenorrhœa is to be obviated, during the interval, by removing if possible its cause; and during the paroxysm by palliating suffering by rest, a dose of castor oil or a purgative enema; a hot pediluvium, hip, or general bath; poultices and fomentations to the hypogastrium; hot applications to the feet, warm mucilaginous injections into the vagina, and the administration of hot drinks and diaphoretics. To allay pain opiates are to be given—camphor and opium, cannabis indica, aconita, veratria, and the inhalation of ether or chloroform. These are, however, never to be used, excepting as temporary remedies.

Menorrhagia is to be treated by removing the local irritation, whatever that may be, and by moderating the discharge. Tonics and even stimulants may be demanded; internal stimulants are, however, seldom of any advantage. The *secale cornutum* is in general useful. Locally, while the warmth of the extremities is kept up by artificial means, cold may be applied to the hypogastrium, cold water injected into the rectum and vagina, and astringent washes applied to the vagina and cervix uteri. The tampon

alone or saturated with astringent solutions should be resorted to in profuse and protracted cases.

"In very obstinate cases, when the powers of the patient are sinking, injections," Dr. H. remarks, "may be thrown into the cavity of the uterus; tepid water is usually sufficient, but weak solutions of alum, sulphate of zinc, or other astringents, may be employed. By some, solutions of nitrate of silver have, also, thus been used. Great care, however, is required in all the uterine injections, as the organ is intolerant of the presence of fluids in its cavity, and hence severe pain, spasms, and, perhaps, inflammation may be excited."

In cases of leucorrhœa, the cause being ascertained, the proper measures for its removal are to be resorted to. All the general and local treatment adapted to anæmic and exhausted cases of irritable uterus, are important adjuvants, for although the discharge is not the result of weakness, when profuse it augments the general debility. Cold vaginal injections, and almost every variety of astringent and alterative washes will be serviceable in hastening recovery. In protracted cases, with great relaxation of the uterine tissues, a portion of these may be thrown into the neck or even cavity of the uterus, with the same precautions, however, as in analogous cases of protracted menorrhagia.

We need not follow Dr. H. in the account given by him of the treatment of the remaining complications of irritable uterus; the principles upon which his therapeutic indications are based, and the means upon which he relies for the carrying out of those indications, will be readily understood from what has been already said. In hypertrophy of the uterus, he relies mainly upon properly constructed pessaries, by preventing thus displacement, pain is relieved, congestion removed, and the morbid development of the organ counteracted. The pessary may be assisted, perhaps, Dr. H. remarks, by the use of anodyne suppositories, or washes to the vagina, by cool or cold ablutions internally to the uterus, and externally to the surface of the body, and, probably, also, by the use of iodine and nitrate of silver to the uterus.

While Dr. H. admits that *acute ovaritis*, when actually present, demands for its removal rest, leeches, and other antiphlogistic remedies, he warns against a similar practice in *chronic cases*, under the idea that pain in the region of the ovary is indicative of ovaritis, especially in those numerous cases where no fever is present. Judging from his own experience, he is led to believe that this pain is generally dependent on uterine displacements, and is often relieved by removing the uterine trouble.

"*Tumours of the ovaries*," he remarks, "when still in the cavity of the pelvis, often irritate the uterus, or the sacral, obturator, or other nerves of the pelvis, by their weight and pressure, and thus excite all the symptoms of an irritable and displaced uterus. Sometimes they themselves become irritable and sensitive. They are, therefore, to be treated on similar principles. The pressure on the uterus and the floor of the pelvis is to be counteracted by mechanical measures per vaginam, so arranged that while this result is obtained, no irritation shall be produced. Perfect relief may be expected on this principle; while to treat such tumours as inflammatory, or to attempt their 'discussion' by evacuants, counter-irritants, and alteratives of any character, would be useless, and positively injurious on account of the confinement to which the patient must be subjected, the loss of strength and consequent aggravation of her wretched, painful, and nervous affections. The patient should be strengthened by tonics, fresh air, good diet, and exercise, as in other irritable complaints.

"If the tumours of the ovaries be large, so as to rise into the cavity of the abdomen, they are generally innoxious, for then, like the uterus after the fourth month of gestation, they are supported by the brim of the pelvis, so that no pressure is made on the delicate organs and nerves of the pelvis, and the tumours themselves are found seldom inflamed or sensitive. In such cases it has been

my practice for many years, and there has been no cause for regret, to attend to the general health and strength of the patient, and to abandon all local treatment. If possible, let the patient forget the existence of the tumours, and she will usually enjoy that portion of health and activity which belongs to her age and sex. The attempts to resolve such tumours experience proves to be generally vain; and to extirpate them by the operation of gastrotomy is greatly to endanger life, for a disease which very rarely terminates fatally, and even then from accidental complications, which of course do not invalidate the general principle or the practical deduction."

Laying down the general principle that the proximate cause of the entire class of the neurotic or hysterical diseases of the female is, in the majority of cases, to be sought for in the presence of an irritable uterus—the consideration of which is the subject of the first part of the treatise before us—Dr. H. proceeds, next, to show the source by which the irritable condition of the uterus is produced. Among its causes he assigns the most prominent and important rank to displacements and distortions of the organ. These he believes to be often the sole cause of the morbid irritability of the uterus, and the nervous complaints and sufferings thence resulting, and which will, in very many cases, persist under every plan of treatment adopted, that has not for its basis the replacement of the dislocated organ, and the redressing of its flexures. In order, therefore, to complete his analysis of the pathology and treatment of irritable uterus, Dr. H. found it necessary to enter into an examination of the causes, effects, and management of the several malpositions of the organ, which is the subject of the second part of the treatise.

The first chapter of this second part is devoted to a consideration of the provisions made for the sustentation of the uterus. These provisions are such as to prevent any permanent displacement or injury to the organ in the healthy and robust female, under all the muscular efforts demanded in walking and in the various occupations she may be called upon to fulfil.

"The fundamental principles are, the obliquity of the pelvis necessitating the obliquity of the uterus, the parallelism of this organ to the pubis, and of its long axis to the axis of the superior strait. This obliquity is maintained by the similar oblique attachments of the ligamenta lateralia, and the conjoint action of the ligamenta rotunda with the ligamenta utero-sacralia, the one keeps the fundus towards the pubis, and the other the cervix towards the sacrum, while the pressure of the intestines surrounding the uterus is so directed as to form a barrier to the anterior and posterior displacements, and, even by the indirectness of its action, to prevent a prolapsus uteri. All these agencies, acting in concert, maintain the obliquity of the uterus as regards the axis of the body, and yet permit a certain degree of motion to this organ. Thus, when the bladder is distended, it is pushed backwards towards the sacrum; when this viscus is emptied, and the intestines, particularly the sigmoid flexure of the colon and of the rectum, are distended in the ever varying condition of the alimentary canal, the uterus will be pressed towards the pubis. So, also, in all the muscular movements of the body, the womb will be more or less depressed, but will again rise as soon as the effort has ceased. The ligaments of the uterus are so elastic and yielding as readily to allow those necessary movements with impunity, and, at the same time, have such strength and contractility as to bring the organ back to its normal oblique position."

The various forms of displacements of the uterus are next considered (Chapter II.), together with their causes. As predisposing causes, Dr. H. enumerates relaxation and elongation of the ligaments and of the reflected pelvic fascia; the first occurring during states of general debility and exhaustion, the second as a natural condition after parturition, and, abnormally, when, from any cause, the uterus has acquired an increase of weight

and size, or is subjected to undue pressure, whether from improper dresses or any species of bandages applied around the abdomen, or from distension of the abdomen due to an accumulation of feces or of urine, in the intestines or bladder, to tympanites, to ascites, to enlargement of the ovaries, mesenteric glands, liver, kidneys, omentum, &c. As exciting causes are set down sudden or prolonged muscular efforts, straining in micturition and defecation, especially soon after parturition, ascending stairs or heights, violent sneezing, coughing, or vomiting, raising weights, carrying burdens, &c. Displacements are also often determined by tumours developed on one or other face of the organ, also by osseous growths from the pubis or sacrum, abscesses, ovarian enlargements, &c.

Dr. H. admits (Chap. III.) that displacements of the uterus may exist in innumerable cases with apparent impunity, certainly without exciting any symptom to direct attention to their presence. The woman may feel well, and all her functions may be easily and comfortably executed. It is admitted that, in various temperaments, and in all classes of society, such cases may be met with; they chiefly occur, however, in the strong and laborious, in those of a cold, phlegmatic temperament, and in northern countries; in those in whom the nervous system is not very sensitive, nor particularly excitable. In general, however, Dr. H. maintains that in a large number of cases, among women in civilized life, uterine displacements are the cause of more or less inconvenience, even when the uterus is by no means very sensitive; when, however, from the simple fact of its displacement, or from other causes, physical, intellectual, or moral, the uterus becomes morbidly sensitive, then, according to Dr. H., we have more or less fully developed local and general phenomena, indicative of pelvic and cerebro-spinal irritation, together with a series of severe reflex influences on the various organs of the economy.

"The practical deductions," says Dr. H., "as confirmed by long-continued experience, seem to the author inevitable, that mal-positions of the uterus are so intimately connected with all the phenomena of irritable uterus, as original or aggravating causes, as to make their removal generally absolutely necessary for the restoration of the organ, and of the cerebro-spinal system to its normal state; that dysmenorrhœa, menorrhagia, and leucorrhœa; the pelvic sufferings, the inability to walk or stand; the spinal and cerebral irritations, with all the occasional but terrible disturbances of the larynx, lungs, heart, stomach, liver, kidneys, and bowels—especially the rectum—can hardly be completely relieved, and sometimes not even palliated, if the displacement be permitted to remain; but, on the contrary, if this be removed, recovery will generally quickly follow.

"There are, perhaps, many cases of cerebro-spinal irritation, of 'hysteria,' as such complaints are usually named, in which no displacement exists; and that many possibly may be found, even where there is no uterine irritation of any kind—inflammatory or otherwise—no one will deny. Beside these cases, there have occurred to the author, and doubtless to others, those in which the displacement had been completely relieved, yet the local and general symptoms, although palliated, still persisted. This persistence is very often due to our ignorance of the sources of sacral irritation, as from pessaries in the vagina, from enlarged and displaced ovaries or other pelvic tumours, from some state of the rectum, of the tissues in the region of the sacrum, or of the bone itself. There must be a local irritation to disturb the nerve centres; it is generally in the pelvis, although it may exist in any tissue or organ of the body. It seems impossible, at least for the author, to conceive of spinal or cerebral irritation without a local cause. There must be an irritant acting on the irritability of a tissue to produce irritation."

In the treatment of the various displacements of the uterus four indications, according to Dr. H., are to be fulfilled: 1st. To remove or palliate

any existing causes; 2d. To replace the organ; 3d. To maintain it steadily in its normal position; and 4th. To strengthen the natural supports of the uterus. The consideration of the manner in which these indications are to be fulfilled is the subject of Chapter IV.

It would give us great pleasure to follow the author in his very interesting exposition of the various measures adapted for the cure of uterine displacements, but the length to which our analysis of the preceding portions of the treatise has extended, will oblige us to notice these only in a very cursory manner. A reference to the enumeration given above of the causes of displacement of the uterus will suggest at once the means proper for their removal. When this is effected it is seldom, however, sufficient. When the ligaments of the uterus have become elongated to such a degree that their natural elasticity and contractility have been lost, it is not the mere removal of the original cause of displacement that will suffice for the restitution of the organ—the weight of the intestines and the contraction of the abdominal muscles will still keep it depressed.

To replace the uterus in its natural position is sometimes a very easy thing, but occasionally it is difficult, and in some instances impracticable. Anteversion and simple prolapsus are the displacements most easily rectified; retroversion and retroflexion, especially the latter, are those which are the most difficult to rectify; requiring a resort to accomplish this, to the uterine probe or sound, of a suitable curvature, and to a properly shaped pessary. These, according to Dr. H., will always answer, provided there are no adhesions, large tumours, or contractions of the vagina to mechanically resist their safe operation.

After the replacement of the organ it is necessary to resort to such means as will prevent the pressure of the intestines, the contraction of the abdominal muscles, the upright position and the various movements of the patient causing a speedy recurrence of the displacement. To maintain the uterus in its normal position various plans have been resorted to, all of which are condemned by Dr. H. with the sole exception of a properly constructed and skilfully applied pessary. A pessary constructed of incorruptible material, and of such a form as shall restore and retain the organ in its proper position at all times, under the ever-varying pressure from above, being movable with it, allowing of all its natural motions, yet effectually preventing any displacement; being one, as it were, with the uterus; one that can be worn without pain, uneasiness, or discomfort, without, indeed, any consciousness of its presence on the part of the patient; one that will relieve, not increase nervous irritations; that will excite no organic or vascular disturbance, no engorgement, no inflammation; that will have no tendency to increase leucorrhœal or menorrhagic discharges, and which shall contribute to the resolution of chronic inflammations of the os and cervix uteri.

Dr. H. enters very fully into an examination of the leading varieties of pessaries which have been recommended by different writers on the diseases of females, and the particular advantages and disadvantages of each. He has endeavoured to show that, with the exception of the intra-uterine, and what he terms “the lever” pessary, all fail in fully replacing and sustaining the fundus of the uterus. He has attempted to prove, further, that although the intra-uterine pessary may, mechanically considered, be more directly efficient than “the lever,” yet the objections against its use are insuperable. The “lever pessary” is the favourite of the author. Of this there are two

varieties, the open and closed; see figs. 1, 2, 3. For a description of their modifications, we must refer to the work before us.

Fig. 1.

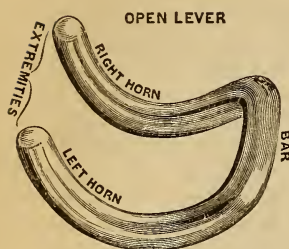


Fig. 2.

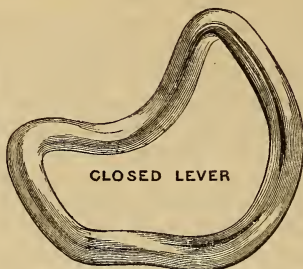
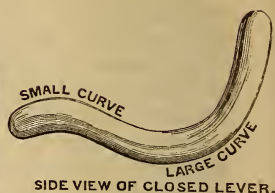


Fig. 3.



"The lever pessary," Dr. H. remarks, "will more slowly accomplish the restoration of the displaced organ, but, eventually, with equal certainty. It does not necessarily produce any irritation, organic or nervous, or any leucorrhœa, menorrhagia, inflammation, &c. It can be worn at all times, night and day; it interferes with no motion and no function; the patient has no attentions to pay to it, excepting a daily vaginal wash; she may, and often does, forget its presence; can enjoy her connubial pleasures; can move about in society without anxiety; is free from local and general nervous irritations; from corporeal, intellectual, and spiritual disturbance; and her physician may hope, that the uterus being perfectly sustained, the ligaments, now free from every counteracting influence, will continue to contract to their normal length, and acquire their original tonicity, so that a permanent cure may be effected, or, that pregnancy ensuing, the continued use of the pessary will preserve his patient from those irritations so frequently excited by displacements, and so apt to result in abortions."

After considering in Chapter VII. the treatment of the varieties of displacements of the uterus, prolapsus of the vagina, hernia of the bladder, &c., he treats of the fulfilment of the fourth indication, namely, so to strengthen the natural supports of the uterus that artificial assistance may be dispensed with. Dr. H. believes this may be effected simply by sustaining in situ the organ for a sufficient length of time. Under such support the ligaments sooner or later become shortened, particularly when to the sustaining action of the pessary is added such hygienic and remedial measures as have a tendency to increase the tone and energy of the general muscular system. Dr. H.'s usual plan is, at the end of a year, to remove the pessary, but if symptoms reappear, and especially if, on successive examinations, the uterus is found gradually returning to its malposition, the instrument is to be replaced immediately.

Chapters VIII. and IX. are devoted to a consideration of the treatment of complications of displacements of the uterus, flexures, menstrual disturbance, enlargements and tumours of the uterus, enlargements of the ovaries, &c. We regret that we cannot command the space to present even a brief analysis of the author's views on these important subjects. We can merely state that flexions of the uterine neck he treats by the passage of a uterine probe two or three times, and the use of a ring pessary; while to the lever pessary he mainly trusts for the cure of menstrual disturbances, as well as for the removal or relief of enlargements and tumours of the uterus and of the ovaries.

The *third part* of the present treatise is devoted to the pathology and treatment of the diseases of sedation, but more especially to the sedation of the uterus, its causes, its consequence, amenorrhœa, and its treatment. To

the important subject discussed in this portion of the work we can give only a passing notice.

After pointing out the sources of uterine sedation, moral impressions which depress the energies of the brain and nervous system, bad physical education, all exhausting pursuits and practices, certain mechanical and vital conditions of the sexual organs, a state of general atony, certain organic diseases, rheumatism and gout, fevers and phlegmasia, nervous irritations, exposure to cold, &c., Dr. H. presents the following general conclusions: 1st. That the various pathological conditions associated with amenorrhœa are not, as popularly supposed, the consequences of the latter, but rather its causes. 2d. Although the suspension of the menstrual flux is often of serious import, yet the mental or physical phenomena coincident with such suppression have not a merely local effect on the uterus and its appendages, but a similar sedative influence also on other organs, as the brain, heart, and their dependencies. Hence the general disturbance of the organism; the general deterioration of the animal economy. 3d. The restoration of the menstrual flux, although a most important phenomenon as a sign of improvement, does not necessarily re-establish the health of the patient. 4th. Emmenagogues, or those remedies which are supposed to act immediately upon the uterus, are of themselves altogether inefficient as a means of re-establishing the menstrual function. To effect this the practitioner must have recourse to a course of treatment, having only an indirect and secondary influence upon the pelvic viscera. 5th. Amenorrhœa, therefore, is, in all cases, merely a sign, a symptom, of abnormal states of the uterus or ovaries, or of the general nervous and vascular systems. In respect to treatment in cases of amenorrhœa, Dr. H. directs, during the supposed intervals of the menstrual nixus, every effort to be made to rectify the condition of the system generally, to relieve local diseases, and to favour the development and functions of the entire organism. When the return of the periods is expected, to determine, as far as possible, the circulation to the pelvic viscera, by equalizing the general excitement, by promoting secretion from the entire cutaneous and mucous surfaces, by revulsive remedies internal and external, by direct stimuli to the uterus, and sometimes by the employment of medicines supposed to act specifically on the uterine function. Disappointment, it is true, must often ensue, the causes of amenorrhœa being frequently irremovable.

We have endeavoured, in the somewhat brief analysis we have thus presented of the treatise of Dr. H., to give to our readers a correct idea of the teachings of the author in respect to the character, causes, phenomena, and treatment of an important class of diseases incident to the female sex. Diseases that have excited always the deepest interest on the part of the medical profession, and yet, in respect to which, notwithstanding the amount of talent, industry and laborious research which have been devoted to their elucidation, almost everything, whether relating to their causes, their nature, or their management, remains a subject of dispute among even the highest authorities in the profession.

The work of Dr. H. is calculated, we candidly believe, not merely to attract the attention of the medical men of our country, from the interest that surrounds the subjects of which it treats, and the reputation, skill, and experience of its author, but to make a highly favourable impression upon the opinions and practice of the profession here and elsewhere, in respect to some, at least, of the more frequent ailments of the female sex, from the evident soundness of the general views taught in it, the accuracy of the descriptions given of the morbid conditions and phenomena investigated,

and the apparent strength of the evidence presented in support of the correctness of the teachings of the author in respect to the theory of certain, at least, of those ailments, and their proper therapeutics.

We are not, we may remark, entirely convinced of the truth of the leading dogma of Dr. H., that the nervous derangements, the hysterical affections to which women, in modern times, are so peculiarly liable, are the result of a state of morbid irritability of the uterus, of a state of undue exaltation in the excitability of the organ, and the reflex morbid impressions made by it when thus affected, through the cerebro-spinal system, upon remote organs. That all the morbid phenomena Dr. H. describes as those of nervous debility are to be met with in many cases, every physician is aware, from his own clinical experience. We feel tolerably certain that, upon a careful investigation of the phenomena referred to, every one, who is not too firmly wedded to some preconceived hypothesis, will become convinced that they are the result of a state of irritability of the proper nervous tissue alone, unassociated, excepting in complicated cases, with any appreciable state of irritability, any inflammatory condition of the other tissues. We rather think, however, that, upon close examination, it will be found the starting-point of the irritations from whence result the entire series of neurotic affections observed in females, is by no means always or even very generally the uterus, but that the state of irritability and the disturbed functions of the latter, in common with the same condition of things in other portions of the organism, are the result of the general morbid irritability of the system, the state of undue excitability of the entire series of nerves of animal life and of the nervous centres. This conclusion we should be led to almost alone from the admissions of Dr. H. himself; who, in the exposition he has given of the irritable uterus and its consequences, will be found to reason somewhat in a circle, referring the latter disease, at one time, to the state of nervous irritability of the organism at large, while at another time he would seem to consider the latter, or, at least, the morbid phenomena to which it gives rise, as entirely the result of the pre-existing irritable uterus.

The treatise of Dr. H. is, throughout, eminently practical, and in the proper sense of the term. His remedial measures are invariably in strict accordance with the pathological views inculcated by him. Even while we are unable to grant to slight displacements of the uterus, occurring at any period during the term of the proper sexual life of the female, the same degree of importance that Dr. H. does; while we cannot refer with him the production of the nervous or hysterical affections of the female so generally to a condition of irritability in the uterus, making the latter condition, in fact, as he asserts, the "*ipse morbus*," the disease itself; although we profess not to understand how it is, that while the vulva, vagina, rectum, etc., in common with the uterus itself, are in a state of such acute sensibility that the slightest pressure exercised upon the latter, or by it upon the surrounding tissues, is productive of intense suffering and wide-spread mischief, the introduction and continuance of a large, heavy, metallic pessary into the vagina and in contact with the uterus, as a mechanical means for the restoration and support in a proper position, of the latter, can be simply tolerated, much less produce such prompt and efficient relief as is ascribed to it by Dr. H.; still, notwithstanding all this, many an important lesson in respect to the proper management of the nervous disorders of the female, whether their cause be located in the pelvic viscera or in some distant organ, may, we admit, be acquired from the work before us. For the fact cannot be doubted that a very intimate connection does often exist between an irri-

table condition of the uterus and the general symptoms of nervous disorder so common in women; whether we trace the latter to some local irritability or refer whatever decided symptoms of local irritability may be present to the same general condition, whether congenital or acquired, which constitutes the eminently irritable temperament, and assume this as the cause of all the disturbances produced in those who possess it, by the influence of normal or abnormal impressions. Although it may be shown that the diseased condition or displacements of the uterus are in no instance to be viewed in any other light than as a source of aggravation to the nervous disturbance and suffering of remote organs, through reflex impressions transmitted from it to the latter, still, even under this view of the subject, the therapeutic teachings of Dr. H. will be found of value.

We would call especial attention to his views in respect to uterine and ovarian tumours, hypertrophy of the womb, and the supposed inflammations and ulcerations of the os and cervix uteri.

D. F. C.

ART. XII.—*An Elementary Treatise on Human Anatomy.* By JOSEPH LEIDY, M. D., Professor of Anatomy in the University of Pennsylvania, &c. &c. With three hundred and ninety-two illustrations. 8vo. pp. 663. Philadelphia: J. B. Lippincott & Co., 1861.

THE appearance of a new elementary treatise of anatomy following so closely upon the recent edition of Wilson, and the descriptive and surgical anatomy of Gray, furnishes conclusive evidence that its distinguished author, at least, recognizes a want which existed; and he has, accordingly, prepared "such a book as he feels would have been of service to himself in the commencement of his studies," and sought to lighten the labour of students "for whom, and by whose frequent solicitation, it was written." Let us ascertain how the task has been accomplished, and what meed of thanks or praise must be awarded to the work.

In the first place, we regard as a most commendable feature of the treatise under consideration the association of general with descriptive anatomy; which, although not an innovation, is more satisfactorily harmonized than in any similar treatise with which we are acquainted. We have long entertained the idea that the distinction between what are termed separate branches of anatomy is arbitrary and unnecessary, for these both comprehend the same systems, organs, and tissues, and consider their physical qualities and the relations of their parts. We ought not to determine their severance because we do not arrive at a knowledge of all the facts by the unaided sense alone; for it is not the eye which sees, but the judgment by the eye, whether armed with the microscope or unassisted by that instrument. Furthermore, illustrations of microscopic details ought to be as intelligible to the student as those of less minute parts, the figures of which are diminished to the same extent that the former are magnified. Placed in juxtaposition, histology and special anatomy give each other a mutual support; and the one prepares the mind for an interest in the other, which will be rarely awakened in the beginner who is forced to consult two or more distinct works.

Another charm which attaches to the new elementary anatomy may be found in the force and simplicity of the text, the language in general being clear and the descriptions graphic; and in the admirable manner in which positive knowledge is presented and kept fully in view, and the speculative

debarred from unprofitable intrusion. We may even venture to assert that the redoubtable muscles of the back are shorn of the terrors which they have worn in former treatises; and that the hepatic ducts at length stand in reasonable relation to the "long drawn" cells which have been by turns the triumph and the despair of observers.

If in a manual, so excellent in many respects, we might particularize portions of engaging interest and superior merit, we would mention with unequivocal satisfaction the chapters on the organs of special sense and the nervous system, for which the author deserves the lasting gratitude of medical students, and, perhaps, of not a few "general practitioners." We opine that the neophyte must have little zeal or application, who, after a study or perusal, even, of this volume, still "gropes in ignorance of his own insides," for in the main the story is well told and its comprehension is assisted by no less than three hundred and ninety-two illustrations, which are executed in a highly creditable style of art. And it is worthy of note that of these but two hundred and sixty-six have been gleaned from older authorities, while the author has contributed the very considerable number of one hundred and eleven original figures, besides fifteen others which are equally new. Until recently, illustrations continually reproduced had become venerable from their antiquity, so much so that we almost suspected the same fidelity in copying in every portion of the work; but Draper's and Dalton's Physiologies, and the Elements of Anatomy under consideration, may safely be mentioned as honourable exceptions to a rule which none but thinkers would have ventured to infringe.

It is but fair, in a candid examination of a volume which, as a text-book, is destined to be extensively used by students, to state what we deem to be its demerits. These are of two kinds, of omission and of commission. The first sort may be exemplified by the "topographical sketch of the anatomy of the neck" (p. 199), in which, upon the presumption, we suppose, of knowledge in the reader, the triangles of the neck are named, but their limits are not defined. It is true that the author passes beyond the sphere of special anatomy proper; but having seen fit to trench upon regional anatomy, he should hardly have done otherwise than determine the boundaries of fields or spaces which a particular region comprises. In a didactic work nothing should be left to the imagination, especially when a few explanatory words would relieve the reader from all embarrassment.

We further regret the omission of the *relations* of muscles in the chapters on myology, and the brevity with which the course of arteries is traced, both of which, in our opinion, detract materially from the usefulness of a work from which the earliest and most vivid impressions are to be derived; and were it not for a direct appeal, in the preface, to be tried under the very title of the treatise, we should be inclined to fear lest the author, in seeking to avoid the Scylla of complexity, had brought himself into dangerous vicinity to the Charybdis of superficiality.

A further objection has reference to the novel or quasi modernized nomenclature, and against it we beg leave to express our positive disapprobation, because it is inconsistent and not thorough, is confusing and not lucid, and is an innovation without being an improvement. A new system of nomenclature must arise out of a clearly appreciated necessity, and is expected and required to be more simple and more distinctive than that which it proposes to supersede; and, besides, amelioration must be everywhere sustained, or else the incongruous parts appear as "disjuncta membra." It is not the same thing to attempt a change almost amounting to revolution, and to effect, after a definite plan, by retrenchment or otherwise,

such alterations as are likely to relieve the memory of an unnecessary load; to present an array of words in part new, but not more easy of recollection, and not by any association more suggestive than older titles, and to reject such cumbersome articles of nominal furniture as take up unnecessary room. Thus we are free to admit the propriety and advantage of suppressing such a name as *sphenosalpingostaphylinus* muscle, and of adopting the simpler appellation *levator palati* in its place; but we are at a loss to understand how students are to be gainers in the employment of "greater and lesser *terete* muscles" instead of the familiar terms *teres major* and *minor*. If in one place the author says round ligament for *ligamentum teres*, why not, in another, translate *musculus teres major* into greater round muscle. Why not discard *lumbricales* muscles in favour of worm muscles; why say "asperous ridge," "dorsal pedal artery," or "superior profound artery," when *rough line*, *instep artery*, or *superior deep artery*, would be more in keeping? Finally, wherefore should "carpal" be retained when *wrist* might be plainer; and the ear-bone stapes be called "stirrup" and its muscle be entitled "stapedius" in lieu of stirrup muscle?

It is true that a clue is afforded to usual and to unfamiliar names in the "synonymy," at the bottom of the page, by means of which the painstaking student may recognize in other anatomical text-books, or in surgical works, the parts to which the volume we are now reviewing introduces him; and for this the necessity is apparent. But believing, as we do, that the study of anatomy is a simple matter of memory, we are of opinion that the consistent old system of nomenclature, which has the merit of being generally employed and universally understood, if not accepted, is not invested with any greater difficulties than the newer, imperfect system, proposed by the author. Nevertheless, we would not wish to be understood as contending for the old system under all circumstances, but venture to express our conviction that for the present, at least, the line of improvement may more profitably be made to follow another direction.

There are errors in the work which are clearly attributable to oversight; thus on page 97, "the vertebral column is said to be composed of twenty-four true and *five* false vertebræ," while on page 103, the number of the latter is correctly given. Again, the chapter on muscles of the buttock is full of confusion; the last line of page 244 is continuous in sense with the third line of page 245. The last line of page 246 is to be succeeded by the first two lines of page 245, while the remainder of the sentence is to be met with at the conclusion of the chapter, on page 247. Again, on page 558, we find the "accessory" denominated "*the twelfth* pair of nerves."

We are far from wishing to give undue prominence to errors of this class, which more deliberate writing and proof-reading would have prevented; but we dwell upon them for the reason that inadvertences which might be harmless in a more elaborate work, are positively hurtful in a treatise designed for the young and uninitiated. For we feel assured that accuracy and perspicuity are indispensable above all things, in text-books of science or art; and that no labour should be spared which might render the position of the reader of them perfectly secure.

On the whole, the treatise in question must be regarded as original in plan, and, with the exceptions above given, successful in execution. It is conceived in a spirit which seeks to inspire students with love for science, and to lead them into its green fields by pleasant paths; and, if we have recognized some errors or shortcomings in the guide-book, let us not be unmindful of the mass of merit in which we have encountered them.

The work is beautifully printed on faint buff tinted paper; and, as a specimen of typography, reflects great credit on the publishers. C. J.

ART. XIII.—*De la Nature et du Traitement du Croup et des Angines couenneuses, étude clinique et microscopique, démontrant: 1. Que les concrétions, source de tous les accidents, sont des produits d'origine parasitaire ou moisissures. 2. Que la base du traitement repose sur l'application de topiques parasitocides, médication aussi rationnelle qu'heureuse en pratique.* Par le Dr. N. JODIN, Médecin du 9^e Bureau de Bienfaisance de Paris, Chevalier de la Légion d'Honneur. Paris, 1859. 8vo. pp. 39. (Adrien Delahaye, Libraire-éditeur.)

A Clinical and Microscopical Study of the Nature and Treatment of Croup and of Pseudo-Membranous Angina, showing: 1. That the concretions, the source of all the symptoms, are products of parasitic or vegetable origin. 2. The basis of treatment consists in the application of topical parasitocides, a medication as rational as it is happy in practice. By Dr. N. JODIN, &c. Paris, 1859. 8vo. pp. 39.

CROUP is a fearful disease. To determine its nature and the best mode of treatment, Napoleon I., in 1807, ordered a "*concours*," which set the whole medical world to work, and brought forth hundreds of memoirs, in which may be found a general exposition of the science of the time. Its summary may be stated in the following propositions:—

1. The anatomical character of croup is the formation of a false membrane in the windpipe, and its functional symptoms are cough and a peculiar alteration of voice, with paroxysms of suffocation. With some authors the functional symptoms alone constitute croup, without the expulsion of false membrane, in cases of recovery, or its presence after death; they say that the formation of false membrane had been prevented either by the activity of the treatment or the rapid course of the disease.

In modern medicine, cases without false membrane form the pseudo-croups, among which is placed the acute asthma of Millar, spasm of the glottis, &c. In the opinion of Dr. Jodin, the addition of the class of pseudo croups constitutes a source of error as to the symptoms and mortality of true croup.

2. This disease is distinct from the Syriac ulcer of Areteus or gangrenous angina, which, when present with croup, is considered to be a complication. Nevertheless, some authors, having observed in epidemics of gangrenous angina that croup commenced in the throat and extended into the larynx, confound the two diseases. In this they agree with popular sentiment, which recognizes the suffocation and death, without regarding the precise site of the anatomical product.

3. It appears to have existed through all time, although false membrane in the windpipe was mentioned for the first time in 1576, by Baillou. To it must be referred the *angina gravissima*, the *pulmo repletus* of Hippocrates. It must have been more frequent in the second half of the last century, if we may judge from the great number of memoirs which appeared in that period.

4. It appears in countries very different from each other, north and south, east and west—in Sweden, England, Germany, France, Switzerland, Spain, Italy, and America.

Humidity of climate or of the season seems to favour its development.

5. It is generally attributed to a principle diffused through the air.

6. It is contagious according to some, non-contagious according to others, who, however, admit the contagion of gangrenous angina.

7. Everywhere false membrane is considered to be the product of a peculiar inflammation.

8. Authors differ as to the asphyxiating action of the false membrane: some regard it as the sole cause, acting like a foreign body; others attribute to it only a part in the suffocation, in the production of which they include both the inflammatory tumefaction and the spasmodic condition.

9. The treatment is in accordance with these differences of opinion. The first direct all their therapeutic means against the false membrane, seeking to dissolve and detach it from the mucous lining with calomel in large doses, sulphuret of potassa, polygala, or to expel it by emetics, and tracheotomy.

The latter, consistently with their theory, and alleging besides that to one false membrane expelled succeeds another as long as the productive cause—inflammation—continues, place no great value on all these means; if they employ any of them, mercury or emetics, it is only as auxiliaries. They reject the sulphuret of potassa as not having been approved, and they condemn tracheotomy as useless and dangerous. They attack the inflammation by letting blood, by revulsives or derivatives, vesicatories, sinapisms, purgatives, &c.

10. The preservative treatment consists only in hygienic precautions, to protect children against cold and humidity.

This summary shows that the "*concoure*" has been useful, because it brought together whatever was known of the disease in different parts of the globe, and spread this information before the medical public; but this is the only fruit it has produced. It did not determine the nature of the disease, for we learn very little by being told that it is a peculiar inflammation, nor has it determined the treatment.

Discoveries in medicine are not made either at an appointed time or by command. Truth often obstinately eludes the most profound and persistent investigations, and is at last revealed by some fortuitous circumstance, such as the fall of an apple.

Ten years later, M. Bretonneau did more for croup than all the distinguished physicians who participated in the "*concoure*." An epidemic angina maligna, which prevailed at Tours from 1818 to 1821, afforded him the opportunity for observation.

He began to doubt the gangrenous character of the angina from observing at an autopsy that the velum palati enveloped in sloughs presented, on being divided from behind forwards, a red section betwixt two superficial gray lines. His doubts were increased by seeing, during life, all the soft parts of the back part of the mouth, which, while covered by sloughs, seemed a putrid mass and deeply sphacelated, remained, on the detachment of the sloughs, in a state of perfect integrity.

At the *post-mortem* examination of a subject who had presented a gray tint throughout the whole back part of the mouth, and who had perished with symptoms analogous to those of croup, he found in the laryngo-tracheal passage a tube of white, supple, elastic membraniform substance, extending downwards to the last bronchia, and continuous above with the sloughs which still covered the isthmus faucium; on removing these supposed sloughs, he saw that the face which rested upon the mucous membrane was neither gray nor blackish, like that exposed to the air; it had the consistence, whiteness, and lustre of that which had been drawn from the trachea; the mucous membrane did not present the slightest trace of gangrenous alteration; the red spots, themselves pict in deeper red, without erosion, without thickening of tissue, were the only observable marks of inflammation; the redness was still less decided than in the trachea.

Continued observation through the epidemic constantly yielded the same

results. With a single exception, the false membrane of the laryngeal tube was always consecutive to concretions in the throat.

From his experience he deduced the following conclusions:—

1. The gangrenous character of concretions of the throat in gangrenous angina is only in appearance; this appearance is due to putrid decomposition, favoured by the moist heat of the mouth and by the action of the air.

2. These concretions are in fact false membranes, in every respect identical with those of croup, which are only an extension of them.

3. The identity of the affections leads to that of the disease.

4. This disease is a specific phlegmasia, consecutive to a diathesis, and for which he proposed the name diphtherite, designed to distinguish it from other inflammations.

In the treatment of this disease, M. Bretonneau relies chiefly on the application of caustics. When the concretion has descended into the throat so far as to be beyond the reach of caustics, he recommends, as a last resort, mercury and tracheotomy.

Diathesis and specific phlegmasia have been received without discussion, and have never been criticized.

Cauterization has been variously received. It has been adopted as the only means of treatment by some physicians, who, without troubling themselves more than did M. Bretonneau as to whether or not it was in harmony with theory, applied it energetically from the beginning, and generally with success.

It has been accepted with reserve by a majority of physicians, who, to reconcile practice with theory, have added it to the treatment in use; but, little confident in its efficacy, and fearing extensive or deep sloughs, they have had recourse to it only tardily, when they saw the affection advance in spite of the antiphlogistic treatment, and applied it timidly, selecting the most feeble caustics. They reckon more cases of failure than of success.

It has been attacked, particularly in later years, by physicians who oppose it on the ground that they cannot admit the omnipotence of the action of a remedy purely local against a diathetic product or one from an internal cause, renewing the old charge of adding an artificial inflammation to an inflammation already existing, and proscribing it absolutely. They advise alkaline preparations, as alone capable, they say, of destroying the primary cause of the affection, the plasticity of the blood. Though logic is on their side, practical results are not favourable to their views.

For the present, chlorate of potassa has succeeded the alkaline preparations. It is greatly in vogue; it is employed in a host of diseases in which an alteration of the blood is suspected. Failures come; already M. Blache and others have remarked that it is without action in laryngeal croup, and it will be abandoned in its turn, and cauterization will be revived, to be again attacked.

These attacks, incessantly renewed against cauterization, are fatal to it, for a simple reason—it is inconsistent with theory.

A medication purely empirical may be sustained without theory; there are so many things inexplicable in medicine—as, for instance, the action of mercury in the cure of syphilis, and of quinia in paludal intoxication. But reason will always refuse to admit a medication, however successful it may be, if it is inconsistent with theory; for it then implies contradiction, and in medicine, equally with other sciences, contradictory principles are not tolerated. When placed face to face, one must necessarily be false. Which is here false, the medication or the theory?

Cauterization may invoke in its favour numerous incontestable facts.

In therapeutics it is often very difficult to determine how much is due to medication, and how much to the medicatrix naturæ; but when a remedy is frequently repeated, and the same result uniformly follows its application, this action must be acknowledged.

Diathesis is a pure hypothesis, admitted to explain the formation of false membrane and its dissemination.

The formation of false membrane is rather against than in favour of diathesis. Under various circumstances in which it is produced, we find, in place of diathesis, a local cause. It is the most ordinary termination of serous inflammation; but then it is due only to the anatomical arrangement of the inflamed parts, of which it is the product, inclosed in a sac without opening, incapable of being freely poured out exteriorly, as in cutaneous or mucous phlegmasia, it becomes organized in false membranes, when it is not of a nature to rupture the pouch which contains it. It takes place around foreign bodies, to which it forms an isolating envelop, on the surface of blisters, or on the vesical mucous membrane in subjects where cantharides have been absorbed; but there we still find a local cause applied directly upon the part affected, or conveyed to the bladder in the urine. There is not the shadow of diathesis. In all cases, it is true, the false membrane remains circumscribed, as M. Bretonneau has judiciously remarked. It is disseminated in croupal affections.

Dissemination is the grand argument put forward in favour of diathesis in croup as in eruptive fevers.

But dissemination is not peculiar to diathetic affections. It is observed also in itch, and the tineæ produced by a parasitic animal or vegetable. Consequently, of itself, it proves nothing either for or against diathesis.

But if we follow the course of dissemination when it spreads on the skin, we find a peculiarity which furnishes a strong argument against diathesis; it is, that the affection attacks exclusively the surface of blisters, leech bites, or the edges of the wound made in the trachea; the post-aural region, the vulva, the fingers or toes ulcerated by chilblain. Every-body has observed these facts, and passed them by without giving them the least consideration, except to make them available in favour of diathesis. All these points, often distant from each other, have been regarded only as different causes; here vesication, or a cutting instrument, post-aural scrofula, the action of the nails on the vulva, ulcerations of the toes from chilblain; that these determine an alteration, physical, common, consecutive to the tearing or raising the epidermis, the denudation of the skin. No one has observed, what, however, is as clear as day, that this denudation—existing previously to the invasion of the skin by the affection, which respects every part, which preserves its protecting epidermis—singularly resembles all affections inoculable, or from external cause, and especially the mould of fruits, which suffer only when the envelop is injured, either by friction or the puncture of a worm. Croupal affections are identical with those of the mucous membrane, with which they are continued on the edges of wounds of the trachea; it is probable that what happens on the skin occurs on the mucous membrane, although epithelial abrasion may not be always so apparent.

Croup is communicated by respiration and by inoculation.

The disease has been carried into a place where it did not previously exist, by an individual going from an epidemic centre, and communicating it to those who approached him, or who only inhabited the same apartment.

It has been respired by physicians who have watched at the bedside of patients (Blache fils, Valleix).

It has been inoculated upon the lip, in the mouth, or in the nasal fossæ of the physician who cauterized the throat (M. Gendron), or who did tracheotomy (M. Herpin de Tours), by concretions ejected by a convulsive cough, in the first instance from the throat, and in the second from the opening made in the trachea.

It has also been inoculated on the foot where excoriated by chilblain, and by contact with a portion of expectoration from a patient.

There are physicians who do not admit the contagion of croup. They object that the instances of inoculation belong to angina, and not to croup, which is a different disease. But the inoculation, above stated, produced by laryngo-tracheal false membrane, overthrows this objection. It is urged, too, that attempts made to inoculate animals with croup have failed. The same objection might be brought against diseases generally reputed contagious, as smallpox and itch. Contagion alone is not enough to produce the disease; there must exist, also, susceptibility in the subject. A principle contagious in man is not necessarily so in animals. Besides, the same subject may be apt to contract it at one time, and become refractory to it at another, and *vice versa*.

If the contagion of croup be denied in the sense of Fracastor—that is, having power to reproduce the germ from which it originated—there is one point on which all are agreed, that is, contagion in the Hippocratic sense, namely, that it is produced by a principle diffused in the air.

2. The disease is developed without *initial fever*. Fever may exist at all periods of croup, even in the beginning; but it never pertains to the croup itself; it is always symptomatic of another disease, which may be pre-existing, as scarlatina; intercurrent, as pneumonia, or consecutive as phlegmon of the submaxillary glands; and in this last case it takes the character of suppurative fever, mentioned by all authors who admit the existence of fever in croup; it is enough to be advised of the cause of errors to avoid them. Every impartial observer, who encounters a fever, should always find by diligent search, the disease of which it is only a symptom.

3. The *local affection* is developed in a peculiar manner, very different from that of diathetic affections.

It begins by white points, transparent beneath, and around which is perceived a redness altogether superficial, and without the least swelling, constituted by a very fine vascular injection, and by little ecchymoses.

Later, these points spread in all directions, from spots, striæ, or bands, which throw out bridges, unite with each other, and by forming more or less extensive masses, without however any regularity. At the same time the concretion thickening, loses its transparency, changes colour, becomes deeper, gray, brown, or blackish, the thickness being always more decided at the centre than at the periphery. The peripheric redness, in a manner, precedes the concretion, accompanies the tumefaction, and forms a sort of border, which gives it a sunken appearance, and the aspect of a sordid ulcer (*crustam circumveniunt rubor excellens et inflammatio*, Areteus). This tumefaction belongs less to the mucous membrane than to the sub-mucous cellular tissue. This border and the subjacent mucous membrane, bleed on the slightest touch. The development of the concretion is always more or less rapid; it is sometimes found established on parts which a few hours before appeared to be perfectly sound.

The concretion, at first adherent, finally detaches itself, and leaves float-

ing shreds surrounded by puriform matter; sometimes it forms submucous abscesses. The local disease, and the local effects produced by it, may be joined with general phenomena, adynamia, or convulsions.

1. Contagion supposes the existence of a morbid principle, whose origin is external. This principle can be only a miasm, a virus, or foreign body; all external morbid principles are necessarily arranged in one of these categories.

Is this miasm? The absence of initial fever is in conflict with this idea. Because all miasms introduced into the economy constantly determine febrile reaction, unless they instantaneously destroy life. They also provoke—in plague for example—buboes, which have been assimilated to the submaxillary engorgement of pseudo-membranous angina, and this has been adduced as an argument in favour of their miasmatic nature; but it is valueless, because this engorgement is met in some cases in which it is a result of mere vicinity, in the groin from excoriation of the foot, in the axilla from panaris.

Is it a virus? The absence of fever opposes the idea of a virus; for the reason that fever constantly precedes the appearance of virulent exanthemata. This law has no exception, be the virus what it may; be it introduced by respiration or inoculation. Only in the last case, it is on the point where there was a preparatory action entirely local; but fever always announces the general infection, and precedes the constitutional exanthema, whatever may have been the duration of incubation, whether some days, as in variola, or many months, as in syphilis or hydrophobia. We may add, this exanthema undergoes an anatomical evolution, which is never the case in the croupal affection, which appears from the start, and gives, if we may use the expression, both fruit and flower at the same time.

If this principle is neither virus nor miasm, it must necessarily be a foreign body.

Is this foreign body inert, acting like a thorn or cantharides, or is it an agent which reproduces itself? The extensive character of croup is opposed to the first supposition; cantharidal false membrane remains circumscribed.

This agent which reproduces itself must be animal or vegetable. We know that croupal affections are communicated most frequently without immediate contact with the patient, or with objects which he has touched, and that the communication is only consecutive to denudation of the cutaneous or mucous tegument.

This producing agent cannot be an animal parasite, because it is never deposited on the integument without contact; but, being provided with offensive arms, it requires no previous denudation to establish itself.

Therefore, it must be a vegetable parasite, sufficiently light to float in the air, which serves it as a vehicle; but incapable, with its rounded spores, to fix itself, unless it meets a locality unprotected and already prepared.

These two etiological conditions seem to indicate the vegetable parasite the more clearly, as we find them in all affections in which the presence of a vegetable is admitted as an efficient cause; that these affections are developed in man and other animals, on vegetables or on fruits.

Communication, without immediate contact, has been observed in the tineæ of the hairy scalp, of the face, or other parts of the body; in the muscardine of the silk-worm, in the *oidium* of the vine. Tegumentary denudation has not been indicated in the tineæ, the muscardine, or *oidium* of the vine; but the same is true as to cutaneous croupal affections, and it has not been observed because it has not attracted attention. The tineæ

prefer scrofulous subjects, who always have some cutaneous exanthema, following the operation of the razor, which rarely fails to raise the epidermis upon some points; the *oidium* of the vine appears, especially after bursts of sunshine, which tear the tissues swollen by humidity; the mould of fruits attacks only those whose envelopes have been injured by contusions, or punctured by worms. When mould is found in the interior of fruit, we are sure to find, on close examination, a communication with the outer air. Denudation has been observed in fishes, the integuments of which have been bruised in the nets (Ch. Robin, *Saprolegnia ferox*). Once planted, this mould very closely resembles croupal affections, and follows the same course.

Very expert microscopists have failed to discover anything but false membrane in croup; but their failure is attributed to the mode of seeking the cryptogam. If the subject be living, we must draw out the product or seek it among foreign matters with which it has been expelled; if the subject be dead, and the affection has endured some time, we should seek the parasite among the products he may have thrown about him, false membrane and pus.

The first subjects that came under observation were attacked not with croup itself, but with those affections described as putridity of the gums and gangrene of the mouth, in which there formed upon the mucous lining of the gums and cheeks a whitish, grayish, pultaceous matter, resting upon the mucous membrane, tumefied, livid red, eroded, bleeding on the slightest touch, and ending in true gangrene. This pultaceous matter examined under the microscope by Dr. Bazin, distinguished by his work on *tinea*, exhibited a very fine, pure, cryptogam and nothing else.

The existence of the mushroom or cryptogam in an affection which, since the time of Areteus, has been considered to be of the same nature as malignant angina, and since that of Mr. Bretonneau as forming the first link in the diphtheritic chain, which may extend from the buccal and nasal openings to the remotest ramifications of the bronchiæ, induced our author to hope to meet them at other points of the air-passage, and suggested to him that a cryptogam must be the cause not only of the gangrene of the mouth, of blistered surfaces, and of the vulva, met with in subjects attacked with croupal affections, but also of that which springs up on the surface of suppurating wounds, of hospital gangrene, and which behaves exactly like cutaneous croupal affections.

Subsequently concretions from the throat were examined by the microscope, and the cryptogam found, but more or less mixed with false membrane.

At length an opportunity was afforded to examine false membranes which had been expelled from the laryngeal tube through an opening made in the trachea. The first particles examined showed only false membrane; but other fragments of the same product exhibited the cryptogam, though lost amidst false membrane and globules of pus which enveloped it on all sides. Afterwards it was found more readily and less mixed a great many times in the larynx, and four times in the throat of subjects who had sickened after their brothers had died of laryngeal croup, and who were cured before the affection could pass the throat, thanks to prompt treatment. For a moment our author suspected he might have been deceived, and might have fancied that he saw spores in granulations or tubes in fibres of false membrane.

But he declares they were spores such as they have been described and figured by micrographers, such as they are seen in all moulds, rounded or oval, regular, isolated, or grouped in twos, threes, or a greater number,

glued end to end, or joined by a pedicle of more or less length, forming by their groupings chains, from which secondary chains were detached; these were, indeed, sporiferous tubes containing spores in their interior, and permitting some to escape from their free extremity; straight, curved, simple, or ramified tubes, articulated or not, interlaced, entangled, sometimes resembling bunches of gooseberries. There was, also, the empty tube or mycellium. There was no more regularity in the disposition of the tubes than in the grouping of the spores. Here the analogy to the mould of fruits continues. On the human moulds we may follow the evolution of the cryptogams; in their infancy and later on the surface spores only; then sporiferous tubes with or without isolated spores, with false membrane and compact mass; again to all this are joined empty tubes which predominate in the last period, and end sometimes by existing alone; it seems that all the rest had disappeared, as a dry fruit that has cast all its seed. Had any doubt remained now, it would have been dissipated by the work of M. Ch. Robin (*des végétaux, parasites, &c.*); others had previously observed cryptogams in the air-passages with false membrane.

Remak (1845) found ramified fibres of *thallus* in mucus detached from the velum palati of an infant dead from croup. (Ch. Robin, p. 513.)

Vogel states that upon true diphtheritic membranes exuded on the buccal or pharyngeal mucous membrane, the mushroom of thrush has sometimes been found, even when only some points or small white spots were formed.

A host of authors, Eudes Deslongchamps, Rayer, Montagne, Ch. Robin, and others, have found in subjects, men and birds, who had perished from chronic diseases, generally phthisis, throughout the air-passages, even the pleura perforated (M. Rayer), different cryptogams associated with false membranes. These products had been expectorated or were found at the autopsies. In the latter case the cryptogam was on the surface, then came the false membrane interposed between it and the mucous membrane, which was more or less altered, congested, and often ulcerated.

Of the observations referring to croup, that of Remak has been considered valueless by M. Ch. Robin, for the reason that it is incomplete, and does not speak of croupal false membrane.

That of Vogel has been assailed by M. Empis, who, never having found cryptogams on the false membranes which he examined under the microscope, declares that Vogel was deceived, and that he mistook for false membranes products which were not.

Observations, in which the coexistence of cryptogams and false membranes has been established, have not suggested the analogy which there may be between these cases and croupal affections. Besides, the cryptogam has been considered as an epiphenomenon, as a product developed consecutively to the false membrane which serves it as soil.

Dr. Jodin thinks those gentlemen took for the effect what he regards as the cause, and suggests that this difference arises from their modes of proceeding.

They commenced while he ended with the microscope; he had recourse to this instrument only to give to a rational theory founded upon the study of the disease, the support of a sensible fact. He set out to explore a new world without a preconceived idea, armed only with the microscope, and they permitted themselves to be controlled by the microscope, forgetting or not knowing that this instrument, like percussion or auscultation, is a good servant but a bad master; when the study of a disease is commenced with these physical means, and with their assistance an organic lesion is found,

there is great risk of treating lightly, if not omitting entirely, interrogations of the patient and the study of the whole of the disease.

Thus, says Dr. Jodin, is lost the only route which might lead to a knowledge of the evolution of the affection. And this has happened to the micrographers. They saw nothing beyond the field of the microscope, and they took the products in the order in which they were presented: the cryptogam on the surface, then the false membrane interposed between it and the mucous membrane, and concluded that the former put forth from the latter, just as they might say that a thorn planted in the flesh, grew from the surrounding pus, the formation of which it had provoked.

However, two circumstances, which they knew perfectly, ought to have enlightened them. 1. The oozing of animal mucous or semisolid matter which immediately determines the presence of the cryptogam, a fact recorded by M. Ch. Robin in several passages of his book; 2. Ulcerations of the mucous membrane remarked in all cases in which they have met the cryptogam. It is as clear as day, in the opinion of Dr. Jodin, that the oozing is the commencement of the false membrane, just as ulceration is the favourable condition for implanting the parasite. But with M. Ch. Robin, they considered the appearance of the cryptogam as an epiphenomenon.

Dr. Jodin declares that all his microscopic observations have given the same results: the proportion of false membrane relative to the cryptogam has always been great according to the duration of the affection, and inversely in recent cases. A concretion taken from the tonsils of a subject, ill less than twenty-four hours, and whose brother had perished fifteen days previously from a consecutive laryngeal croup, presented scarcely anything but cryptogam.

Since the existence of the parasitic cryptogam is demonstrated, we should inquire whether there are not many species of cryptogams which may cause the difference observed in the character of the material products, sometimes membraniform, sometimes gangrenous, as well as that of the effects on the economy, limited to the local affection, or showing itself by the phenomena of general intoxication. We do not know whether the microscope will ever reveal the reason of these differences; we know that in large mushrooms, the structure of which may be observed by the naked eye, it is, nevertheless, very difficult to distinguish the edible from those which are venomous.

Dr. Jodin designates all these parasitic affections under the generic term of *moulds*. In this family are found all the mucous or cutaneous affections in which a cryptogam is recognized as the generator. The mucous membrane furnishes thrush, gangrene of the gums, of the mouth, gangrenous angina, croup, croupal catarrh of the nasal fossæ. The skin will produce membraniform or gangrenous affections, whether concomitant or not with those of the mucous membrane, gangrene of the vulva, and even hospital gangrene, which has never been suspected to have such an origin.

If a monograph of the moulds were prepared, it would be necessary, according to Dr. Jodin, to establish genera, species, or varieties of this family, based upon the differences of their sites and the physical characters of the concretions; upon the absence or presence of the accidents of intoxication, &c. He gives the following division, based upon their situation:—

1st. Superlaryngeal moulds, comprising all those which invade parts situate above the larynx, the mouth, throat, and nasal fossæ.

2d. Laryngo-tracheal moulds.

3d. Cutaneous moulds.

Our author believes that this discovery of the cryptogam sheds light upon different points in the history of croup which have been hitherto inexplicable.

In etiology.—The reason why croup generally spares infants at the breast is, that the milk which they continually swallow forms a coat over the mucous membrane that prevents the cryptogam from taking root. It attacks, in preference, the scrofulous and convalescents from eruptive fevers, because the first have the tonsils hypertrophied, which arrests the cryptogams in their passage, and the second have the mucous membrane deprived of its protective epithelium.

In symptomatology, the absence of initial fever is natural, while the cause of the disease is altogether external. This absence of fever explains how croupal affections may be developed in a latent manner, even in parts accessible to sight; there is nothing to induce an examination of the affected parts, and parents rest in deceptive security until the moment of suffocation, which leads to the belief in sudden croups; the storm, after being abated by the expulsion or displacement of the false membrane, reappears with greater violence, and hence we have intermittent croup. But there is neither sudden nor intermittent croup; the disease is always progressive and permanent.

In diagnosis, the absence of fever becomes a most valuable sign, to which physicians and parents cannot give too much attention. Coexisting with alteration of voice and croupal cough, croup ought to be suspected, and induce an examination of the visible parts. On the other hand, the presence of fever with the same symptoms banishes the idea of croup, though not absolutely, because there may exist a symptomatic fever.

In no case, as a general rule, should an examination of the throat and all parts accessible to sight be dispensed with, and this at every visit, because we know how rapidly the moulds sometimes form.

In prognosis, the reproduction of false membrane being always possible, as long as a spore or reproductive germ exists, the expulsion of false membrane, and a cessation of suffocation, are not sufficient to announce a cure prematurely, which may be cruelly disappointed.

Dr. Jodin, with these views, of course condemns bloodletting in every form, and all antiphlogistic agents in the treatment. The only indication is to kill the cryptogam, and this is effected by topical applications alone.

Of all the parasiticide agents, and he mentions under this denomination, nitrate of silver, chlorohydric acid, sulphate of copper, corrosive sublimate, and actual cauter, Dr. Jodin prefers liquid perchloride of iron, because it completely penetrates the cryptogam, its action is limited to the surface, and it may be absorbed without danger.

Nitrate of silver, he says, forms on the parts first touched, a kind of crust, which prevents the agent from reaching the ultimate attachments of the cryptogam.

We have selected the perchloride of iron, he says, hoping that it would prove not only parasiticide, but a modifying agent of that hemorrhagic condition which constantly exists around moulds. Success has crowned our choice, and we boldly declare it has never failed us in the numerous cases in which we have applied it. Besides, it fulfils a second indication, for the reason that it immediately begets a necessity to spit, and consequently, the expulsion of the false membrane. After this expulsion, only a redness remains, which is dissipated at the end of two or three days, and gives place to the normal condition.

It is indispensable to the success of the operation, 1st, to exercise a cer-

tain degree of pressure on these concretions, because, it has been observed, that a parasiticide liquid which simply runs over them is of uncertain action; and, 2d, to reach all the mould; a point spared may become the source of a new growth.

Dr. Jodin hopes that the topical parasiticide applications will be received by all physicians, even the most circumspect, the moment they are convinced of the efficacy of these applications, as well as of their entire harmlessness.

After each application, the patient is to gargle with cool water, until the parts are entirely cleansed; or water, or a solution of chlorate of potassa may be injected.

The application of the parasiticide agent, in the laryngo-tracheal moulds is attended with great difficulty, but it is not insurmountable. The index finger, protected from the teeth of the patient by a wide metallic ring, reaches the epiglottis, and raises it, and serves to conduct a tube, the entrance of which into the larynx is known by the whistling of air, and through this tube injections may be made. The parasiticide agent thus reaches the concretion, but nothing is attained, unless the whole of it can be subjected to the necessary pressure. And as this cannot be effected, Dr. Jodin suggests that by tracheotomy an opening may be made through which may be introduced sponges charged with the parasiticide liquid, which may be moved upwards and downwards, to cleanse the air-passage.

He urges that tracheotomy is not in itself a dangerous operation. Patients perish, he asserts, not from the operation, or its consequences, but rather from the incessant progress of the membraniform concretion. They die, not because they have been operated upon, but because the operation was too long postponed, or all the benefits derivable from it have not been obtained. The opening in the trachea has been regarded only as a way for the expulsion of false membrane, but never for the introduction of parasiticide agents to prevent its reproduction.

But even when the remedy is thus applied, Dr. Jodin thinks the cure is not certain, because the mould may extend to the ultimate bronchial ramifications, and even if these were reached, death might be consequent upon complicating with pulmonary inflammation. Nevertheless, he claims that the means of certainly arresting the supra-laryngeal moulds is a grand discovery, because it is preventive of those of the larynx.

In cutaneous moulds, the application of the parasiticide agent is easy. Dr. Jodin invites the attention of surgeons to this mode of treating gangrene of the vulva, and hospital gangrene, which sometimes destroys the tissues with frightful rapidity. Although he has had no experience in these cases, he believes that the perchloride of iron will promptly effect cure, aided by substantial diet, generous wines, &c.

We have given succinctly and substantially the statements of Dr. Jodin, under the belief that they may interest our readers, and may be considered worthy of further examination, though we must confess that we are far from being convinced of the correctness of his conclusions. Indeed, it seems to us to be a strong objection to his views, as to the cause of croup, that the disease is most prevalent in the winter, and therefore under circumstances the most unfavourable for the diffusion of cryptogamic spores, or their germination.

W. S. W. R.

BIBLIOGRAPHICAL NOTICES.

ART. XIV.—*Transactions of State Medical Societies*:—

1. *Transactions of the Medical Society of the State of Pennsylvania at its Twelfth Annual Session, held in Philadelphia, June, 1860*, 8vo. pp. 181.
2. *Transactions of the State Medical Society of Indiana, at its Eleventh Annual Session, held in the City of Indianapolis, May 17 and 18, 1860*. 8vo. pp. 68.
3. *Minutes of the Eleventh Annual Meeting of the Medical Society of the State of North Carolina, held at Washington, N. C., April, 1860*. 8vo. pp. 26.
4. *Communications of the Rhode Island Medical Society, for the year 1860*. Providence, 1860, 8vo.

1. THE Transactions of the Twelfth Annual Session of the *Medical Society of the State of Pennsylvania*, held June, 1860, were opened by an address from the President, urging upon the members of the profession throughout the State the importance of systematic organization as one of the most effectual means for promoting in their midst the interests of legitimate medicine. When the subject is considered in all its true bearings, it would naturally be presumed that, in the furtherance of efficient medical organization, we should find every high minded, honourable physician ready to lend his aid. In the way of its accomplishment there are, it is true, in certain portions of Pennsylvania, serious difficulties. Difficulties originating partly from the condition of the profession itself—the want of the proper moral standing and the requisite scientific training in many of its members, and their bickerings and petty jealousies arising out of their supposed opposition of interests; and partly from the absurd prejudices and suspicions prevalent amid the community at large, too often excited, kept up, and augmented by irregular practitioners of every species, to subserve their own private interests. These difficulties, formidable as they confessedly are, could, nevertheless, be ultimately overcome were the work of organization to be undertaken properly, earnestly, and in good faith, by all who recognize the utility and importance of the profession of their choice, and who cherish the high aims and noble aspirations by which its members should always be actuated.

The first of the scientific papers is by Professor S. D. Gross, and is entitled “Practical Observations upon the Nature and Treatment of Prostatorrhœa.” This affection is marked by a discharge from the prostate gland of, generally, a thin, mucous character, dependent upon irritation, if not actual inflammation, of the component tissues of that organ. It is a disease which has not, heretofore, been described; the symptoms pathognomonic of it having been confounded, invariably, with those of certain chronic affections of the urethra or bladder. Prostatorrhœa is of rare occurrence in patients under twenty years of age, or in very aged subjects. Little definite is known in reference to its predisposing causes, nor are we well informed in respect to its exciting causes generally. In most of the cases which have fallen under the notice of Dr. G. the disease was traceable, either directly or indirectly, to venereal excesses, chronic inflammation of the neck of the bladder, stricture of the urethra, or other disease of the latter part. It is sometimes caused by disease of the rectum—hæmorrhoids, fissure, fistula, ascariæ, or the lodgment of some foreign body. A common cause of the disease is masturbation. Temporary attacks may be produced by the use of drastic purgatives, cantharides, spirits of turpentine, or by whatever else has a tendency to invite an undue afflux of blood to the prostate gland and neck of the bladder, or to the posterior portion of the urethra. Once established, the

disease will be kept up by intemperance in eating and drinking, frequent horseback exercise, etc.

The discharge in prostatorrhœa is, in general, more or less ropy, but perfectly clear and transparent; very rarely puriform or purulent. In quantity it varies from a few drops to a drachm or upwards in the twenty-four hours; it is usually most copious when the patient has occasion to strain at stool. Often a peculiar sense of tickling is felt in the situation of the prostate gland and along the entire length of the urethra. In many cases a lascivious, pleasurable sensation is experienced, and in others one of dropping, as if a fluid fell drop by drop from the prostate gland into the urethra. There are often present, also, a feeling of weight or of fatigue in the region of the prostate, anus, and rectum, or along the perineum, with, perhaps, some uneasiness in voiding urine, and a frequent inclination to micturate. Sometimes morbid erections occur, and lascivious dreams. The mind of the patient usually suffers the utmost disquietude; he fearing that the disease will entail upon him the most disastrous consequences.

Dr. G. points out the diagnosis of the disease from gleet or chronic diarrhœa, discharges of semen, and chronic cystitis, affections with which it is most liable to be confounded.

Although prostatorrhœa is often very obstinate, the prognosis in most cases is favourable. When, however, the mind of the patient deeply sympathizes with the local affection, as it so frequently does, especially in young men of a nervous, irritable temperament, there is no disease which, according to the experience of Dr. G., is more difficult to manage, or in which our efforts are more liable to result in disappointment.

In the treatment of the disease, the condition of the prostate and its associated organs, with the habits and state of health of the patient, being duly and carefully examined into, we shall find, if the patient is weak and his digestive powers are impaired, that tonics constitute our best remedies, particularly iron and quinine, with a nutritious diet, a glass of generous wine, gentle exercise in the open air, either on foot or in an easy carriage, but not on horseback. Dr. G. speaks favourably of the tincture of the muriate of iron, twenty drops, with ten drops of the tincture of nux vomica, four times a day. If the patient be plethoric, small doses, insufficient to nauseate, of tartar emetic, in a saline mixture, are useful. Drastic purgatives are to be avoided as well as condiments and high seasoned dishes.

"Among the most important topical remedies," remarks Dr. G., "are, first, moderate sexual indulgence, as a means of allaying undue excitement of the prostate and its associate organs; secondly, cooling and anodyne injections, or weak solutions of nitrate of silver and laudanum, or, what he generally prefers, Goulard's extract with wine of opium, in the proportion of from one to two drachms of each to ten ounces of water, thrown up forcibly with a large syringe three times a day, and retained three or four minutes in the passage. In obstinate cases, cauterization of the prostatic portion of the urethra, or even of the entire length of this tube, may be necessary, the operation being repeated once a week. The cold hip-bath should be used twice in the twenty-four hours; the lower bowel should be kept cool and empty; and if the disease do not gradually yield, leeches should be applied to the perineum and around the anus. Whatever plan may be employed, perseverance and an occasional change of prescription are indispensable to success. When there is deep mental involvement, hardly anything will effect a cure, or, more correctly speaking, it is almost impossible to induce the patient to believe that he is well, or that nothing serious is the matter with him. Under such circumstances, our chief dependence must be upon travelling and an entire change of scene and occupation. If the patient be single, matrimony should be enjoined."

The Report of the Committee on Meteorology is next in order. It embraces a series of tables showing the results of meteorological records kept at Harrisburg, Gettysburg, Somerset and Philadelphia, during the last quarter of the year 1859. The four counties, of which the foregoing towns are the capitals, which differ, it is remarked, much both in situation and elevation, may be said to represent the meteorology of the eastern and southern sections of Pennsylvania. The report from Adams County, made at an elevation of 625 feet above the sea, may not

unfairly represent the meteorology of that section of the State which comprises the southern tier of counties lying in the valley of the Susquehanna. The observations made in Somerset, at an altitude of 2195 feet, will prove a standard of comparison for the mountainous counties lying directly west of the preceding. Those made in Dauphin County, at an elevation of 300 feet above tide, will serve, in the absence of further statistics, for the towns lying in the more central portions of the great Susquehanna basin, while those taken in Philadelphia will equally answer for the alluvial portion of the Delaware basin lying within the State.

Reports follow from the County Medical Societies of Beaver, Bradford, Chester, Indiana, Lancaster, Lebanon, Mercer, Montgomery, Perry, and Philadelphia.

In the report from Beaver it is stated that the periodical fevers which, in former years, regularly made their appearance every autumn, have been superseded, in a great measure, by fevers of a continued type. While a decided change seems to have taken place in the character of nearly all diseases—they having all become less sthenic or inflammatory, and demanding for their cure a more stimulating and tonic plan of treatment. In the report from Bradford, on the other hand, we are assured that the predominant type of disease is there inflammatory, calling for, in the majority of cases, the lancet, mercury, and the antimonials, remedies which almost invariably are well borne.

In the report from Lebanon County, it is remarked that intermittent fever prevailed there to a considerable extent, and equally during the winter as the summer months—thus seeming to indicate a morbid influence other than of a miasmatic character. The disease manifested a remarkable tendency to relapse, being easily enough arrested by quinia, but requiring for its permanent cure Fowler's solution and other mineral tonics.

In the report from Mercer County, Dr. Griswold remarks, that a considerable change in the character of the diseases of that section of the State has been gradually taking place during the last two years. Fevers—except intermittents and the exanthemata—have not prevailed to the same extent as before, while the types and grades are markedly different. In the prodromic stage, he has found it difficult, if not impossible, to make a satisfactory diagnosis. Those cases which in their course assume a typhoid character, lack that uniformity in the succession of their symptoms formerly observed. There is decidedly less tendency to tympanites and diarrhoea, and more to epistaxis in all stages of the disease—even in that of convalescence. Most of the cases of fever begin with considerable chilliness, and some have decided paroxysmal chills; when these are broken by quinia the disease gradually assumes in many cases a continued form. There appears to have taken place a blending of the types of fever generally.

In the report from Montgomery County, we are told that the year 1859 was characterized by an absence of fevers as well as of epidemics. Even typhoid fever seems to have been rarely met with.

In the report from Perry County, Dr. Singer remarks that there has been an extraordinary freedom from the usual epidemic of that section of country. Since 1849, the year in which the Pennsylvania Railroad Company completed their road, bilious, remittent, and intermittents, have very nearly disappeared; which cessation of miasmatic diseases Dr. Singer very properly attributes to the draining of the marshy grounds so common along the Juniata flats by the construction of the road.

The foregoing observations, in connection with those reported to the State Society at its previous sessions, prove very evidently that, from various causes, some of which are evident enough, while others evade our closest scrutiny, many of the former endemic diseases in different sections of the State are rapidly disappearing, and where the same diseases still continue to prevail annually, as they did in former years, they have undergone, for the most part, a very decided change in their type or character.

Dr. Ross, of Darlington, Beaver County, describes a severe form of fever which prevailed as an epidemic along the valley of Little Beaver Creek, from the beginning of May to the end of December, 1859. The attack was preceded by a furred tongue, icterode sclerotica, and headache. Fever was ushered in with a chill, more or less severe. In a few cases, for three or four days, the fever

assumed the tertian intermittent type, and then most of the characteristics of the continued form for from three to four weeks before there was any abatement of symptoms. The remissions were always slight, mostly occurring in the morning. The chief symptoms of the disease were a full but not frequent pulse, often 80, seldom over 100; furred tongue, with undue redness of its edges; and, in all obstinate and fatal cases, the papillæ were covered to the point with a white fuzz, while the inter-papillary spaces were clean; conjunctiva injected; stupor, listlessness, and, in some cases, delirium. Some cases were attended with diarrhœa, others with constipation. Convalescence rapid, a perfect restoration to health taking place in from three to four weeks. In one-fourth of forty cases attended by Dr. Ross, enteritis was present. In two of the fatal cases it was extensive, in other two fatal cases there was cerebral congestion.

The fever was treated by gentle continued purgation with hydrarg. cum creta and rhubarb. In a few cases ptyalism ensued, when the mercury was discontinued. About the expiration of the second week small doses of quinine were tolerated; these were continued during convalescence. When dryness of the tongue supervened, spirits of turpentine in emulsion had a good effect, especially when there was constipation, the stools when obtained being dark coloured. In two cases of an intermittent quotidian type, the disease was arrested in a few days by full doses of quinine. In two other cases the symptoms were identical with those of typhoid fever. Nauseants or opiates were not tolerated in any case. The disease proved fatal in about one-tenth of those attacked. This fever prevailed only on low grounds where there was impure stagnant water. Here *only* was it communicated to the attendants on the sick; the greatest cleanliness and best ventilation furnished no protection; while on high grounds, with pure water, under no circumstances was the disease known to spread from the sick to the well.

In the report from Bradford we are informed that, at the meetings of the Medical Society of that county, an hour is set apart for a clinic. The idea, remarks Dr. Holmes, by whom the report is drawn up, of a clinic in a county society may, perhaps, appear somewhat novel to some; nevertheless, it has thus far succeeded remarkably well in our society. By granting to the inhabitants the privilege of bringing obstinate and chronic cases before the society to be examined and prescribed for free of charge, an interest and feeling in the community favourable to the profession has been excited that could have been procured in no other way, and is fast doing away with the idea at first prevalent that the meetings of the medical society were solely for selfish and pecuniary motives. Even to the members themselves these clinics, when regularly and judiciously conducted, must necessarily prove beneficial. A physician and surgeon are annually appointed to take charge of them, and a regular record of the cases presented, with their symptoms, treatment, progress, and termination, is kept.

The report from Chester County is devoted entirely to a biographical notice of Dr. James H. Bradford, who was born in Philadelphia, November, 1802, and died at St. Augustine, Florida, April, 1850, aged 48 years.

The report from Indiana is a very full and elaborate one. It embraces an excellent sketch of the hydrography, topography, geology, meteorology, and mortality of the county, with a brief notice of the prevalent diseases. It is accompanied with a well drawn and excellent geological map.

From the strictly medical department of this report we learn that intermittent fever is rarely met with in Indiana County, while remittent fever was, during 1859, more prevalent than in former years. It is confined mostly to a few localities. It is occasionally severe, in a few instances fatal. Generally, however, it terminates favourably when promptly treated. Typhoid fever is a common disease in the county. It prevails, to a greater or less extent, every year; commencing, generally, early in the summer, and continuing throughout the autumn, and sometimes into the winter. It is not considered contagious, nor is it very fatal. Dr. Anderson, the author of the report, has observed the first cases in the season to commence on the east side of streams, and, occasionally, where there was but a small quantity of pure fresh water, but more or less moisture on the west side of the residences. In a few localities contiguous to mill-dams or ponds of water, it is frequently marked by remissions, simulating remittent

fever. According to Dr. Stewart, of Greenville, during an epidemic of typhoid fever in that place, in the best marked cases of the disease, a few doses of sulphate of quinia would cause it to assume a remittent form.

Dr. Anderson is in the habit of treating typhoid fever with small, one grain doses of sulphate of quinia every three or four hours, in conjunction with such remedies as are required to control the local and general disturbances. Since he has adopted this plan of treatment he has seldom lost a case of the fever when occurring in good constitutions and without complication.

"In May, 1858, there were a few cases of typhoid fever on a branch of Mahoning Creek that assumed a very malignant form. About a week from the commencement of the attack, on some part of the surface, a severe burning sensation would be felt, very painful to the touch; in a few hours it would become red, then purple, and, shortly after, black; generally, not more than twelve hours from the time the pain was first felt the part would begin to slough, and even the bones would exfoliate. Most of the cases died. A great many remedies were tried, but the treatment that appeared to mitigate the symptoms most was quinia and spirits of turpentine, pushed as far as the patients would bear, with anodynes sufficient to allay pain and procure rest. Some of the patients lived until portions of the body sloughed and dropped off."

No mercurial was used in any of the cases.

Consumption, Dr. A. informs us, has been increasing rapidly in Indiana County during the last few years. A large number of deaths occur from it annually. Most cases are hereditary, a few seem to be traceable to neglected colds, etc. The rapidly increasing hereditary tendency to the disease in the county is truly alarming. Scrofula is also a common disease; it is in most cases hereditary, and usually eventuates in pulmonary phthisis.

The report closes with a very pointed denunciation of the habitual use of tobacco, as a cause, if not in most instances of positive disease, of at least a very decided impairment of the health of the organism, which, it is believed, may, and most usually is transmitted from parent to child.

The subject of the report from Lancaster County is a biographical notice of Dr. William Henry Boone, who was born in Chester County, Pa., August 9, 1826, and died at Lancaster, December, 1859, in the 34th year of his age.

Acute rheumatism, we are informed in the report from Lebanon County, attended, in almost every case, with grave cardiac complication, prevailed to a great extent in that county, particularly during the winter of 1859. Of 13 cases, eight were complicated with either peri or endocarditis, with, in most instances, effusion. Four cases were either complicated with rheumatic ophthalmia, or the latter existed as the sole disease. A case is related to show how rapidly in the course of rheumatism hypertrophy of the heart may ensue. The patient was a blacksmith. The entire duration of the attack was twenty-two days. In the first week, the case was one of acute articular rheumatism. With the second week pericarditis set in, with a full, hard pulse, indicative of inflammation of the muscular tissues of the organ, with the most tumultuous action. Copious blood-letting was twice resorted to, at an interval of four days, without any perceptible impression being made upon the pulse. Digitalis was found to produce no beneficial effect. Veratrum viride, to the amount of twenty drops, given in divided doses, in the space of five hours, reduced the pulse from 144 to 65, and held it so for twenty-four hours. The remedy being discontinued, in consequence of excessive gastric irritation, the pulse soon rose again, and continued full and bounding until within twelve hours of death. Calomel did not produce its usual beneficial effects; it failed to salivate. A post-mortem examination revealed enormous hypertrophy of the heart. Over the whole right side of the organ the pericardium was closely glued to its surface by a layer of very firm lymph several lines in thickness, and arranged in a reticulated form. On the left side, the pericardial cavity was free, and contained upwards of a pint of fluid. The walls of both ventricles were very much thickened—those of the left measuring more than an inch.

Previous to the attack of rheumatism, the patient, we are told, had always enjoyed excellent health. Dr. Schneck believes that the hypertrophy of the

heart could not have been of any long duration without his having detected it, if it existed at all prior to the rheumatic attack.

In the report from Montgomery County, we are informed that Dr. Poley, whose interesting observations on the use of digitalis were noticed in our review of the last two or three volumes of the *Pennsylvania Transactions*, reiterates his confidence in the value of that article when given in doses sufficiently large to bring the system quickly under its influence. His common dose to adults is a teaspoonful of the tincture every hour or two, until the pulse is reduced below the normal standard. To even the smallest children he gives it with great confidence, and in doses which, to those "who go by the books," would seem truly hazardous. He has, however, inspired in his brother practitioners so much confidence, that they also give now much larger doses than formerly.

It is remarked by the authors of this report, that the use of digitalis and of veratrum viride, though highly approved by the members of the Montgomery Society, does not appear to have lessened the use of the lancet. On the contrary, it has tended to disprove the too prevalent belief that diseases have so greatly changed—become so much less inflammatory—that they will not bear depletion. It is argued, if the system will bear for days the prostration induced by large doses of veratrum viride and the like remedies, and react promptly when these are discontinued, we surely cannot fear that it will sink under the loss of a few ounces of blood; and facts confirm the correctness of such reasoning. Some members of the Montgomery Medical Society, who have been in practice for more than a quarter of a century, still use the lancet freely, and find that depletion by it is as well borne as it was thirty years ago. Many patients, particularly among children, are, it is believed, lost in consequence of its neglect.

The report closes with brief biographical notices of deceased members.

We pass over without comment the report from Philadelphia County. It is made up simply of an abstract of the more elaborate report on the meteorology and epidemics of Philadelphia for 1859, presented to the College of Physicians of Philadelphia, by Dr. Wilson Jewell, which latter report was published *in extenso* in a former number of this journal.

Appended to the report are biographical notices of members deceased during the past year.

2. The *Transactions of the Eleventh Session of the State Medical Society of Indiana* commence with the "Presidential Address" of Dr. D. Hutchinson, of Mooresville.

The first of the professional papers is on "Artificial Lactation," by Dr. C. M. Wetherill, of Lafayette, Ind. The leading object of the writer is the investigation of the qualities requisite to adapt the food with which the infant is supplied, when from any cause it is deprived of the mother's milk, for its proper and adequate nourishment.

The milk of a healthy cow is the food which Dr. W. believes to be best adapted to this end. To assimilate it as nearly as possible to the milk of the human female, he recommends the preparation of a mixture of sugar of milk with a small quantity of alkaline salts—a phosphate, if necessary—and chloride of sodium; to be prepared in such a manner that one teaspoonful of the mixture can be added to a certain quantity of pure fresh cow's milk, or milk and cream, with the addition of water, according to the age of the infant to be nourished. This artificial milk to be taken from a biberon.

We have little faith in artificial food of any kind for the proper nourishment, development, and growth of a young infant, or in any of the means that from time to time have been suggested to render artificial a substitute for natural lactation. With Dr. Meigs, we hold that it is "a sacred duty," whenever it can possibly be accomplished, to provide the new-born infant, if from any cause it be deprived of its mother's milk, with the milk of a human nurse, in order to save it from the risk of distressing illness, and of even speedy death, by which the great majority of such as are attempted to be nourished by the spoon or biberon are threatened.

The next paper is on "Medical Inhalation," by Dr. T. W. Fry, of Crawfordsville. After a general sketch of the opinions of medical men, by whom the

subject has been examined, in respect to the value as remedial agents, in certain diseases, of substances inhaled into the lungs, Dr. F. presents a brief summary of his own experience, illustrated by the history of several cases of pulmonary disease treated by the inhalation of medicinal substances. For several years past he states that he has resorted to this mode of medication in all the affections of the lungs, from the slightest irritation produced by an attack of simple catarrh, through all the several gradations of pulmonary disease, including the formation and softening of tubercular depositions. In the first and last stages of pulmonary consumption he has had recourse to inhalation; sometimes with marked benefit, at others with only temporary relief or a slight alleviation of the severe suffering under which the patients often labour.

We think it not at all improbable that certain substances in the form of vapour may be administered by inhalation as a means of soothing and quieting certain prominent and troublesome symptoms which occur in many of the forms of chronic disease of the lungs. We place no confidence in them, however, as, strictly speaking, curative means. How any very important therapeutic agency can be exerted in cases of tubercular phthisis by substances inhaled into the lungs we are unable to understand, with our present views of the pathology of the disease.

The article next in order is the "Report of the Committee on the Progress of Medicine," by Dr. J. H. Brower, of Lawrenceburg. A considerable portion of this report is occupied by an abstract of some of the more recent observations on the subject of diphtheria, a disease which, in consequence of its prevalence within the last two years in different portions of Europe and of the United States as an epidemic of a particularly fatal character, has become one of deep interest to every medical practitioner. But this is so fully discussed in a preceding article in this number (*see p. 145 et seq.*), that we need not dwell on it here.

The next report is one on the same subject—diphtheria. It is drawn up by Dr. R. E. Haughton, of Richmond, Ind. Diphtheria, it appears, has prevailed during the last two years as an epidemic in various sections of the State of Indiana, and has been marked by a considerable amount of mortality.

Dr. Personett, of Wayne County, Indiana, had seen, we are told, much of the disease. In the report of his observations he describes it under *four* different forms, which would appear, however, to differ from each other only in their different degrees of severity.

In the first of these forms the approach of the disease is insidious. There is experienced at first a soreness about the fauces, with slight stiffness of the neck, and pain along the Eustachian tube; there are, generally, a little cough, tickling about the epiglottis, dryness of the throat, and more or less thirst. One or both tonsils are swollen, the mucous membrane is red, its superficial vessels greatly injected, and its surface covered with a thin, transparent, shining pellicle. A glairy, ropy, tenacious fluid is secreted by the fauces, and with difficulty ejected from the mouth. The pellicle generally retains its transparency throughout the attack; occasionally it becomes so dense as to appear white or yellow. When confined to the tonsils or uvula, it is of a dirty white colour. When caused to separate by the application of nitrate of silver, the surface beneath is left clear, smooth, and of a healthy appearance. This form usually does well under domestic treatment. The patient is well, generally, in a few days; some cases, however, linger for eight or ten days; occasionally it runs into one or other of the more severe forms.

In the second form we have the same symptoms intensified. There is malaise for two or three days; chills and heat rapidly succeed each other, with headache, red, swollen, and suffused eyes, stiff and sore neck, swelling and pain of cervical glands and about the ears, heat and dryness of surface, severe pain of lower extremities and along the spine. Urine scanty, high coloured, and often turbid and ropy. Bowels regular, occasionally constipated. Some cases exhibit but slight febrile reaction; others are attended with fever of an intermittent, remittent, or adynamic type. Often the appetite is but little impaired; sometimes there is a tender epigastrium, with vomiting of a mucoid fluid tinged with bile, and containing shreds of, apparently, organized lymph. The throat, early in the attack, is hot and sore; deglutition, or any motion of the throat, is painful. The

faucial mucous membrane soon becomes of a deep red colour, and covered with a layer of transparent mucus, while a tenacious ropy fluid clogs up the throat, and extends in bands across the pharyngeal chamber. The deposit on the mucous membrane becomes more abundant, forming a firm pellicle, of a dirty white, yellowish, or grayish colour. Sometimes the deposit occurs simultaneously over the entire surface of the throat; at others it commences in small patches or points, which expand rapidly, coalesce, and often present the appearance of an ulcer. Not unfrequently, in a few hours from the first appearance of the patches or specks, one or both tonsils will be covered with a whitish, yellow, or gray incrustation, surrounded by a red, elevated, oedematous border, simulating the appearance of an actual ulcer. But when the pellicle is thrown off, the mucous membrane is found to be smooth and intact. From these surfaces a bloody serum occasionally oozes. Early in the attack the tongue is narrow, pointed, and covered with a thick yellow coat, through which the papillæ protrude in florid, shining points. When properly managed, the disease in this form generally terminates favourably. The faucial deposit is repeatedly thrown off and renewed, at each renewal becoming thinner. If the case is neglected or improperly treated, it may run into the third form.

The symptoms of the third form are the same as in the former, but of a more extended and intense character. It most generally attacks with a chill followed by severe fever, which seldom abates within forty-eight hours, and frequently continues as long as the disease. There is severe pain of the head, limbs, and back. The cervical glands and neck are much swollen. Deglutition is rendered difficult, and in the attempt to swallow liquids a portion is returned by the nares. The extension of the false membrane into the larynx gives rise to stridulous breathing and a hoarse cough. The voice sinks to a whisper, and finally becomes extinct; and if speedy relief is not given, the patient dies of mechanical obstruction to respiration. Sometimes in this form there is an extensive formation of false membrane upon the fauces before any evidence of severe illness is apparent, and death from suffocation occurs within a few hours after the true character of the attack is recognized. Many linger, with deceitful changes, for ten or fifteen days; others die after a relapse; while others recover, under very unpromising circumstances, by the aid of a supporting treatment. In the course of the disease, bronchitis, manifest from its onset, or pneumonia, insidious in its approach, may occur. Both must be met by a prompt and judicious treatment, or a fatal result will ensue.

In the fourth form of diphtheria the condition of the throat is such as to acquire for the disease the appellation putrid, malignant, or gangrenous. The whole of the fauces is black, or, more strictly speaking, covered with a soft blackish or black exudation, which exhales a fetid odor. A fetid sanies is discharged from the nostrils; the cervical glands are greatly swollen, and the integuments over them of a dark red or mahogany colour; suppuration of the entire anterior portion of the throat often takes place, and the matter infiltrates the cellular membrane to a great extent; when openings occur, there is a discharge of an unhealthy, acrid sanies, mixed with flakes of dead membrane. Sometimes extensive sloughing happens, laying completely bare the trachea, cervical glands, and bloodvessels. Sometimes the disease extends from the pharynx into the posterior nares, and involves finally all the soft parts between the roof of the pharynx and base of the brain. This form of the disease is very generally fatal.

Dr. P. treated seventy cases of diphtheria. Three only terminated fatally; two of croup, and one of hemorrhage consequent upon sloughing of the soft parts in the neighbourhood of the posterior nares.

The Committee on Medical Education presented its report, through its chairman, Dr. C. Fishback, of Indianapolis. It is a short and sensible paper, but presents no points of a novel or very striking character.

The *Transactions* close with a report by Dr. John Rea, on behalf of the New Castle Medical Society, comprising an abstract of cases of disease treated during 1859, as reported to the society at its meeting of April 6, 1860. The cases are none of them given in detail. The report states that the treatment adopted in diphtheria was uniform among the members. It consisted of mild cathartics, quinine, and the local application of nitrate of silver to the throat. Chlorate of

potassa was also used locally and internally. Some of the cases were attended with an eruption; in some the air-passages became implicated, which greatly increased the danger. Out of 211 cases of diphtheria treated, two deaths only are reported to have occurred.

3. From the *Minutes of the State Medical Society of North Carolina*, at its session of 1860, we learn that, in addition to the address of the retiring President of the Society, Dr. William T. Howard, "On the True Utility and Dignity of the Medical Profession: some of the Means for its Culture and Improvement, and the Proper Aims and Aspirations of its Members," a number of interesting papers and cases were presented, all of which were ordered for publication in the *Medical Journal of North Carolina*, a periodical belonging to, and under the control of, the State Society.

4. *The communications of the Rhode Island Medical Society* embrace, in addition to the annual address of the president, a paper on tetanus and its treatment, and the history of a case in which a foreign body was retained in the air-passages for twenty-three years. To the communications are appended the minutes of the semi-annual meeting of December, 1859, and of the annual meeting of July, 1860, with a list of the officers, &c., of the society.

The excellent address of the president, Dr. J. James Ellis, of Bristol, is mainly occupied with a consideration of the subject of cell-formation in reference to the views now held by the most authoritative physiologists. Dr. Ellis lays no claim to originality in what he advances; his exposition of the manner in which cells are formed, the relation of the nucleus to animal cells, and successive transformation of cells, is founded altogether upon the recorded microscopic observations of others.

The paper on tetanus was communicated by Dr. Henry Turner, of Newport. It comprises remarks, chiefly on the treatment of tetanus, based upon observations made during a practice of twenty-two years, during which he has seen rather more than an ordinary proportion of cases of this formidable disease. He presents a list of 16 cases, namely: 12 of traumatic tetanus, of which 4 recovered and 3 died; 2 of idiopathic tetanus, of which *one* recovered and *one* died; and 2 cases of *trismus nascentium*, both of which died; making in all 5 recoveries and 11 deaths. Thus we perceive that of the cases of traumatic tetanus, 33 *per cent.* recovered; of the cases of idiopathic tetanus, 50 *per cent.*; while all the cases of locked jaw in the infant terminated fatally.

The cases referred to by Dr. T. were nearly all of them under the care of different medical gentlemen, and in consequence there was a considerable variety in the treatment pursued. In some nearly every remedy was tried that could possibly suggest itself to the mind of the practitioner as adapted to the disease. In all cases was opium in some form or other administered, and as we should infer the most freely in the cases that had a favourable termination. Opium, the experience of Dr. T. has convinced him, is our "sheet anchor" in tetanus. Although he would not neglect such adjuvants as the warm bath and anæsthetics, he would in no case omit "the early and persistent application of such doses of morphia as would, in a great measure, control the extreme symptoms."

The case of the residence of a foreign body in the air-passages for twenty-three years is related by Dr. J. H. Eldredge, of East Greenwich. The subject of it was the wife of a ship-master of New York. In apparently the last stage of pulmonary consumption she came to Rhode Island to pass in quietness the short remaining period of her existence. One night, about four days before her death, she discharged, during a violent fit of coughing, mixed with blood and purulent matter, a portion of metal, which, upon examination, was found to be a brass tip for one of the ribs of an umbrella. When first discharged it was an inch long, and a quarter of an inch in diameter at the larger end, which was hollow, and something less at the smaller end, which was rounded and solid. It was so much corroded that the thin parts soon crumbled away, reducing it to less than half its original size.

When the lady was but five years old she caused a disturbance in school by whistling through the umbrella tip, and to conceal it from her teacher tossed it

hastily into her mouth, and with such force as to cause it to pass through the glottis into her windpipe. It lodged in the air-passages, where it remained for twenty-three years. It gave rise at first to the most distressing symptoms, and for several years, whenever it changed its position, it gave rise to a violent attack of pulmonary inflammation. Finally, it became permanently fixed, and ceased to annoy her. She would occasionally amuse herself and companions by whistling through it, which she did by forcibly drawing in her breath. The lady was married when she was twenty-one years old, and had one child. She made several voyages with her husband, and enjoyed a fair amount of health until about eighteen months before her death, when a troublesome cough manifested itself, and soon all the symptoms of pulmonary consumption became developed, and she died in the twenty-eighth year of her age. Her family were predisposed to consumption. Her mother and one sister died of it. She was tall and slender, with light hair and complexion, and evidently of a scrofulous diathesis. It is, therefore, by no means certain that the presence for so many years of the foreign substance in the air-passages had any decided influence in hastening her death.

D. F. C.

ART. XV.—*Reports of American Institutions for the Insane.*

1. *Of the New Hampshire Asylum, for the fiscal year 1859-60.*
2. *Of the King's County (N. Y.) Asylum, for the fiscal year 1859-60.*
3. *Of the Maryland Hospital, for the years 1858 and 1859.*
4. *Of the South Carolina State Asylum, for the fiscal year 1858-59.*
5. *Of the Northern Ohio Asylum, for the fiscal year 1858-59.*
6. *Of the Hamilton County (Ohio) Asylum, for the fiscal year 1858-59.*
7. *Of the Indiana Hospital, for the fiscal year 1858-59.*

1. THE time for the termination of the official year of the *New Hampshire Asylum for the Insane* having been changed from the first of June to the first of May, the report now before us from that institution embraces a period of but eleven months.

	Men.	Women.	Total.
Patients in asylum, June 1, 1859	94	88	182
Admitted in course of the year	47	38	85
Whole number	141	126	267
Discharged, including deaths	46	37	83
Remaining, May 1, 1860	95	89	184
Of those discharged, there were cured			38
Died			17

Causes of death.—Chronic insanity, 6; general paralysis, 2; tubercular consumption, 3; paralysis, 2; typhomania, 2; congestion of lungs, 1; epilepsy, 1; suicide by submersion, 1.

Dr. Bancroft makes the following remarks upon the subject of "treatment:"—

"Either as the cause of the mental disturbance, or the effect of the continued operation of disturbing mental or moral influences, disordered physical function is generally found to exist. Whatever the functional disturbance may be, its correction is an early object of attention, and this is not unfrequently followed by immediate restoration to mental health.

"In the moral treatment of the insane our experience has not led to the adoption of the extreme of any exclusive system of views. A constant effort is made to confine mechanical restraint within the narrowest possible limits, and to give to the patient the largest liberty and indulgence consistent with his welfare; and yet it has not been found practicable to discard, indiscriminately, all mechanical restraint, being satisfied that it would involve exposure to the alternative of equal if not greater evils.

"From this point of view the question of restraint is decided, and never used unless its use is more humane than its rejection. And yet we would not allow

our reluctance to its use to deprive any patient of its protection and benefits, when it is manifest that there is no substitute of equal value. That instances have occurred within the year, in which, for delicacy and tenderness of control, as well as safety, it surpassed all other possible means, I have no doubt. But beyond these peculiar cases, every consideration urges to the policy of securing order and propriety of conduct among the insane by an appeal to self-respect, and the use of the various diversions which are within our reach.

"The appliances with which we are furnished have been diligently used to occupy the attention, and afford exercise and entertainment to patients. Among these the bowling-alley affords the most substantial and enduring satisfaction. A new carriage has been purchased during the year, which adds much to the comfort and pleasure of the drives for the female patients."

Every humanitarian enterprise embracing within its scope the masses of the people, should, if possible, be so devised and so organized that its beneficent agency shall not fail to reach the parties to whom its aid is the most absolutely necessary. Wealth may purchase assistance; poverty must look abroad for it. Hence the States which, in the foundation of their hospitals for the insane, provided that the whole expenses of those establishments should be defrayed from the treasury of the commonwealth, have set a noble example by placing the benefits of hospital treatment easily within the reach of the humblest individual in the land. They are as easily accessible to the poor as to the rich. But, in many of the States, admission to the hospitals can be gained only by payment. The consequence is, that many paupers, who are proper subjects for treatment at those institutions, are debarred from it. In allusion to this subject we copy from the report the subjoined extract:—

"The spirit of economy which regulates the financial operations of many towns, declines to afford to their poor insane the benefits of hospital care and treatment, at an expense exceeding the cost of the support of ordinary paupers at the almshouse. This policy recognizes only the question of subsistence, leaving out of view the fact that the insane poor have wants to be provided for, and claims upon the community for care and protection, which do not pertain to ordinary poor.

"Additional wants spring from the fact of insanity, which are not and cannot be supplied at the almshouse; and to disregard these necessities in the question of support is not less derogatory than to ignore the obligation to provide for bodily subsistence. Most of the insane at almshouses, in order to insure the safety and convenience of others, are subjected to an amount of confinement which renders even their outward condition materially less comfortable than that of ordinary paupers; while, morally considered, the insane pauper has a claim upon the community for that additional and special care which the loss of reason and self-control unfits him to exercise for himself. Affording subsistence is but a fraction of what may be done for the benefit of the insane, and the legitimate demands of humanity cannot be satisfied without giving them, in the care and attention of others, the best and only substitute for the self-control of which misfortune has deprived them."

As a partial compensation for the fundamental fault in the organization of the asylum, the legislature of New Hampshire appropriated, in 1858, twelve hundred dollars to the relief of the indigent, now pauper, inmates of the asylum; and again, in 1859, an equal sum, to be divided, one-half to those who were supported by towns, the other to private or pay patients.

Whole number of patients, 1843 to 1860	3845
Discharged recovered	765
Died	189

2. The exceedingly rapid growth of the city of Brooklyn, New York, has been accompanied, as might naturally be inferred, with a corresponding augmentation in the number of the insane within the county in which it is situated. This increase has been so great that the extensive buildings of the *King's County Lunatic Asylum*, erected but a very few years since, are already crowded to inconvenience, and, to meet the still enlarging demands, the Board of Supervisors have decided to enlarge the establishment by the construction of two additional wings.

	Men.	Women.	Total.
Patients in the asylum, July 31, 1859	122	168	290
Admitted in course of the year	84	96	180
Whole number	206	264	470
Discharged, including deaths	80	82	162
Remaining, July 31, 1860	126	182	308
Of those discharged, there were cured	45	42	87
Died	14	12	26

The patients at this institution represent nearly as heterogeneous a mass of nationalities as those of the New York City Lunatic Asylum. Of the 308 remaining at the close of the fiscal year, 216 were foreigners, and only 92 Americans. Of the latter, 75 were natives of the State of New York. Of the former, 140 were from Ireland, 44 from Germany, 22 from England, and the rest from six other countries.

Dr. Chapin says: "There is no kind of diversion that seems so well calculated to advance the recovery of the insane, and contribute at the same time so much to their enjoyment, as musical entertainments."

The patients' library, commenced in 1858, was increased in the course of the last year by contributions, and by the purchase, with an appropriation made by the Board of Supervisors, of 118 volumes of Harper's "Family Library."

3 The report for 1858 of the *Maryland Hospital for the Insane*, relates to a period of thirteen months.

	Men.	Women.	Total.
Patients in the hospital, Dec. 1, 1857	58	52	110 ¹
Admitted in course of the year	70 ²	18	88
Whole number	128	70	198
Discharged, including deaths	71	19	90
Remaining, Dec. 31, 1858	57	51	108
Discharged cured { from insanity	12	4	16
{ from mania-à-potû	25		25
Died	6	4	10

The number of patients supported at public expense being limited to sixty, many of this class for whom admission is requested cannot be received. Some applicants have been compelled to wait "more than two years" for vacancies which they might fill. This condition of things cannot fail to be attended by the most disastrous consequences to many persons afflicted with insanity. Treatment is postponed until the disease has become chronic, and, in but too many cases, incurable.

Dr. Fonerden closes this brief report with an account, which we extract, of a liberal donation to the institution under his care.

"It was the good fortune of the Maryland Hospital to receive, last April, a generous gift of three thousand seven hundred and fifty dollars, from Dr. Wm. Fisher, who was for ten years Resident Physician of the Hospital. The board immediately set it apart as the 'Fisher Fund,' to be used for specific purposes; and, subsequently, in agreement with this intention, voted to appropriate a part of it to the introduction of gas, for the sake of giving a more cheerful light to the patients."

We proceed to the report for 1859.

	Men.	Women.	Total.
Patients at the beginning of the year	57	51	108
Admitted in course of the year	79 ³	20	99
Whole number	136	71	207
Discharged, including deaths	82	19	101
Remaining at the end of the year	54	52	106
Discharged recovered { from insanity	16	6	22
{ from mania-à-potû	46	1	47
Died	9	4	13

¹ Two of these had mania-à-potû.

² Twenty-six of these had mania-à-potû.

³ Forty-six of these had mania-à-potû.

"The Committee on Repairs have expended another portion of the 'Fisher Fund,' for the purpose of introducing hydrant water. There could not have been a more judicious use made of Dr. Fisher's gift, than that which has so generously supplied the hospital with these two household blessings—light and water."

The old and, as we believe, always injurious practice of permitting, upon specified days, and without discrimination, all persons who, by what motive soever impelled, may seek a visit to the interior of an institution for the insane, to pass freely through the wards and among the patients, was continued at the Maryland Hospital until June, 1859. It was then prohibited by a resolution adopted by the Board of Directors. "The adoption of this resolution," says the report, "has relieved the hospital of a very great annoyance."

The office of Assistant Physician to the hospital was created in June, 1859, and filled by the appointment of Dr. James A. Stewart.

4. From the report for 1859, of the *South Carolina State Lunatic Asylum*, it appears that since we last passed under review the annual publication of that institution, a new edifice, intended, as we suppose, eventually to supersede the old one, has been commenced. A portion of one wing has been completed, and the male patients transferred to it from their former quarters.

	Men.	Women.	Total.
Patients in the asylum, Nov. 1, 1858			184
Admitted in course of the year			63
Whole number	124	123	247
Discharged, including deaths	29	24	53
Remaining, Nov. 1, 1859	95	99	194
Of those discharged, there were cured	12	13	25
Died	14	5	19

Causes of death.—Chronic diarrhœa, 8; consumption, 3; inanition, 3; paralysis, 1; disease of heart, 1; diseased heart and kidney, 1; hydrothorax, 1; tetanus, 1.

From Dr. Parker's remarks upon treatment, both medical and moral, we make the following extracts:—

"Medicinally, but little is done beyond the relief of such diseases as are common to the human family, irrespective of disordered intellection. Our long experience with the many forms and degrees of insanity has clearly convinced us of the dangers of indiscriminate and routine medication. The copious and repeated venesection, active purgation and emesis, and the host of other heroic remedies suggested by the early writers on insanity, have fortunately passed into merited disrepute, and are religiously ignored by every scientific and conscientious man. * * * * When nature requires assistance, and when no idiosyncrasy or other contra-indicatory conditions exist, the agents of the *materia medica* are our reliance and sheet-anchor; and under their benign and soothing influences we are often rewarded with the most flattering success.

"The effects of the shower-bath have not been abandoned, but its uses are restricted to the cases where its sedative and soothing influences are clearly demanded; and it is never used as an instrument of torture. * * * * The library and reading-room are places of favourite resort. * * * * Draughts, cards, chess, bagatelle, and ten-pins are popular pastimes, and oftentimes beguile, with permanent good effect, the morbid fancies of the insane. Music, drawing, painting, fancy work, and embroidery still occupy attention, and many among us have obtained, in these pursuits, a high degree of perfection. Several of our inmates, with commendable zeal, have devoted themselves to the higher departments of knowledge, embracing law, theology, medicine, politics, poetry, astronomy, and mathematics."

5. Our usual formula of statistics, as filled from the report of 1858-59 of the *Northern Ohio Lunatic Asylum*, is as follows:—

	Men.	Women.	Total.
Patients in the asylum, Oct. 31, 1858	80	75	155
Admitted in course of the year	57	63	120
Whole number	137	138	275
Discharged, including deaths	71	66	137
Remaining, Oct. 31, 1859	66	72	138
Of those discharged, there were cured	36	21	57
Not insane	2	1	3
Died	1	2	3

Died from exhaustion of chronic mania, 1; consumption, 1; pulmonary congestion, 1.

"Of those discharged recovered, during the year, four have had a recurrence of insanity, and been readmitted to the asylum. Two of them had been subject to a repetition of attacks for a number of years, and in three out of the four the recurrence was occasioned by a return to the same associations which had originally caused the disease."

The patients mentioned in the above paragraph were three men and one woman. The effect upon the foregoing statistics of the method of reporting these cases is, first, that the said cases are counted twice in the number of admissions; and, secondly, that four cases are reported as "discharged recovered," when the *persons* representing those cases are actually *at the Asylum and insane*. The subject of statistical registration and report was discussed, no less than fourteen years ago, at a meeting of the American Association of Medical Superintendents; and it was then found that there was a want of uniformity among the different members in the method of reporting such cases as were admitted more than once in the course of the same year. By some they were reported but *once*; by others, *as many times as they were admitted and discharged*. The discussion resulted in a general understanding among the members of the association, that, thenceforth, such cases should be reported but once.

We have known a case of periodical insanity, with perfectly lucid intervals, in which the person, during a period of perhaps fifteen years, was admitted into some asylum—not always the same—on an average of about five times, annually. Had this case always been reported as "discharged recovered," the statistics of insanity would have been swollen by about *sixty cures of one person*, although that person, even until the time of death, was *not cured* of the constantly recurring paroxysms of the disease.

"Three were discharged from the institution as *not insane*. One of these was a female of notoriously-loose habits, no more insane than others of that class; another, an intemperate man, whose passions became aroused under the stimulus of drink, a fit subject for an asylum for inebriates. The third was consigned to the institution upon false representations, and removed, after a few days' residence, by order of the probate judge who committed him."

Dr. Kendrick presents the following general outline of his method of medical treatment:—

"The fact that heroic treatment will not answer for the purely nervous disorder is no argument against the employment of all the resources of the *materia medica* in those cases in which an accurate diagnosis discovers complications with bodily disease. * * * In the various forms of insanity we use different classes of remedies; and among these we have special favourites. As an anodyne we use hyoscyamus largely, in the convenient form of fluid extract, either alone or in combination with the fluid extract of valerian. The latter has proved very serviceable in the milder forms of maniacal and melancholic excitement. The wine of opium is also a favourite narcotic with us. We have made brief trial of the cannabis indica (Herring's extract) in cases to which it seemed applicable, but as yet with indifferent success.

"Of tonics we have found the double salt of the citrate of iron and quinine to answer a better purpose, in a large number of cases during the past year, than the carbonate of iron in the preparation so commonly used.

"In melancholia, we have been highly pleased with the alterative effects of the chlorates, especially the chlorate of potassa, which, given with camphor

water, regularly and *persistently*, has admirably prepared the anæmic for the subsequent administration of iron.

"When there has been inactivity of the digestive functions the milder cathartics have been employed. The fluid extract 'rhei et sennæ,' made more active, if necessary, by the addition of a little fluid extract of jalap, has been found the most agreeable and efficient.

"We have found the regular use of tepid baths a valuable auxiliary in the treatment of melancholia; and the general warm bath, with cold water to the head, very effectual in promoting tranquillity and sleep, in cases of irregular circulation and paroxysmal excitement."

In regard to moral treatment, he says: "Discipline—and by this I mean such a system as leads to order and subordination through higher motives than the fear of chastisement—is a most essential element in the moral treatment of every class of the insane."

6. As has heretofore been mentioned, the buildings of the *Hamilton County (Ohio) Lunatic Asylum* are but poorly adapted to the purpose for which they are occupied, and hence inadequate to the prosecution of a system of treatment commensurate with the knowledge of the present day. At the time the report now before us was written, a new building, at Carthage, was in progress, with a prospect of completion in September or October, 1859.

	Men.	Women.	Total.
Patients in the asylum, June 6, 1858	112	118	230
Admitted in course of the year	105	59	164
Whole number	217	177	394
Discharged, including deaths	74	47	121
Remaining, June 5, 1859	143	130	273
Of those discharged, there were cured	43	19	62
Transferred to Southern Ohio Asylum	11	5	16
Died	5	12	17

Died of purpura, 1; marasmus, 2; epilepsy, 2; nephritis, 2; maniacal exhaustion, 3; chronic dysentery, 2; paralysis, 1; gastritis, 1; phthisis, 1; delirium tremens, 1; suicide, 1.

"The man who died of delirium tremens came into the house in a dying condition, and only lived six hours after admission. The patient who destroyed himself had been in the house but twenty-four hours, and we had no reason to believe, either from the conduct of the man or the statements of his friends, that there was in the case a tendency to suicide."

After an enumeration of the apparently deleterious influences to which his patients, through the defects of the asylum buildings, are exposed, Dr. Mount concludes: "That the freedom from sickness among these old, demented people, and the tenacity of life manifested by them, must be accounted for on the ground that the derangement of the mental faculties renders the system less vulnerable to the ordinary causes of disease and death." This conclusion is diametrically opposed to the opinion of most physicians who have had much experience in the treatment of the insane. The doctor, however, acknowledges that, "as heretofore, and as must always necessarily be the case under conditions similar to those affecting us, the diseases attacking our inmates, no matter what may have been their type primarily, speedily assume an asthenic character requiring the most prompt, efficient, and persistent stimulating plan of treatment to successfully combat them."

We hope for a better state of things in the new building at Carthage.

Of the 394 patients at the asylum in the course of the year 105 were Americans, and 289 foreigners. Of the Americans, only 45 were born in Ohio; the remaining 60 were from thirteen of the other States. The nativity of the foreigners was as follows: Ireland, 104; Prussia, 45; Bavaria, 28; Wurtemberg, 19; Hanover, 13; other German States, 51; England, 9; Scotland, 7; Switzerland, 5; France, 3; Italy, 2; Poland, 1; unknown, 2.

7. During the year, covered by the report under notice, the *Indiana Hospital for the Insane* was so crowded with patients that fifty-seven applicants for admission were rejected.

	Men.	Women.	Total.
Patients in the hospital, Oct. 31, 1858	141	136	277
Admitted in course of the year	98	105	203
Whole number	239	241	480
Discharged, including deaths	88	89	177
Remaining, Oct. 31, 1859	151	152	303
Of those discharged, there were cured	49	46	95
Died	5	7	12

Causes of death.—Maniacal exhaustion, 7; acute meningitis, phthisis pulmonalis, tabes mesenterica, gastro-enteritis, and fistula in ano, 1 each.

Patients admitted since Nov. 1, 1848	883	870	1753
Recovered	410	409	819
Died			128

The epileptic insane are not received at this hospital on account of their supposed incurability. They are, consequently, without any resource within the limits of the State for hospital treatment. As they are becoming numerous, Dr. Athon advises that some method be adopted by which they may find an asylum in a public institution. In view of these cases, and of the numerous applications which he is compelled, for want of room, to reject, he urges the enlargement of the buildings of the hospital under his care. P. E.

ART. XVI.—*The Life of John Collins Warren, M. D., compiled chiefly from his Autobiography and Journals.* By EDWARD WARREN, M. D. 2 vols. Boston: Ticknor & Fields, 1860. 8vo., pp. 420 and 382.

WHEN a man like Dr. Warren has passed away from the scenes of his labours and triumphs—one who has so worthily sustained the historic value of his patronymic, who has done so much to adorn, dignify, and advance our profession, it seems but fitting that we should take some notice, however brief, of his biography, even though we should thereby depart in some degree from the ordinary province of medical reviewers. He led a busy life, and one fruitful of much good; and there are some things in the history of his life that we desire to hold up to the admiration of all, but especially to press upon the younger members of our calling for their guidance; for it becomes young men not only to gather lessons of professional wisdom from those who have gone before them, but when they may, to receive admonition touching the conduct of life. It should not be forgotten that the success as professional men, depends in no trifling degree upon our conduct as citizens.

John Collins Warren, as the son of Dr. John Warren, inherited a position that many—nay, most young men, attain only as the result of arduous labour; but it must be remembered that he retained that position, and advanced beyond it only by virtue of labour as severe, and toil as constant as if he had inherited no fame. In the struggle for the rewards of our profession, he who starts without the prestige of a great name, has less to contend with than his envied competitor whose father was great before him. Whatever Dr. Warren gained of honour or emolument, was won, as it were, at the point of the bayonet, by the force of a strong will, and the steady operation of an unflinching resolution. Whether at home or abroad his days were rounded and filled by the same persistent will, making itself felt in each and every detail of life as much as in those grand occasions which so seldom come to any of us; and he was equal to the greater because he was not unfaithful to the less. He who would *achieve* must not be unmindful of the labour requisite to success. While one is content to think

himself a born genius he must be content also to be passed by in the race of life by many whom he may despise.

Dr. Warren was not, to our apprehension at least, a man of genius. What he was and what he achieved was not owing to possession of unusually great gifts of mental endowment. The secret of his power, and the cause of his success, is plainly taught on every page of the volumes before us; wherever we open them we are sure to meet the *man at work*; at home or abroad, as student or teacher, in private life or public, he was still and always the same industrious, persevering man. Now, in commending this trait of his character to others for their imitation, I do not mean to say, that with equal zeal and labour all can attain a similar position, for, other things being equal, the prizes in our profession are not so numerous as its members; but I do mean to say, that that is no reason for neglecting the course that has made not only Dr. Warren, but many others eminent and honoured.

But it was not our purpose to enter either upon a review of the work which heads this article, or to write a panegyric upon the character of Dr. Warren; we designed only to commend to all who may have leisure the perusal of these volumes, feeling sure that no one can resist the conviction that in his death our ranks have lost a great and good man, and one the force of whose influence was ever on the side of virtue and morality. His life was but another illustration added to the many preceding, of the benevolent and humane tendency of the medical profession. How many are there, when such a man is gathered to his fathers, who rise up and call him blessed.

H. A. C.

ART. XVII. — *Memoranda Medica, or Note Book of Medical Principles.*

Being a Concise Syllabus of Etiology, Semeiology, General Pathology, Nosology, and General Therapeutics; with a Glossary for the Use of Students.

By HENRY HARTSHORNE, A. M., M. D., Professor of Theory and Practice of Medicine in the Medical Department of Pennsylvania College, etc. etc., *Ὠφελειν ἢ, μὴ βλάπτειν*, "To heal, or, not to harm." 12mo. pp. 190: Philadelphia, 1860, J. B. Lippincott & Co.

THE idea of these "*memoranda medica*" is a very excellent one, and it has, most certainly, been carried out by the author with great ability. Although the work lays claim to no higher character than that of a simple outline of medical principles; holding, as it were, an intermediate rank between a mere skeleton synopsis or syllabus of a course of instructions on the institutes of medicine, and the more elaborate and finished text-book or manual; it is, nevertheless, one that cannot fail to be eminently useful to the student—almost indispensable, indeed, to all who would follow to the best advantage "the fundamental and introductory portion of the course of lectures delivered by the author in the department of the theory and practice of medicine"—while, at the same time, it will prove, we are convinced, by no means devoid of both interest and instruction to the practitioner likewise.

The volume presents, in language at once concise and perspicuous, a faithful outline exposition, admirably arranged, of the more important points relating to the subjects embraced within its scope. Not a mere complication of a series of aphoristic sentences from the writings of the leading authorities in general pathology and therapeutics, but laying fair claims throughout to originality, not merely in its conception and arrangement, but in much, also, that relates to both its style and matter. Many, indeed, of the general positions laid down in its pages are couched in language particularly happy, as well for its clearness and accuracy, as for its comprehensiveness.

We have examined few publications of an elementary and unpretending character, such as the *memoranda medica* of Dr. Hartshorne, with which we have found cause to be so well pleased—none where we have had as little occasion to dispute the accuracy of the views formally expressed or merely implied. Its

teachings bear throughout the strong impress of apparent truthfulness; they are in accordance, certainly, with the most reliable and extended series of experiments and observations. The aim of the author, it is apparent, has been faithfully to indicate, with clearness and conciseness, established facts, rather than to create for himself a doubtful character of originality by indulgence in bold and dazzling paradoxes.

He may be accused, perhaps, of indulging in the use of a somewhat novel terminology. It cannot be denied that the received medical nomenclature stands much in need of reform. This reform, however, we fear, will scarcely be effected by each new writer attempting to replace the medical terms in common use by others which he considers more appropriate or expressive. In this manner medical terminology becomes overloaded with a long array of synonyms, while it loses the important desideratum, uniformity.

So successfully, upon the whole, has Dr. Hartshorne fulfilled the task he had assigned himself, in the preparation of the volume before us, that we hope he may be induced, at an early period, to render his memoranda more complete, and, at the same time, enhance the usefulness of the series, by the fulfilment of his promise to add parallel memoranda upon the subjects of nosography, special therapeutics, and the practice of medicine.

D. F. C.

ART. XVIII.—*A Treatise on Medical Jurisprudence.* By FRANCIS WHARTON and MORETON STILLÉ, M. D. *The medical part revised and corrected with numerous additions.* By ALFRED STILLÉ, M. D. Second and revised edition. Philadelphia: Kay & Brother, 1860. 8vo. pp. 1031.

FIVE years ago we had the pleasure of inviting attention to the first edition of this excellent work; and we now have the satisfaction of announcing the fulfilment of expectations then expressed, in the appearance of a second edition which is likely to prove doubly welcome as a decided improvement on its predecessor. The comparatively early exhaustion of the first edition of so voluminous a work, speaks well for the appreciation of it by the members of the profession of law and medicine in this country; and would seem to indicate a growing interest among our American practitioners in the too long neglected subject of legal medicine.

It is hardly necessary to say that this professional estimate of the value of the work, as well as of the importance of its subject, is a right one; and that the call for a new issue of a standard and classical production on such a subject does quite as much honour to the good sense and intelligence of those who make the demand as it can to the merits of the book itself. Nor need we stop to dwell a second time upon the various excellencies and practical advantages of the treatise of Messrs. Wharton and Stillé, in some of which we think it superior to all of its competitors. These good qualities are so generally acknowledged that a brief account of the additions and other improvements will be sufficient to commend the volume once more to the particular notice of the professional reader.

The new preface informs us that "nearly three hundred pages have been added to the legal and psychological department. The chapters on insanity have been rearranged, expanded, and in some material points corrected, so as to bring them in harmony with the current decisions of the English and American courts. Several distinct topics have been introduced and examined at length; among which may be mentioned Survivorship; Medical Malpractice; the Legal Relations of Identity; the Presumptions to be drawn from Wounds and the Instrument of Death, and the Psychological Indications of Guilt. On the other hand, the chapters on circumstantial evidence have been condensed by abridging cases which in the first edition were new to the professional eye, but which have since become generally accessible." We are glad to note that among the cases which have thus been curtailed, the apocryphal one known as the Rue Morgue case, of the unfortunate Poe, has been entirely left out.

The editor of the medical portion, Dr. Alfred Stillé, has made no alteration in the general arrangement of this portion, but "has added to it about eighty pages of new matter, consisting of a chapter on the signs of death, besides many illustrative cases and recent methods of investigation." The preface further states, that the original text of Dr. Moreton Stillé "has been carefully revised, and pains everywhere taken to render the language clear and the statements accurate; in a word, to make this portion of the work as nearly as possible what its lamented author would have done, had he survived to perform a similar duty."

After a close examination of the many chapters and paragraphs, we can fully testify to the fidelity with which the revision of the text has been performed, and to the material benefit that has accrued to the matter of these chapters in the process to which they have been so thoroughly and ably subjected. The additional comments and explanations, new cases, and numerous references and citations, especially from French and German writers, add much to the value of the medical chapters; while they demonstrate the care and judgment as well as the great amount of labour which have been devoted to the work by the distinguished editor in his determination to do fitting honour to the memory of a gifted brother, by maintaining unimpaired the high character of his last and greatest scientific legacy. Those who are familiar with the old edition will be struck with many improvements of the new in minor matters of arrangement—in the headings—the analyses—the typographical details, as well as in the references, and cases, and new quotations throughout the volumes, which materially enhance the interest and usefulness of the whole treatise, and bring it up to the latest date of progress in medico-legal experience and practice. Author and editor have thus happily succeeded in adding largely to the attractiveness and authority of their volume, and in enriching its pages with an ample store of new and choice material, without any objectionable increase in actual bulk.

We might agreeably occupy many pages in pointing out, and quoting passages and especial cases which have interested us in renewing our acquaintance with this admirable work. It would be much better for our readers, however, to refer at once to the fountain-head of knowledge in this instance; and we advise them, without delay, to resort to the book in its present form, as to one in which they cannot fail to meet with a great deal of entertainment, along with a greater amount which is unusually well adapted for more important purposes. We sincerely doubt whether they could find a more accurate, clear, and competent counsellor and guide in regard to most of the various questions which are therein so elegantly and lucidly discussed.

E. H.

ART. XIX.—*On Hæmoptysis as a Symptom.* By JOHN WARE, M. D., Formerly Hersey Professor of the Theory and Practice of Medicine in Harvard University. From the Publications of the Massachusetts Medical Society. 8vo. p. 31. Boston, 1860.

THE history of hæmoptysis, and its value as a symptom are the objects aimed at by Dr. Ware, in the preparation of the present paper. The statistics and conclusions it embraces are based upon observations collected in three hundred and eighty-six cases that had fallen under the notice of the author since the year 1819. Care being taken to exclude all such cases in which small quantities of blood appear, mixed with the expectoration, in the course of ordinary confirmed phthisis. In the advanced stages of this disease, blood very generally appears sooner or later—at least a few times—either merely streaking or tinging the sputa, or else separately, from the amount of a few drops to one or two drachms. These discharges are obviously, as Dr. Ware remarks, of a very different nature from the hæmoptysis which occurs in the early stages, and no doubt proceed from the walls of tubercular cavities just as blood is often discharged from any other purulent cavity.

The cases, included in the present inquiry, are 1st. Those in which the hæmoptysis occurred as *the first indication of disease*, in persons who had before exhibited no evident or strongly marked pulmonary symptoms. 2. Those in which it occurred in *the forming stage of phthisis*, as indicated by cough, fever, and other alarming symptoms, without any evidence, however, of tubercular softening or purulent expectoration. 3. Those in which it occurred *at any subsequent period of phthisis* to such an extent as to demand particular attention from its quantity. 4. Those in which it occurred in *the latter stages of phthisis*, either as the immediate cause of death, or at least in such a manner as to constitute a prominent symptom. 5. Those in which it occurred *in connection with non-tubercular diseases*, as pneumonitis, bronchitis, cardiac disease, or asthma.

From a table showing the months in which, in 355 cases, the first attack of bleeding occurred, we find that in much the larger number of instances it happened in the colder months. Thus the greater number of cases—38 and 39—occurred in March and November, respectively; the smallest number, 18, in June. The seasons of the year in which the largest number of instances are recorded, may be denominated the transition seasons—spring and autumn—which gave, respectively, 101 and 102. Those in which the smallest number are recorded, were the equable seasons—winter and summer—which gave, respectively, 83 and 69.

From a table exhibiting the age and sex of 317 cases, we ascertain that previously to the age of twenty years, females are more liable to hæmoptysis than males—in the proportion of 130 to 13; that during the next ten years, the liability of the two sexes is nearly equal, 67 males to 72 females; during the ensuing decade the excess is on the part of the male, 49 to 28 females; during the next the numbers are again nearly equal, 20 to 28, while from the 50th to the 60th years, the preponderance is, once more, greatly on the side of the female, 13 to 5 males.

“Whether,” Dr. W. remarks, “this has any connection with a disturbance in the balance of the circulation at the two periods during which there is an excess in the number of females, connected with the establishment, and cessation of the function of the uterus, is an interesting question.”

The numbers of each of the sexes in the last table were 153 males and 164 females, but the sex without the age was noted in 65 other cases, 42 males and 23 females, making in the whole, 384; of which, therefore, 195 were males and 189 females. Taking, then, the whole of life, no very probable deduction can be made as to any greater tendency to hæmoptysis in one sex than the other. So far as the cases under consideration are to be relied on, it is about equal.

The following is a brief summary of the classification by Dr. W. of 329 cases recorded by him, in sufficient detail to afford materials for some deductions with regard to their character, progress, event, and treatment.

1st. Cases with symptoms and course of ordinary phthisis, in which hæmoptysis was the first, or at least a very early symptom, bloody sputa not being particularly liable to occur in the advanced periods of the disease, and never as a marked symptom. The amount of blood discharged in these cases was generally small in amount. Whole number of cases 91. Of these, 52 were observed throughout, in 39 the course was inferred, and, it is believed with great certainty, from the symptoms and physical signs of their early stages.

2. Cases in which the hæmoptysis was preceded, accompanied or followed by symptoms of pulmonary phthisis—the course of the latter being very protracted and fluctuating from better to worse, or from worse to better. The patients were generally older than those of the first class, and the quantity of blood discharged was usually larger than in ordinary phthisis, and sometimes very large. Whole number of cases, 86. Course ascertained, 43; inferred, 43.

3. Cases in which the hæmoptysis recurred after an interval—sometimes of many years—of apparent health, death taking place finally from phthisis or with tubercles in the lungs. Whole number of cases 6. Course ascertained, personally, in 3, and of the nature of the 3 others, there was satisfactory evidence.

4. Cases in which hæmoptysis is followed by an apparent recovery and without any recurrence of the bleeding subsequently; death taking place from diseases having no especial connection with the attack of hæmoptysis or with the

existence of tubercles. Whole number of cases 114. Ascertained, 62; probable, 52.

5. Cases in which hæmoptysis is the predominant symptom at the close, whether it had occurred or not at any previous stage. Whole number of cases 14. In 4 death was the immediate consequence of the hemorrhage; in 7 from pulmonary congestion; in 3 death took place not as an immediate consequence of the hemorrhage, and without any tuberculization of the lungs.

6. Cases in which death took place a short time subsequent to an attack of hæmoptysis from some other disease, most generally disease of the heart, and without suspicion of any tubercular affection. Number of cases, 15.

7. Cases in which hæmoptysis occurred in patients labouring under confirmed asthma, without any indication of the existence of tubercles. Number of cases, 3.

The entire paper of Dr. Ware is replete with facts and deductions of the deepest interest. We should be pleased to present a more extended analysis of it; more especially of the very judicious remarks of the author on the management of hæmoptysis and the cases in which it is liable to occur. We must content ourselves, however, with simply giving to our readers the following general recapitulation of the more important points connected with the prognosis in those cases of which hæmoptysis constitutes a prominent or occasional symptom.

"1. That there is rarely any immediate danger from an attack of hæmoptysis, especially the first, except when there is some general affection of the lungs of a congestive or inflammatory character, attended by much embarrassment of breathing and other grave symptoms, and that the danger in such cases does not depend upon the amount of hemorrhage.

"2. That of all cases of hæmoptysis, so far as we draw our judgment from those which have been now examined, less than two-thirds end in phthisis; consequently that in all those unaccompanied by distinct indications of the presence of tubercles, there may be held out to the patient a reasonable prospect of recovery, or, at least, of a disease that will be gradual in its progress.

"3. That the most favourable cases, as to complete recovery and future immunity, are those preceded or accompanied by catarrhal symptoms.

"4. That no patient who has once had hæmoptysis, however slight, can ever afterward be regarded as entirely secure from the development of tubercular disease.

"5. That those cases of phthisis which are ushered in or accompanied by hemorrhage, especially if it be copious, are more likely than others to be protracted, and more capable of being prolonged and rendered comfortable by suitable treatment.

"6. That hemorrhages occurring in the latter stages of phthisis in large quantity and of continued duration, are liable to be either speedily fatal from their quantity, or to prove so after a short period from the embarrassment to the function of the lungs which accompanies them, but that such cases are of rare occurrence."

D. F. C.

ART. XX.—*A Practical Treatise on the Etiology, Pathology, and Treatment of the Congenital Malformation of the Rectum and Anus.* By WILLIAM BODENHAMER, M. D. Illustrated by 16 Plates, and exemplified by 287 Cases. New York: Samuel S. & William Wood, 389 Broadway, 1859. 8vo. pp. 358.

THIS is an opportune and valuable addition to the means of acquiring a knowledge of the diseases of the rectum, which the excellent works of Ashton, Quain, Syme, Bushe, and Copeland have so clearly and fully furnished to English and American readers. As a practical monograph it is not inferior to either of these, so far as relates to its particular department of the subject; while it surpasses them all in completeness and extent of illustration, and in the facilities afforded to the student for the purposes of further investigation.

"The investigation of these abnormal conditions of the rectum and anus is invested with a deep interest," says our author, "not only as an important patho-

logical inquiry, but, above all, as conducive to the adoption of measures calculated to be highly beneficial to a class of little sufferers, the most unfortunate and deplorable." And yet no general treatise on the subject has hitherto been published. Its literature is certainly rich in records and discussions, but too much beyond the reach of the great majority of practitioners to be of general service. Dr. Bodenhamer has endeavoured to collect the scattered materials from the various scientific channels "into one continuous whole, adding to them his own reflections and experience on the subject; and thus presenting, in a systematic and connected form, a complete and accurate exposition of the congenital malformations of the rectum and anus, their etiology, pathology, classification, and treatment."

He does not pretend to an extensive practical and personal knowledge of his subject, as very few writers can, but claims entire originality of general design and mode of treating it in the elucidation which he has prepared; and he offers his work as the result of much reflection and long labour, of which the careful study and more or less full citation of some three hundred different authorities and cases formed a very material part.

The methodical arrangement of his volume is a meritorious feature which will be recognized by the most cursory reader, and which must greatly increase its value to the actual practitioner. We find, in the first place, a table of contents which affords an interesting and certainly convenient synopsis of each section and chapter, and is in itself a very useful summary of the facts and opinions of the author, and his authorities, in regard to all the important questions mooted in his volume. Next there is a series of lithographic plates, sixteen in number, in which numerous cases, instruments, and operations are well illustrated by representations copied from the publications of Von Ammon, Cruveilhier, Bourguery, and Bernard & Huette. These illustrations add much to the value of the work for all purposes; and the only regret we have to express in regard to them is, that one of them should have been perverted from its proper use and position into a frontispiece which, in more than one sense, may prove more of a scarecrow than a bait; at all events, it is neither useful nor ornamental as a figure-head to the publication.

Next to the plates comes the introductory chapter (I.), beginning with a copious bibliography which occupies nineteen pages and refers to three hundred and seven different articles in ancient and modern writings. This index, as the writer justly remarks, proves the unequalled extent of his researches, and affords a correspondingly complete history of the progress of experience, as to congenital vices of rectal and anal conformation, which must vastly facilitate the study of them for every purpose.

The succeeding portion of this chapter discusses, in seven different sections, some general considerations, the etiology, anatomical and pathological characters, classification, general symptoms, and prognosis. Then follow nine chapters, which are devoted, in three sections each, under the respective heads of description, treatment, and cases and remarks, to the successive consideration of the nine different species of malformation into which he has arranged his classification of the various forms observed. The last of these chapters (X.) terminates with a recapitulatory table exhibiting "at one view the whole number of cases of congenital malformation of the rectum and anus, collected from various sources, which are reported in this work." The concluding chapter (XI.) is occupied, in about a hundred and thirty-four pages, with an elaborate and interesting discussion of the operation for the production of abdominal artificial anus. The volume is then appropriately and very acceptably completed with an alphabetical index to the illustrative cases and a general index of topics.

In regard to the numerous cases collected by the author, he tells us in his preface, what is evident enough in the body of his work, that the record "will show their singular variety; they have all been carefully classed according to their species, and most all of them have been reported in full, and, as near as could be, in the precise language of their authors." Many of them have been translated from the French, German, and Latin, especially for the work, and generally from the original sources, being accompanied by but few remarks of the author, he "preferring to present in full the instances themselves, as facts, from which

each practitioner might form his own opinion and draw his own conclusions." In doing this, he has succeeded in the paramount object of comparing the hitherto uncollected experience of different nations, having "brought together in one harmonious body, for the mutual benefit of the whole, authorities both English, French, German, and American." The work is not an empty compilation, however, since the author's views are clearly and precisely given upon all practical points, and many useful practical details are pointed out in a manner which shows them to be the product of much especial thought and observation, as well as practical skill and intelligence. E. H.

ART. XXI.—*Annual Address delivered before the Philadelphia Medical Society, at its Meeting held on the 26th of March, 1860.* By R. LA ROCHE, M. D. Published by order of the Society. 8vo. pp. 61.

TURNING away from the beaten path pursued in most of the annual addresses delivered before our medical societies, and rejecting the hackneyed topics discussed in them, Dr. La Roche, in the address before us, has struck out for himself a road and subject as novel as they are, confessedly, interesting and instructive. His theme is the fifth of the first book of the Satires of Horace, in which the distinguished Roman poet gives an account of his journey from Rome to Brundisium—the Brindisi of the present day—an important city of Calabria, on the Adriatic coast, and the then capital of the Salentines. This journey was performed by him in company with three of his devoted friends and patrons. Mæcenas, Coccius, and Fonteius Capito, commissioners sent to adjust the difficulties which had arisen between Octavius and Mark Antony, and by whom was negotiated the well-known treaty of Brundisium; a principal result of which, besides averting from Italy the horrors of an impending civil war, was the reconciliation of Octavia, the sister of Octavius, with her unfaithful husband, Antony.

While Dr. La Roche, for the sake of perspicuity, sketches briefly the leading details and incidents of the journey, the main object of his examination of the Satire referred to is to indicate its application to questions of a purely professional character. The prominent subject commented upon is the contrast which there exists between the condition of the Campagna di Roma, over a considerable portion of which Horace travelled, as it is represented in the Satire referred to, and its condition at the present time, with the object of determining, if possible, the causes by which a portion of country that in ancient times was amply peopled and thickly spotted with small cities, to a certain extent independent of each other, the inhabitants of which fed their flocks or cultivated the land in the vicinity of their residences, and where at a later date were located the villas of the patricians, with their celebrated parks and gardens, has become reduced to its present desolate and pestiferous condition. A tract of country, over which Horace and his companions travelled with perfect safety, and apparently without the slightest suspicion that any danger was incurred as they slowly traversed it, by day and by night, sleeping several nights in villages and other stopping-places in the very heart of paludal districts, which at the present day cannot be visited, at the same season and under the same circumstances, and remained in for the same length of time, without the utmost risk to health and life.

The address is one of uncommon ability. It bears the impress alike of the classical attainments, the scientific acumen, and the professional zeal of its distinguished author. It is impossible to read it attentively without the most intense interest being excited in the theme discussed, and the manner in which it is treated—without the acquisition of an amount of knowledge which, if not directly applicable to the purposes of medicine as an art, is of a character that no true member of a learned profession such as ours should be without.

D. F. C.

ART. XXII.—*Bed Case: its History and Treatment.* By WALTER CHANNING, M. D., Honorary Fellow of the Obstetrical Society of London. "Impotent and bed-ridden."—*Hamlet*. Boston: Ticknor & Fields, 1860. 8vo. pp. 52.

THE malady which is most graphically described by Dr. Channing in the pages before us is neither a new nor an unfrequent one. Although heretofore it has received no distinct consideration—there being no treatise, so far as we are aware, devoted to its especial consideration—yet every physician must have met with at least one "bed case," if peradventure he has not been himself the unintentional instrument in producing one.

It is by no means a rare occurrence for an individual, more especially one of an eminently nervous temperament—a young, delicate, tenderly reared female, for instance, surrounded by all the comforts and luxuries of life—to take to her couch, by the direction perhaps of her physician, in consequence of her supposed affliction with some affection of womb, or spine, or heart, or in the regular course of treatment for fracture, sprain, articular inflammation, or other disease in which rest is essential to the cure; or perchance of her own free will, because of a feeling of languor, fatigue, malaise, or actual suffering when in motion or in the upright and sitting postures; and in either of these cases to become actually bed-ridden—at first from an indisposition to rise and move about, but subsequently from an inability to do so without inconvenience or actual suffering, more or less decided in different cases.

The poor bedridden patient never fails to become the centre of sympathy, of kindness, and of the most assiduous attentions on the part of friends and acquaintances. All exhibit the utmost anxiety to prevent her life (in the great majority of instances it will be found that the patient is a female) from becoming intolerable, and to render it, if possible, even more agreeable when chained to her couch than it was when she was free to move about and act as she listed.

"The complexion," as Dr. C. remarks, "may become fairer, more delicate, by an always in-door life, and the weight may be increased by varieties of nourishing food, and by the slight waste which entire rest and the absence of grave care often involve. Intellectual habits will secure varied and grateful occupation. There is talking, and reading, and writing, it may be. The life may be passed much in the past; but when its pleasant is at our call, the present will be agreeable enough. The religious sentiment may be entire; and this, even in its occasional exaggerations, as with all other circumstances of the Bed Case, it is likely to be, brings with it enough of resignation to make the patient comfortable, and a willing waiter upon the future. The temper is preserved in the present class; fretfulness, dissatisfaction, impatience, are rarely present, or, should either or all of them occur, there is a wide garment of charity for such infirmities, and they are soon covered up and forgotten.

"I have spoken now of an extremely interesting class of patients. They do not constitute the whole or the greater number. Still, in their various modifications, these elements are frequently met with. From what is here affirmed may be gathered the circumstances or symptoms of other cases. In these, from their length, from their hopelessness, effort for cure is gradually given up, and the bed is the home of the sick. In these we have sometimes great emaciation; some symptom—it may be cough—is gradually established, and some of the imitative signs of phthisis may be present. In these the mind may suffer with the body; weakness extends to this, and moral manifestations are made, and grow habitual, which are anything else but agreeable. Often, in these, dissatisfaction and discontent declare themselves very strongly; and, as is common in some other forms of mental weakness, the friends of the patients—those who, by night and by day, do most to make their sufferings less—are the patient or impatient recipients of most of the current complaints.

"One fact in this history deserves notice, as it has some bearing on diagnosis, prognosis, or treatment. It is the force, the intensity—let us use the best word, the exaggeration—which characterizes the expression of the whole views of the patient. No matter what may be the subject of conversation, this characteristic

of it is very generally present. It doubtless has effects beyond itself. It may be upon the patient or on friends. The former comes to estimate the severity of her case by her habitual descriptions of it; and as these grow in strength, her consciousness of the hopelessness of her condition is increased. The physician and friends are in great danger of a like influence, until at length the case is abandoned, or, what most frequently happens, it passes into the current quackery, and drifts into whatever direction such an agency, or mere accident, may impel it."

There may be discomfort, depression, even pain, attendant upon the bed case. Motion is so intolerable often that it is rarely attempted, except under the strongest motives from the physician and friends. Continued rest diminishes the power of the muscular system—disturbs the relations between voluntary muscles and the will—so that in time there is not only pain on attempting motion, but absolute want of ability to move. Nay, constant lying, certain positions, and accidental but unnatural action of muscles, may, and often do, produce various deformities—the very distortions of spine and limbs to prevent or relieve which rest in the recumbent position was enforced. The nervous system of organic life, and that under the dominion of the will, fall into the condition of their organs, and from disuse become disturbed and enfeebled, and their operations perverted.

The "bed case" may be confounded, by the careless observer, with hysteria, neuralgia, consumption, irritable uterus, disease of the heart or of the spine, palsy, etc. From all of these, however, it may be distinguished by a careful analysis of the morbid phenomena present, in order that we may determine their true import. One striking feature of the bed case, by which alone it may in general be distinguished, is the complete change which sometimes the character of the symptoms and their location suddenly undergo, while in other cases there will be a continuance, a persistence, a neither getting better nor worse, in the more prominent of the simulative phenomena. They will continue the same in character, location, and intensity from day to day, for weeks, and sometimes months and years.

To describe the pathology and lay down the treatment of the disease alluded to—which had no name until Dr. C. gave to it that of bed case—is the object of the essay before us, an essay we would recommend to the earnest attention of every practitioner, old as well as young. It is replete with important truths, a general recognition of which would, we are persuaded, be the means of causing many a patient long bedridden to rise upon her feet, prepared and able to fulfil properly her allotted part in the task of life.

D. F. C.

ART. XXIII.—*An Essay on the Treatment of Phthisis by the Chlorate of Potash, with observations on Oxygen as a Therapeutic Agent, and the Exposition of a New Theory of the Formation of Ozone.* Read before the American Medical Association, at its Thirteenth Annual Session, held at New Haven, June, 1860. By E. J. FOUNTAIN, A. M., M. D., of Davenport, Iowa. 8vo. pp. 24. New York, 1860.

THE above essay was presented by its author as a voluntary contribution to the Section on Practical Medicine and Obstetrics of the American Medical Association, at its session of the present year. By a vote of the Section it was referred back to Dr. F., "with the request that he pursue his investigations and report at the next session of the Association."

By the publication of the essay its author believes that he shall most effectually carry out the wishes of the Association in reference to it. He hopes in this manner to "attract the attention of the profession to the investigation of the value of oxygen in its diversified application as a remedial agent." The essay Dr. F. submits, "not as a conclusive demonstration of any theory or practice, but as containing suggestions which may lead others to test their value,

and thus to a prompt investigation in a new direction." He solicits contributions from his professional brethren on the subjects of which the essay treats, in order that sufficient materials may be obtained for a satisfactory report to the next session of the American Medical Association.

In a paper published in the *New York Journal of Medicine*, for July, 1859, Dr. F. called the attention of physicians to the importance of meeting a frequently occurring indication arising in the progress of many diseases from *imperfect aëration of the blood*, and detailed some cases in illustration of the fact that in the *chlorate of potash* we have an agent fully capable of fulfilling such indication. He, at the same time, endeavoured "to establish the theory, that in many instances where the blood is not deficient in its usual supply of oxygen absorption of many organic products may be promoted by rendering the blood more highly arterialized through the agency of the chlorate of potash. Among these were included tubercular deposits, as these were believed to be the result of an imperfect elimination from the system of the products of organic decay of the tissues of the body, and are composed principally of protein compounds, which are rendered soluble by the addition of one or two equivalents of oxygen, converting them into the deutoxide or tritoxide, and permits their absorption by endosmosis into the adjoining vessels; it was suggested that the chlorate of potash might be found a valuable remedy by supplying the blood with an excess of oxygen sufficient for this purpose."

This latter suggestion is further enforced in the essay before us. In illustration of the views advanced the history of three cases are given; from these cases, and other observations elsewhere recorded, Dr. F. deduces the following conclusions:—

"1. The chlorate of potash can be given in large doses every day, for a long period, without injury.

"2. It aids the function of respiration by supplying the blood with oxygen.

"3. It operates as a natural *tonic*, *alterative*, and *blood depurant*, by increasing the supply of that element which is the most active agent of nature in the chemical changes which take place in the laboratory of the human system.

"I will now add, that in the practical application of these principles, it is my belief, which may or may not be confirmed by the experience of others, that it is a peculiarly appropriate remedy for the *early stages of phthisis*, by which the resolution and absorption of incipient tubercles may be effected, and their further deposit arrested. Even where the disease has progressed to the second stage I have found patients derive great benefit from the use of this remedy; more, indeed, than from all others, single or combined. I cannot, however, expect it to be of permanent benefit, only in the early stages of the disease, before there is any disorganization of the lungs or suppuration of the tubercles. To all who are labouring under the symptoms of this disease, before it has progressed to such an extent, I would recommend the liberal and constant use of the chlorate of potash, aided by appropriate hygienic treatment, in which exercise and pure air hold the rank of first importance. Half an ounce daily is the quantity I usually give when I wish to produce a decided effect for any purpose, and this can be taken certainly for many weeks with impunity; but, when I have occasion to give it for a long time, I find three drachms a day can be depended upon as sufficient in most cases. After all symptoms of phthisis have been arrested by this treatment, I would recommend as a daily beverage a trial of liquids artificially charged with oxygen gas."

We shall be gratified if the anticipations of Dr. F., in respect to the remedial powers of the chlorate of potash, when resorted to in the earlier stages of pulmonary tuberculosis, shall be realized by the result of further experience. As to his views in regard to its mode of action in the dispersion of existing tubercular deposits, and the prevention of any deposit subsequently, we have nothing to say at present. It is scarcely worth our while to enter upon an examination of these views until it shall be ascertained whether any confidence is to be placed in the curative powers of the chlorate of potash in phthisis. "An opinion of a remedy," remarks Sprengell, in his *Commentary on Celsus*, edition of 1708, "without that knowledge of its powers which cometh from experience, is devoid

of any value; for it is only by observing how it acteth when used against a disease that we can come to know surely whether it hath any curative power."

For an exposition of Dr. Fountain's theory of the formation of ozone we must refer our readers to the work itself. He confidently affirms that "ozone is simply oxygen in a nascent condition, produced naturally by electrolytic decomposition of the vapour of water in the atmosphere."

D. F. C.

ART. XXIV.—*On the Reparative Process in Human Tendons after Subcutaneous Division for the Cure of Deformities; with an account of the appearances presented in fifteen post-mortem examinations in the human subject; also a series of experiments on rabbits, and a resumé of the English and foreign literature of the subject.* Illustrated by seven lithograph plates and a series of woodcuts. By WILLIAM ADAMS, F. R. C. S., Surgeon to the Royal Orthopædic Hospital, etc. etc. London: John Churchill, 1860. 8vo. p. 175.

As may be perceived from its title, this monograph is based upon a somewhat extensive range of practical observations. Its subject is of interest, not only as presenting the simplest conditions of the process of healing in the living tissues, but also as bearing upon the surgical treatment of certain deformities.

After detailing the appearances observed in thirteen cases of tenotomy, in most of which several tendons had been divided, in some of which the same tendon had been divided more than once at different points, and in which the ages of the patients as well as the periods which had elapsed between the operations and the dissections varied greatly, Mr. Adams gives a general summary of the views to which his researches have led him.

Immediately after section of the tendo-Achillis is performed, the divided ends are separated, the extent of this separation depending upon the amount of contraction of the muscular belly, and upon the degree to which the previous malposition is corrected by the surgeon. The sheath of the tendon is not severed in cases where the operation is subcutaneous, but yields before the edge of the knife; and even where an open wound is made, it does not retract with the tendon, being prevented from doing so by its relations to the surrounding areolar tissue. Mr. Adams places a high estimate upon the importance of the sheath in the formation of the connecting bond of new material; differing in this respect from Mr. Paget, who ignores it altogether. He, however, agrees with Mr. Paget, in opposition to the French and German authorities, in considering the occurrence of inflammation or of effusion of blood as a hindrance, instead of an aid to the reparative process.

This process is begun by an increase in the vascularity of the sheath, the surrounding areolar tissue and fat becoming also injected, but only incidentally. Along with this condition there occurs an effusion of nucleated blastema into the meshes of the sheath, as described by Mr. Paget. Subsequently, capillary vessels are formed in this new material, and the nuclei assume an elongated shape; the author is inclined to believe in the development of fibres from the latter elements. The new tendon never acquires the shining opacity of the old, nor do its fibres ever become readily separable from one another; its deep surface is more or less adherent to the subjacent tissues, and remains so persistently.

No part is taken in this process by the cut ends of the old tendon. Preparatory to their joining with the new material, they become rounded, a little enlarged and softened, and tend to split; a very fine dovetailing of the new and old tissue takes place, and ultimately the enlargement at the point of juncture disappears. In the human subject, the re-formation of a separable sheath is less plainly demonstrable than in the rabbit.

Mr. Adams strongly opposes the *linear cicatrix* theory, advocated by Tampin, Brodhurst, Coote, and others. According to these gentlemen, the efficacy of the orthopædic section of tendons is altogether due to an alteration in the sphere of muscular contraction; the new material interposed between the cut

ends becoming gradually diminished in length, until it constitutes little more than a transverse line, and the muscular belly being proportionally drawn out. Perhaps, as in the case of other cicatrices, a marked degree of shortening would occur if the newly formed tendon were not kept on the stretch. But in the analogous case of ligamentous union after fracture of the patella, the new tissue, as every one knows, is only too apt to lengthen out; and it is not impossible, by judicious management, to obviate almost entirely the tendency to contraction in the scars formed in the healing of burns. We are, therefore, inclined to think that the new material thrown out after tenotomy follows the general law of cicatrices to some extent, and that in so doing it draws out the muscular bellies concerned, so as to extend their sphere of action, or in other words, that the fleshy and tendinous parts of each muscle act and react upon one another, the contracting cicatrix of the tendon pulling upon the fleshy belly, and the latter again by its tonicity opposing the shortening of the former.

Complete non-union of the divided ends Mr. Adams thinks to be of rare occurrence, except in tendons which have no distinct cellular sheath; he has never met with it in the tendo-Achillis. It usually depends either on some defect of constitution in the patient, or upon injudicious treatment. The extension should always be regulated by the degree of activity of the reparative process, especially in non-congenital and paralytic cases.

The greatest length of new tendon which Mr. Adams has ever seen, was two inches and a quarter. He thinks that the required length in any case is not to be procured by a process of stretching, but that it may be regulated by the position given to the parts. The degree to which the deformity is remedied is, therefore, determined at the end of the time (from two to six or eight weeks), required for the construction of the new material between the divided ends. Relapses are due, not to any absorption of the newly formed tendon, but to changes in the muscular tissue.

In Part II. of this essay, Mr. Adams republishes in full some experiments on rabbits, an abstract of which may be found in the *Transactions of the London Pathological Society* for 1855. They seem to bear out the views already mentioned as defended by him with regard to the healing of human tendons after divisions. Not the least valuable part of this volume is contained in the appendix; which embraces a summary of the observations recorded by Hunter, Delpech, Mayo, Stromeyer, Bouvier, Von Ammon, Duval, Little, Guérin, Velpeau, Pirogoff, Dieffenbach, Phillips, Koerner, Tamplin, Paget, Gerstaecker, Thierfelder, Boner, Brodhurst, and Coote, upon the tendons of the human subject as well as of the lower animals. Independently of the practical character of Mr. Adams' work, this portion of it would render it important to those interested in the subject of tenotomy.

A series of five very admirably executed lithographs, representing the appearances observed in the dissections described in the text, and two others showing the microscopic characters of the new material, add greatly to the value of this essay.

J. H. P.

ART. XXV.—*Journal de la Physiologie de l'Homme et des Animaux*. Publiée sous la direction du Docteur E. BROWN-SÉQUARD. Numbers vi. vii. viii. ix. x. xi., 1859—1860.

WE have placed at the head of this notice all those numbers of M. Brown Séquard's journal which have been published since we last called attention to his valuable periodical. We have done so, not for the purpose of citing in succession all the memoirs therein contained—for the mere mention of them would occupy several pages—but in order that we might subject to analysis such papers in all of them as, from their scientific importance or practical value, were likely to prove especially interesting to our readers.

In the first of the above named numbers there is a memoir, by M. CLAUDE BERNARD, on "*Glycogene in its Relation to Tissue-Development in the Fetus before*

the Appearance of the Glycogenic Function of the Liver," which contains the results of additional observations on this subject with which M. Bernard's name is so inseparably connected. It will be recollected that in a former memoir, M. Bernard pointed out that before the development of the liver, the sugar-forming function is performed by the placenta and its annexes. It was also stated that the glycogenic matter is found in other organs and parts of the fœtus. The object of the present communication—which was originally made to the Academy of Sciences—is to indicate definitely these organs.

M. Bernard divides them into two classes: 1st. The exterior or limiting organs, as the skin and mucous membranes. 2d. The interior or continued organs, as the bones, muscles, nerves, &c. In regard to the first class, he finds that all the epithelial membranes of the skin and mucous surfaces contain glycogenic matter at a certain period of fœtal life. On the cutaneous surface this substance is found infiltrated into the tissue of the skin itself, and also into the epithelial cells which cover it. In the hog this disposition is well marked; but in the rabbit, the cat, and the calf, it is perceived with some difficulty. In order to establish the existence of glycogenic matter in this tissue, it is only necessary to scrape the surface of the skin of a young fœtus, and to place the detached particles in the field of the microscope. Cells and histological products of various forms are met with, and also granules which, by the action of acidulated tincture of iodine, are coloured red. These granules are the glycogenic matter. On the mucous surfaces of the intestines, the respiratory passages, and the genito-urinary channels, the same substance is to be found.

Among the tissues of the second class, the glycogenic matter is met with in the muscles alone. In the bones, the nerves, and the glandular organs, it is not found. An exception exists among these latter—the liver. In the early stage of fœtal existence it follows the same law as the other glands, but with the progressive development of the fœtus it acquires the faculty of forming both bile and glycogenic matter.

M. Bernard is of opinion that this glycogenic matter plays an important part in the organic development of the fœtus, but he thinks additional experiments are necessary before we can arrive at any exact conclusions relative to the subject.

In the seventh number, M. BROWN-SÉQUARD calls attention to a memoir published by him, ten years since, on the *Influence of an Elevated Temperature as a Therapeutic Means in certain Cases of Poisoning, &c.*, which we think worthy of consideration. We have several times had occasion to notice the fact which he mentions, that a smaller dose of poison will kill an animal when the temperature is low, than can cause this result when the heat is kept above the standard by artificial means. Some experiments of Dr. S. Weir Mitchell, which are not yet published (and to which, therefore, we do not feel at liberty to refer at length), sufficiently show the therapeutic influence of a high temperature to be very well marked.

In the ninth number, M. OLLIER continues his interesting researches relative to the growth of bone. The present memoir considers the question under three heads: 1st. The transplantation of bone taken from a living animal and placed among the tissues of an animal of the same species. 2d. Transplantation of bone taken from a dead animal, after a certain lapse of time, and placed among the tissues of an animal of the same species. 3d. Transplantation of bone taken from a living animal and placed among the tissues of animals of different species.

Many experiments are adduced under each of these heads. We cite the following, which relates to the first division of the subject:—

"*Exp. 1. Transplantation of the radius of a rabbit, four or five weeks old, under the skin of the groin of a rabbit, five or six months old.* The operation was performed December 27th, 1858. The periosteum was preserved throughout the whole extent of the radius. Immediate reunion of the wound was obtained, and the animal, under the skin of which the radius was placed, did not appear to feel the operation. There was a little swelling during the first few days after the operation. The animal was killed on the 1st of March, 1859, and the following facts were observed:—

"The graft had succeeded perfectly. The bone was united to the skin by a

cellular fascia, which was blended more or less with its periosteum, and which kept it in place. It had increased in size both in its vertical and transverse diameters since it had been transplanted. The precaution which had been taken to preserve the radius of the opposite side, permitted the exact augmentation to be ascertained. The rabbit, from which the radius was taken, was killed the same evening of the operation. When the two bones were placed side by side, it was immediately seen that the transplanted bone had sensibly increased in size. When the periosteum and cellular tissue which surrounded it were removed, it was immediately seen that the increase was due to the formation of extensive layers through the action of the periosteum. These layers—or rather this layer, for the naked eye could not separate them—were deficient in certain places corresponding exactly to those where the periosteum presented solutions of continuity. It was whiter than the old bone, and seemed constituted of a more compact substance.

“The old part of the transplanted bone had almost preserved its natural colour. It was sensibly vascular, and moreover was evidently continuous with the layer of new formation which surrounded it. The medullary canal had not undergone any notable alteration. The marrow was not so red as in the normal state, but it should be remembered that it was examined after the piece had been some hours in water.

“The increase in length was a little less than a millimetre. It appeared at first to be much more considerable, but after having stripped off all the cellular tissue which surrounded it, it was perceived that the real was not the same as the apparent increase. This increase appeared to belong both to the diaphysis and epiphysis. These portions of the bone were not united together—they are always separated by an intermediate plate of cartilage. The extremities were hard and of osseous consistence. The articular cartilages were deprived of their polish.”

The different phenomena observed are next discussed, and then the other divisions of the subject are proceeded with.

As the main result of the transplantation of bones taken from dead animals, it was found that the vitality of the periosteal and osseous tissues did not disappear with the life of the animal, and that the graft was accordingly successful, if not too long delayed. After the cessation of respiration and of circulation, after the interruption of nervous influence, the bone still preserves for a certain time a latent life which becomes sensible if it is placed in favourable relations. Transplantation under the skin of another animal furnishes the means by which it can continue to live. Its resistance to absorption, its growth, and its vascularity, proven by injections, are sufficient proofs of its vitality. The principal power of re-vivification is possessed by the periosteum. If this be removed, the bone dies.

The experiments of the third class were not so successful as those above referred to, and M. Olliver is of opinion that further investigations are necessary.

The eleventh number contains a paper, in English, by Dr. B. W. Richardson, on the “*Synthesis of Cataract*”—a continuation of the experiments originated by Dr. S. W. Mitchell. This paper has already been noticed in our journal.

As we remarked at the commencement of this notice, it would be impossible for us to call attention to all the admirable memoirs contained in the numbers before us. To do so would require more space than we have at our command. In addition to the original papers, there are a number of valuable translations and abstracts which add materially to the value of the periodical.

In conclusion, we can only say, what we have already said several times before, that the *Journal de Physiologie* is of great service to science, and that it is fairly entitled to occupy the prominent position it does in the medical literature of Europe.

W. A. H.

ART. XXVI.—*Advice to a Mother on the Management of her Offspring.* By PYE HENRY CHAVASE, Fellow of the Royal College of Surgeons of England; formerly President of Queen's College Medico-Chirurgical Society, Birmingham; Author of "*Advice to a Wife on the Management of her own Health.*" Fifth edition. 12mo. pp. 255. London, 1860.

WORKS of the character of the present, could they be made to reach and command the interest of those for whose instruction they are intended, are unquestionably adapted to accomplish a large amount of good. It cannot be denied that, were parents generally, but especially mothers, to be well informed in respect to the proper management of children, they would save themselves much anxiety and their offspring no little discomfort and actual suffering, by guarding them from the influence of the more important of the causes by which their health is destroyed, the vigour of their constitutions undermined, and the lives of so many of them annually destroyed, while upon the survivors there is entailed a strong predisposition throughout life to the attack of disease from slight causes.

We hold it to be a duty incumbent upon all mothers to inform themselves as to the proper hygienic management of their children, so that they may be enabled to preserve the health of the healthful, and to improve the health of the feeble. To be familiar with the means by which disease may be prevented; to be able to recognize and appreciate the signs of any approaching illness, that they may promptly apply for proper medical assistance before disease has gained too firm a footing to be combatted with success; to know how to nurse a sick child, and properly to manage the nursery whether its inmates be well or ill, and to be familiar with the measures that should be promptly taken in the case of accidents, where even the slightest delay may even prove fatal.

Were every mother to be thus instructed, the errors and prejudices that are still entertained to a very great extent in relation to the hygienic management of children, would be dispelled, and the injurious practices thence resulting entirely obviated, rendering in consequence, the task of the physician when called upon to treat the diseases of early life, more easy and agreeable, and his success in their removal more certain.

To impart the important instruction above referred to is the intention of the volume before us, an intention in the carrying out of which the author has been, we think, eminently successful. The precepts and admonitions embraced in his "advice to mothers" are, unquestionably, upon the whole, sound and pertinent. After a careful examination of the volume we find, in truth, but little to object to, and that little having reference to questions of only minor importance.

D. F. C.

ART. XXVII.—*A Manual of Anæsthetics: Theoretical and Practical.* By CHARLES KIDD, M. D. New edition, Lond. 1859. 18mo. pp. 249.

THIS, which the author suggests may, perhaps, be recognized "as the metamorphosed chrysalis of a former edition of a smaller work of the same kind," is one of the most entertaining contributions to the literature of anæsthetics. It presents much information, not easily found elsewhere in regard to the means of relieving pain during surgical operations which have been employed in different ages of the world, and not a few illustrations taken from the poets of the mental condition which anæsthetic agents are capable of producing. Indeed, the author's style, which is always lively and agreeable, has, perhaps, somewhat too deep a poetic tinge, and less of the lucid arrangement of ideas and topics than one desires to meet with in a professional treatise. Not that we would for a moment imply that dulness is a necessary attribute of science, but simply that clearness and method are essential to its appropriate elucidation. That they

are peculiarly so in these days, when every body reads and so many write in the hope of being read, will not, we suppose, be questioned.

A point much insisted upon by Dr. Kidd is the absence of all hurry and excitement, or of anything likely to attract the patient's attention, during the administration of anæsthetics. By way of illustration, he states, on the testimony of Russian surgeons in the Crimean war, that their own stolid and impassive countrymen required far less chloroform to produce insensibility, than the French prisoners on whom operations had to be performed, and who were necessarily under the influence not only of the rage of battle, but of the irritation produced by their capture.

A good deal is said by the author upon the relative value of ether and chloroform as anæsthetics, and while he very decidedly leans in his preference towards the former, or at least towards a mixture of the two liquids, he does not, we think, bring out into as bold relief as he should the dangers of the one and the safety of the other. M. Trousseau is referred to as authority for the statement that "of forty-nine sudden deaths, thirty were from chloroform and nineteen from ether." But, as we have declared in another place, "since the proper mode of administering ether has been understood, there is not one authenticated example of its having destroyed life." On the other hand, not a month passes that we are not called upon by the medical journals to lament the fatal effects of chloroform, and we frequently hear of other equally lamentable cases whose record is never published. A gentleman who had seen a good deal of the hospitals in London, Berlin, and Vienna, informed Dr. Kidd that he had himself witnessed eighteen to twenty deaths directly from chloroform.

With such facts before us, we need not wonder if this dangerous agent were ultimately laid aside by surgeons, if not upon moral grounds, which we should hope might be sufficient, then under the stress of penal enactments forbidding or greatly restricting its use.

A. S.

ART. XXVIII.—*An Epitome of Braithwaite's Retrospect of Practical Medicine and Surgery.* Containing a condensed summary of the most important cases, their treatment, and all the remedies and other useful matters embraced in the forty volumes, the whole being alphabetically classified, and supplied with an addenda, comprising a table of French weights and measures reduced to the English standard, a list of incompatibles, explanation of the principal abbreviations occurring in pharmaceutical formulæ, a vocabulary of Latin words most frequently used in prescriptions, and a copious index. By WALTER S. WELLS, M. D. In six parts, making 2 vols. 8vo. New York: Chas. T. Evans, 1860.

THIS comprehensive title expresses sufficiently well the scope and design of the work, which, so far as we have examined, seems to have been well carried out. It will prove useful to those who do not possess the original work. Its arrangement facilitates reference, and its cost is small.

QUARTERLY SUMMARY

OF THE

IMPROVEMENTS AND DISCOVERIES

IN THE

MEDICAL SCIENCES.

ANATOMY AND PHYSIOLOGY.

1. *Saccharine Function of the Liver.* By GEO. HARLEY, M. D.—Although it is now nearly two hundred years since our countryman, the celebrated Dr. Thomas Willis, made the important discovery of the occasional presence of sugar in the human urine, it was not, until very recently, known that the formation of saccharine matter is constantly going on in the healthy animal body.

Since Bernard, in 1848, communicated to the French Academy the discovery of the saccharine function of the animal organism, physiologists in all countries have more or less directed to it their attention. For a time various opinions were held by different observers regarding the origin of the sugar found in the body; but at length it was generally admitted that the liver had the power of forming a substance to which Bernard gave the name of *glucogen*; that this peculiar substance was transformed into sugar; and that the sugar in its turn disappeared in the capillaries of the different organs and tissues of the body.

In the summer of 1858, however, Dr. Pavy read a paper on the "Alleged Sugar-forming Function of the Liver," before the Royal Society, the object of which was to prove that the presence of sugar in the animal economy is "due to a post mortem occurrence;" that as long as life continues, glucogen only is to be found, and not until after death does the transformation of this substance into sugar begin.

The question of the saccharine function of the liver being a subject to which I have more or less directed my attention since 1853, when I communicated to the Société de Biologie de Paris an account of an experimental procedure whereby diabetes can be produced artificially in animals, the above-mentioned paper was to me one of peculiar interest. The conclusions of the author were so much opposed to the results of my own experiments, as well as those of other observers, that I felt anxious to test them.

Accordingly, having received the kind offer of Professor Sharpey's co-operation, I undertook a series of experiments, the results of which I beg the honour of communicating to the Royal Society.

As the experiments performed were merely a repetition of some of those made by previous inquirers, I shall not enter into detail further than is necessary to explain the precautions adopted with the view to avoid error. And, looking at the object in view, it will readily be understood why in the present instance the tests employed for the detection of the sugar were limited to caustic potash with and without sulphate of copper. The mode of proceeding was as follows: In testing the blood, a quantity of distilled water, equal to about four times that of the blood used, was boiled in a capsule. To the water, when boiling, were added a few drops of acetic acid, and afterwards the blood was very gradually intro-

duced. In order that the albumen might be thoroughly coagulated, a drop or two more of acetic acid were added, care being taken to avoid an excess. When the albumen was completely coagulated, which was known by its separating and floating in the clear liquid, the whole was thrown on a filter. The clear filtered liquid was then tested. The same process was followed when operating on the liver.

The first point to be ascertained was whether, under favourable circumstances as regarded diet, sugar could be found in the circulation. The following experiment proved this:—

Exp. 1.—From the carotid artery of a rough terrier dog, three hours after being fed on bread, milk, and boiled liver, a portion of blood, equal to about three-fourths of an ounce, was withdrawn. This, on being treated in the manner explained, gave distinct evidence of the presence of sugar. A second portion of blood, after standing thirty-five minutes in a room of moderate temperature, yielded a similar result.

As in this instance a few seconds elapsed between the withdrawal of the first portion of blood and its treatment with the boiling acidulated water, and as it was possible that in these few seconds the sugar might have been formed from the glucogen present in the circulation, we (Professor Sharpey and myself) thought it advisable in our next experiment to allow the blood to flow directly from the artery into the boiling mixture, and thereby avoid the possibility of sugar being produced by the transformation of glucogen after the removal of the blood from the body. It was further desired to operate on an animal in what might be considered its natural condition as to food. Accordingly one that had been running at large was selected, and the following experiment performed:—

Exp. 2.—Into the left carotid of a small cocker dog was inserted a canula with a stopcock. The animal was then placed so as to allow the blood to flow directly into the boiling acidulated water. The clear filtered liquid from this blood became of a yellow tint on being boiled with soda, and gave a red precipitate with the sulphate of copper and potash, thereby indicating the presence of sugar. Two ounces of blood from the same animal were similarly tested after the blood had stood twenty-four hours in a room of moderate temperature, and the result obtained was the same as with the first portion.

The next experiment was made on an animal under conditions, as regards food, unfavourable for the production of sugar. In order, too, to avoid any chance of injuring the sympathetic nerve during the operation, and thereby favouring the formation of sugar in the body, the blood was withdrawn from the right femoral artery instead of the carotid. The following are the particulars of the experiment:—

Exp. 3.—A good-sized dog was fed solely on flesh during four days. Three hours after the last meal, which consisted of half a pound of boiled horseflesh, an ounce and a half of blood was permitted to flow from the femoral artery directly into the boiling mixture. The solution obtained from this blood, as in the other cases, contained sugar. Another portion of blood, after standing three hours, was tested in the same way, and, as far as could be judged by the eye, contained a similar proportion of sugar.

In neither of the preceding cases was the amount of sugar in the blood quantitatively determined, as I had already done so on many previous occasions; and I knew that in healthy arterial blood it varied according to the state of the digestion, and the kind of food, from an inappreciable quantity up to 0.24 per cent.¹

Having been now satisfied that sugar is to be found in the blood of healthy animals at the very moment of its withdrawal from the circulation, even when none has been introduced along with the food, we next proceeded to test the grounds upon which it had been asserted that glucogen is not transformed into sugar in the healthy liver during life.

In the paper already referred to, Dr. Pavy stated that the sudden abstraction

¹ "On the Physiology of Saccharine Urine," by Geo. Harley, M. D., British and Foreign Medico-Chirurgical Review, July, 1857, pp. 191-204.

of heat from the liver after its removal from the body checks the transformation of the sugar-forming material, and thereby enables us to operate on the hepatic substance while in the same chemical condition as during life. The plan he recommends is to sacrifice a dog by pithing, and instantly to slice off a piece of liver, and throw it into a freezing mixture of ice and salt. In which case, he says, the absence of sugar is almost complete, and thence concludes that the presence of sugar in the liver can no longer be looked upon as a "natural *ante-mortem* condition;" but "is in reality due to a *post-mortem* occurrence."

In the following experiments, not only was the plan recommended most scrupulously followed, but even the risk of the glucogen in the liver becoming transformed into sugar during the process of preparing the decoction was avoided, by cutting the frozen liver into thin slices, and allowing them, while still in that condition, to fall directly into the boiling mixture of acetic acid and water. The liver was in this way prevented from thawing until it entered a medium as capable of arresting the transformation of its glucogen into sugar as the cold. The decoction so obtained might therefore be presumed to contain the soluble matters as nearly as possible in the same chemical state as they were in the living organ.

Exp. 4.—A small, but full-grown dog was fed during fourteen days solely on animal food. Four hours after a meal of boiled horseflesh he was killed by section of the medulla oblongata. The abdomen was rapidly opened, and a portion of liver cut off and instantly immersed in a freezing mixture of ice and salt. A second portion of liver was as speedily as possible detached, and quickly washed in cold water. The latter portion was then, without loss of time, cut into fragments, which were allowed to fall directly into boiling acidulated water. On testing the clear filtrate, distinct evidence of the presence of sugar was obtained. After half an hour, the frozen portion of liver was taken, without being allowed to thaw, and sliced directly into the boiling water with acetic acid. The clear liquid yielded in this case as distinct evidence of sugar as in the other. Forty minutes after the death of the animal, another portion of liver, which till then had remained undisturbed in the abdomen, was treated like the others. This gave evidence of containing a much greater quantity of sugar, thus confirming Bernard's statement, that the transformation of glucogen goes on in the liver after its removal from the body, or after the death of the animal.

In order to be perfectly certain that the sugar found in the liver at the instant of its removal from the body was really formed where it was found, and not carried there by the portal blood from the food, the following experiment was performed:—

Exp. 5.—A dog was fed during ten days on boiled tripe. Twenty-two hours after the last meal the animal was pithed. In less than twenty seconds, a portion of the liver was in the freezing mixture of ice and salt. While I boiled directly another portion of the liver, Professor Sharpey put a ligature on the portal vein, and collected its blood. He likewise collected some of the hepatic blood which flowed from the cut liver.

In the portal blood not a trace of sugar could be detected. The hepatic blood, on the other hand, gave distinct evidence of its presence. Both bloods were tested exactly alike. The clear liquids obtained from the frozen liver and from the portion treated directly, notwithstanding that they were filtered while hot, and also tested while still hot, both gave distinct evidence of sugar. On the following day, a second portion of portal blood, which had been purposely kept all night in order to ascertain if, on standing some time, sugar would form in it, still yielded the same negative result. Even after treating it with saliva, which would have transformed its glucogen into sugar, had it contained any, no evidence of the presence of sugar was obtained. On the other hand, when saliva was added to the decoctions of the liver above spoken of, a great increase in the amount of sugar was observed. The quantity of sugar so obtained did not appear to be so great, however, as that yielded by a portion of the liver which remained all night untouched in the abdomen of the animal.

Professor Garrod, F. R. S., who was present, not at the commencement of the experiment, but on the following day, when the different decoctions were tested, agreed with Professor Sharpey and myself, that this experiment showed the truth

of Bernard's statement, that the liver might contain both sugar and glucogen when the portal blood contained neither.

The stomach and intestines of this animal were found void of food; the large intestine only contained fecal matter.

For the sake of still further assurance that the sugar found in the liver was neither due to some accidental cause, nor immediately derived from food, we determined to deprive an animal of food for some days before examining the liver. The following experiment was accordingly performed:—

Exp. 6.—A very large and powerful dog, in admirable condition, was subject to a rigid fast for seventy-two hours—three full days. Immediately after death, by section of the medulla oblongata, a portion of the liver was sliced off and immersed in ice and salt. Blood was then collected from the following sources:—

1st. From the portal vein.

2dly. From the liver (*i. e.*, blood which flowed from the liver when a portion of it was sliced off).

3dly. From the right side of the heart.

4thly. From the aorta.

5thly. From the inferior vena cava.

Although these bloods were all treated in a similar manner, and tested with the same quantities of copper and soda, yet none of them gave unequivocal evidence of the presence of sugar, except that from the liver. The blood from the right side of the heart gave doubtful evidence. At first sight it may appear strange that the blood from the right side of the heart should contain scarcely any appreciable quantity of sugar, while that of the liver showed its presence very obviously; but this no doubt arose from the hepatic blood being in great part prevented from reaching the heart: 1st, on account of most of it escaping into the abdomen, when the portion of liver was cut off; and 2dly, on account of its flow being in great measure arrested by the ligature of the portal vessels.

All the bloods, except the hepatic, seemed to be free of glucogen as well as sugar; for none of them, with that exception, gave any evidence of its presence after being treated with saliva in the usual way.

On examination of the frozen liver (after three hours), which, as in the other cases, was not allowed to thaw before being put into boiling water, the decoction was found to reduce the copper readily.

On opening the stomach, nothing was found in it except some neutral mucus. The intestines were equally destitute of food, and in the rectum only a very small quantity of feces was found; so there could be no doubt as to the animal being in a fasting condition.

The only point now remaining was to determine quantitatively the increase in the amount of sugar in the liver after its removal from the body, and for that purpose we preferred operating on an animal fed on a mixed diet.

Exp. 7.—A small dog, which had been previously fed on animal diet, received a full meal of bread and milk. Five hours afterwards the animal was pithed, and a portion of the liver rapidly sliced off, and immersed in a freezing mixture. A ligature was placed on the portal vein, and its blood collected before the circulation had ceased.

On examination, this blood was found to contain a small quantity of sugar, derived no doubt from the food. Bernard, I believe, has erred in supposing that all the saccharine matter found in the animal organism is formed out of the glucogen produced in the liver. This, no doubt, is the case in the carnivora when the diet is restricted to food inconvertible into sugar in the alimentary canal, but cannot be regarded as the natural state of things either in the omnivora or herbivora; for the food of the latter not only contains sugar, but its amylaceous elements may be converted into that substance in the process of digestion. The sugar found in the bodies of animals fed on a mixed diet ought therefore to be regarded partly as the direct product of the food, and partly as derived from the glucogen formed in the liver.

Bernard's chief argument against this view is founded on the fact that the livers of dogs fed on a mixed diet contain no more sugar than those fed on purely animal food. In my opinion, however, this fact is not sufficient to decide the question; for, as the liver does not store up sugar, the quantity it at any time

contains is no criterion of the amount produced in it. Moreover, the sugar derived from the food need not be expected to be found in the liver. Had Bernard gauged the sugar present in the blood, instead of that in the liver, after each kind of diet, the result obtained would, I believe, have led him to a different conclusion. This being a point of great practical importance in the treatment of diabetes, I may be here permitted to mention that I have occasionally found nearly twice as much sugar in the blood of an animal on a mixed, as in that of one feeding on a purely flesh diet.

To return to the last experiment. About two hours after the death of the animal, portions of the frozen part of the liver, and of that which had been kept warm in the body of the animal were carefully weighed, and the proportions of sugar they respectively contained estimated by volumetric analysis.

The portion of frozen liver was found to contain 0.333 per cent., and that of the other 1.55 per cent. of saccharine matter. It is thus seen that in two hours the sugar in the liver had augmented nearly fivefold. As Bernard has shown, the simple washing out of the liver by passing a stream of water through its vessels, would remove all the sugar anteriorly formed. On placing it again aside for a short time, a fresh portion of sugar would form in it at the expense of the glucogen.

0.333 per cent. of sugar seems a small quantity; but if we suppose a liver weighing, as in man, not less than 50 oz., to contain 0.333 per cent., above 70 grs. of sugar would be present in it at the moment of death—no very insignificant quantity, when it is recollected that sugar is removed from the liver with every pulsation of the heart, to be partly consumed, and that it is as continually supplied by the organ.

The results of the experiments now related do not, therefore, in any way countenance the notion that sugar is not produced in the healthy animal body. On the contrary, such conclusions as they afford are altogether in favour of the generally received views upon the subject.

From the preceding experiments the following conclusions may be drawn:—

- 1st. Sugar is a normal constituent of the blood of the general circulation.
- 2dly. Portal blood of an animal on mixed diet contains sugar.
- 3dly. Portal blood of a fasting animal, as well as of an animal fed solely on flesh, is devoid of sugar.
- 4thly. The livers of dogs contain sugar, whether the diet is animal or vegetable.

5thly. Under favourable circumstances, saccharine matter may be found in the liver of an animal after three entire days of rigid fasting.

6thly. The sugar found in the bodies of animals fed on mixed food is partly derived directly from the food, partly formed in the liver.

7thly. The livers of animals restricted to flesh diet possess the power of forming glucogen, which glucogen is at least in part transformed into sugar in the liver—an inference which does not exclude the probability of glucogen (like starch in the vegetable organism) being transformed into other materials besides sugar.

8thly. As sugar is found in the liver at the moment of death, its presence cannot properly be ascribed to a post-mortem change, but is to be regarded as the result of a natural condition.—*Proceedings of Royal Society.*

2. *Amyloid Bodies.*—A paper on these interesting substances, by FRIEDREICH and KÉKULÉ contains the details of a case in which a quantity of this material was isolated and submitted to chemical analysis. The authors preface the case by allusion to the view that these concentric bodies arose from the coagulated fibrin of extravasated blood, which underwent chemical change; and especially to an instance of a hæmatocele, operated upon by Linhardt, in which the cyst wall was covered by layers of altered blood-coagulum, which had undergone amyloid transformation in a remarkable way. The case related was that of a woman, aged thirty-six, who for a long time had been subject to tertian ague, ascites, anasarca, albuminous urine, and enlarged spleen. After death, among other things, the cranial bones were found thickened, and at the vertex exhibited a flat exostosis; the liver was shrunken, its capsule being irregularly thickened.

The spleen, besides being large, was of a waxy consistence, in which was an irregular portion of the parenchyma converted into a bloodless mass, of a whitish or whitish-yellow colour in the centre, and grayish-white towards the circumference. The thyroid gland contained old and fresh tubercles, and the end of the ileum and the cæcum were slightly ulcerated. Moreover, cicatrices of the gums, throat, and vagina were found. On microscopical examination, the amyloid degeneration was found to have proceeded in the walls of some of the hepatic bloodvessels, but not at all in the parenchyma of the liver. In the wax-like part of the spleen, the cells, the trabecular work, and the bloodvessels, this degeneration had taken place to an extreme degree, and the central white part was found to owe its colour to much finely granular fat, the result of fatty degeneration of the areolar-tissue-corpuses of the trabecular work. In the kidneys much amyloid degeneration of the vessels and glomeruli existed, and this was the case also with the bloodvessels of the uterus, the uterine and vaginal mucous membrane, the heart, &c.; but in the muscular structure of the heart and uterus, in the vessels of the lungs and brain, &c., this change had not gone on. This change was also found in the minute bloodvessels of the intestinal mucous membrane, and slightly in those of the stomach; also in the villi of the small intestines, and to an excessive degree in the above-mentioned ulcers of the intestines, which appeared to be destructive processes set up as a result of degeneration of the nourishing bloodvessels. The authors here allude to a case of almost entire deficiency of the intestinal villi in a case of amyloid degeneration, related in another place,¹ and also one related by Beckmann,² in which destruction of the mucous membrane of the intestine existed to a great extent. They look upon the case above related as an instance of amyloid degeneration, in connection with an inveterate syphilitic taint, such as have been already noticed.³ A review is then made of several theories entertained by Virchow, Bush, Donders, Moleschott, Naegle, &c., as to the chemical nature of the so-called amyloid substance, and especial reference to that of Meckel, that the blue substance yielded by the addition of iodine and sulphuric acid is owing to cholesterine, a view shown by Virchow to be untenable. Allusion is also made to the negative results obtained in various attempts to convert this amyloid substance into sugar. The paper closes by the description of a chemical analysis of portions of the spleen, which were found to consist almost exclusively of amyloid substance, enabling the authors to come to the following conclusions, at any rate, with regard to the amyloid substance there formed, viz., that although a large quantity of cholesterine is contained, yet this is by no means the cause of the iodine or sulphuric acid reaction; also that the waxy spleen contains no bodies which chemically resemble either amyloil or cellulose. The so-called amyloid substances appear to be only modified and altered albuminous material.—*Brit. and For. Med.-Chir. Rev.*, Oct. 1860, from *Virchow's Archiv*, Bd. xvi. Hefte 1, 2.

3. *On the Conversion of Cysticercus Cellulosæ into Tania Solium.* By Dr. KUCHENMEISTER.—Some time since Dr. Küchenmeister having fed a delinquent with measly pork three days prior to his death, found several young tæniæ attached to the intestinal canal, and Leuckert has since related a case of death from the same cause. But, as there are still incredulous persons, the author resolved to institute other experiments on the person of a criminal condemned to death. The pork containing the cysticerci was administered on November 24, 1859, and January 18, 1860, and the post-mortem was made on March 31. Almost fifty per cent. of the cysticerci were found in the condition of tapeworms. His general conclusions are: 1. The numbers of the tænia which were found must convince the most incredulous of the reality of the conversion. 2. That the tænia really resulted from the cysticerci administered is seen from so many being still in an immature state. 3. The presence of so many examples delayed the development. In ordinary cases a quarter of a year would suffice for maturity to be attained. 4. Even under the most favourable circumstances, when the cysticerci are freed of their envelops, one-half undergo no conversion, and

¹ Virchow's Archiv, Band xi. s. 391.

² Ibid., Band xiii. s. 97.

³ Ibid., Band xi. s. 393, and Band xiii. s. 498 and s. 500.

swallowed unseparated the proportion would be still less. 5. Raw, measly pork may be exposed to considerable cold without the cysticerci losing their vitality. 6. In weather which is not hot enough to induce early putrefaction, the susceptibility of development can be retained for at least eight days, and probably for a longer period after the death of the pig. 7. The greater the number of the raw cysticerci that are consumed, the greater number of tæniæ will be found. In one case in which such food was largely consumed, thirty-three portions of heads were found. 8. In persons leading a quiet life, avoiding all excess in eating and drinking, and partaking of a uniform diet, even many of these worms may not give rise to any disturbances in the system. This prisoner, between the time of eating the cysticerci and his execution, was remarkably well in health, a considerable increase in the deposit of fat taking place. 9. Notwithstanding the quantity of separated proglottides at the lower portion of the intestinal canal, the muscles contained no cysticerci. This man, however, had no vomiting, and none of the embryos of the tæniæ entered the stomach, which, according to the author's investigations, is a necessary preliminary to the appearance of cysticercus in man. 10. The worms were found very firmly adherent in part to the free surface of the intestine, or at the sides of the valvulæ conniventes, and in part buried between these last. They could only be detached with great difficulty, and when the heads were loosened from one portion of the intestine, they at once fastened on to another with just as much force. 11. This explains why a means which, in some cases, acts very efficaciously in expelling the worms, in other cases proves of less avail. Thus, when the head is attached to the free surface of the intestine, or of the valvulæ, sharp purgatives will detach it; but, when placed at the base of the valvulæ, the effect of the purgatives may be only to bury it still deeper, and when the body is expelled without the head, the worm may be reproduced. When the head of the worm has become detached from the intestine, we must not allow it time to re-attach itself; and these medicinal agents alone, which induce powerful peristaltic action, and are accompanied by abundant secretion, are to be relied upon for procuring a radical cure. 12. Finally, the author replies to the reproaches which have been directed against him for his experimental feeding of condemned criminals. He maintains that, as a curable disease only had been produced, the man, even in the event of his having been pardoned, would have sustained no permanent mischief. He declares that, by employing active purgation by means of pomegranate extract, prepared as directed in his work on Parasites, he has never failed in expelling the worm. A further excuse for the experiment is derived from the fact that, owing to the regularity of diet observed by the criminal, and the absence of all excesses likely to give rise to vomiting, there was no possibility of the proglottides obtaining an entrance into the stomach, and becoming thence diffused amidst other of the structures.—*Med. Times & Gaz.*, Oct. 27, from *Deutsche Klinik*, No. 20.

4. *Bodies resembling Spermatozoa from the Urine of a Woman.*—These bodies were found by Dr. Beale in the urine of a patient of Mr. Cæsar Hawkins's, in St. George's Hospital. Many of the organisms were exactly like spermatozoa, and might certainly have been mistaken for them; but on examination of a number of specimens, it became evident that they were really vegetable organisms. This fact is one of great interest and importance with reference to questions of supposed rape. Dr. Beale had never before met with structures liable to be mistaken for spermatozoa.—*Brit. and For. Med.-Chir. Rev.*, Oct. 1860, from *Beale's Archives*, No. 3, p. 251.

MATERIA MEDICA AND PHARMACY.

5. *On the Physiological and Therapeutical Properties of the Peroxide of Hydrogen.*—Dr. RICHARDSON read before the Medical Society of London (Oct. 15, 1860) a paper on this subject. The peroxide of hydrogen, which was dis-

covered by Thénard in 1818, is, in fact, water charged with oxygen in the active state. In his paper, Dr. Richardson took up the following points: The history of the substance; its preparation, with special regard to pharmaceutical applications; its physical and chemical properties; its relations to ozone; its physiological properties; its therapeutical value.

It was obvious from the author's description that some obstacles lie in the way of the application of the peroxide of hydrogen for medicinal purposes, owing to the difficulty experienced in its manufacture. This difficulty, however, Dr. Richardson greatly simplified; and we should infer that any experienced pharmacist could supply the medicine after a short acquaintance with the process of making it, as readily as quinine or other remedial bodies in preparing which time and care are the most important requisites. It was shown, indeed, that if perfectly pure peroxide of barium were supplied to the profession, every practitioner in the country could make his own solution of oxygen as he might want it.

The description of the chemical and physical properties of peroxides of hydrogen was unusually interesting. Passing over the facts relating to the influence of inorganic bodies upon it, those bearing on organic matter strike one most forcibly. Thus blood freed from fibrin absorbs the oxygen from the peroxide, and, if it is venous blood, it becomes arterial, with a rise in the temperature. Washed fibrin and cellular tissue in the fresh state evolve the oxygen. Albumen, urea, gelatin, fibrous membrane, and skin, produce no change. Grape sugar, and indeed all the sugars brought into contact with it, become decomposed, and evolve carbonic acid. Starch undergoes the same modification.

These observations refer to animal substances recently used; but when putrefaction has commenced, then the oxygen of the peroxide seems to act on all alike, and to produce rapid disintegration.

Another curious fact relating to the peroxide was, that its oxidizing power was easily prevented by the presence of certain bodies having a wide extension of names, but analogous characters. Ammonia in vapour or solution, tobacco, hydrocyanic acid, solution of aconite, and, in short, all the narcotics that are miscible with water, possess this neutralizing property; the permanency of the result being decided by the physical character of the agent employed.

The section of the paper on the relations of the peroxide of hydrogen to ozone was an interesting one, and indicated a careful study of this debated question. It is clear that Dr. Richardson looks upon the two bodies as one and the same. If he has any doubt, it is to the effect that in peroxide of hydrogen there is not any affinity at all between the two elements, hydrogen and oxygen. We pass the matter over to dwell on the physiological actions of the peroxide. These seemed to arrange themselves into the following brief propositions: A weak solution oxidizes blood; but this effect can be stopped by the action of the alkaloids and of narcotics. The peroxide supports the life of fishes; but the body of the animal causes rapid evolution of the gas. The solution injected into the left side of the heart of an animal restores the irritability, but appears to have an opposite effect on the right side. Injected into the arterial system immediately after death, it seems to restore to the muscles the power of contracting on the application of an irritant. It suspends to a considerable extent post-mortem rigidity, and it reduces spasmodic action, excited by such bodies as ammonia and hydrocyanic acid. On the therapeutical value of this powerful agent, Dr. Richardson did not dwell long; but reserved this essential point for another communication. He showed, however, that as an antidote to the alkaloidal poisons, as an external application to decomposing sores, as an internal remedy in fever, where the patient literally dies from deficient oxygen, and in diabetes, the medicine might be used with the very best promises of success. In the way of a pleasant acid drink, one could give, said Dr. Richardson, to the typhus-stricken man 100 cubic inches of active oxygen per hour. In diabetes, one fact had been made out also by the author, that, under the influence of the peroxide, the quantity of sugar at once became less, and the excretion of urine decreased in a relative degree. After illustrating his paper by experiment, Dr. Richardson concluded by stating that, in placing it on the annals of the society, he would guard himself, once and for all, from any exaggerated suggestions as

to the value of this new remedial agent. The subject, indeed, was so novel, that after twelve months' study of it he had feared to use a sentence that had not been considered over and over again. He did not pretend to know all the properties of the peroxide. He did not bind himself inviolably to any opinion offered on the present occasion; nay, experience might show that the substance discussed in a medicinal sense took new and even different directions from those with which he had opened the argument. His own intentions and objects would be served if he did but call forth investigation and fact, let the course of events bend in whatever way they might.—*Lancet*, Oct. 20, 1860.

6. *On the Sulphate of Cinchonine in the Treatment of Intermittent Fevers in the French Army.* By M. MICHEL LEVY.—The great expense incurred in the hospitals and infirmaries of the French army by the amount of sulphate of quinine employed in the treatment of intermittent fevers, especially in Corsica and Algeria, has led, besides the extension of prophylactic measures, to the endeavour to obtain substitutes for the quinine. After various proposals, which have met only with indifferent success, the salt of cinchonine has been more recently experimented on with more satisfactory results in the hospitals of Algiers, Oran, Constantinople, Cherchell, Rome, and La Rochelle. The reports of the chief medical officers of these establishments, which have been published, by order of the Minister of War, in the *Recueil of Military Medicine, Surgery, and Pharmacy* for 1859, afford the following interesting data: There was a total of 205 fevers, of which 106 were first attacks, and 99 relapses; the number of cures amounted to 194; the sulphate of cinchonine failed 11 times. Out of 115 of these cases, which had been carefully observed, 11, which had resisted the sulphate of cinchonine, were distributed as follows: 9 out of 58 treated at Oran; 2 out of 6 treated at Rome. In 19 cases, the first dose had arrested the fever; 37 had one attack during the treatment; 16 had two attacks; 12 had three; 2 had four; 2 five attacks, etc. From these reports it further appears that the association of the sulphates of quinine and of cinchonine neither increased nor diminished the individual power of each. In the various trials the toxic effects of the cinchonine were well marked; in the digestive system, nausea, gastralgia, vomiting, colic, diarrhœa (some of which symptoms, however, may have been due to the fever rather than to the drug); in the nervous system, general excitement, headache, vertigo, intoxication, noise in the ears, deafness, disorders of vision, nervous tremors. But although the toxic effects were fully equal to those of the sulphate of quinine, the therapeutic effects were much inferior (3 : 10).

In 1856, at the suggestion of M. Soubeiran, a series of experiments was made at Blidah by M. Laveran, principal medical officer there, on the comparative effects of the sulphate of cinchonine and of quinium, the results of which were published in the *Gazette Médicale*. M. Laveran divided his observations into four parallel series, according as the fevers were treated—1st, by expectation; 2d, by sulphate of quinine; 3d, by sulphate of cinchonine; 4th, by quinium. He directed his attention not only to the attacks of fever, but also to the accompanying disorders, the earthy hue of the skin, the enlargement of the spleen, and dropsies; and to make the comparison more exact, he employed the same quantity of cinchonine and of quinine, instead of using larger doses of the former, as most military physicians do. The general results were, that patients who before admission to hospital had suffered 5 attacks, had, on an average, with the quinine treatment, 1.30 attacks in the hospital; with the treatment by cinchonine, 2 attacks. Patients who had been the subjects of 6 attacks before admission, gave the average, under quinine, of 0.70; under cinchonine, 1.70 attacks. The cinchonine appeared less efficacious in proportion as the fevers were more recent and more intense. According to M. Laveran, neither the quinine nor the cinchonine had any effect on the engorgement of the spleen.

The observations made under the direction of M. Michel Levy in the hospitals of the East in the campaign of 1854, were postponed from the summer to the autumn season, in consequence of the violent epidemic of cholera which broke out at the Piræus, Gallipoli, and Varna. The experiments with the cinchonine were begun, however, in September, in the hospital of the Piræus under the im-

mediate charge of M. Artigues, and in that of Varna under M. Barby. In the hospital of the Piræus the experiments took place in September and October, 1854. The medicine was administered only in presence of the physician, and after one or two paroxysms had been observed. A solution was used of about four grains to $\mathfrak{z}\text{ij}$ water. It was intended to experiment only on cases free from complication, but this was found impossible from the previous bad health of the troops during the summer. 35 fever patients took sulphate of cinchonine; of these, 9 were affected with regular intermittent fever, 8 remittent fever, 5 had fever of an irregular type, and 3 continued fever of a typhoid form. *Regular intermittent fever.*—Out of 9 cases of quotidian, 7 were first attacks, 2 relapses; 7 were simple fevers, 2 complicated, one with bilious symptoms, the other with acute bronchitis. The dose of cinchonine was 8 to 12 grains; 7 cases yielded to the first dose, 2 required a second. *Bilious and inflammatory remittent fever.*—The dose was 15 grains. Seven times out of eight the attack was cut short or diminished by the first dose, and the cure was complete after three doses. In 8 patients the convalescence was fairly established from the seventh to the eighth day. These, however, still suffered from headache and languor, and only slowly recovered their strength. The mean duration of the treatment was thirteen days, and there was no relapse. The medicine was not equally well borne by all the patients; 3 had vomiting; in 2, a diarrhoea of short duration came on; and twice there were noted, dazzling of the sight, vertigo, violent headache, and extreme weakness, symptoms which disappeared as soon as the medicine was suspended. *Irregular intermittent fever.*—Five cases cured. M. Artigues introduces into this group two cases of paludal cachexia, in which the cinchonine was of the same service as sulphate of quinine. *Typhoid continued fever.*—Only one case. The sulphate of cinchonine had no effect, and as the case tended to become malignant, no time was lost in administering the sulphate of quinine, which produced a “heroic” effect. In the hospital at Varna, M. Barby subjected at the same time one series of patients to the quinine, and another series to the cinchonine treatment. He gave double the dose of cinchonine that he did of quinine, administering it during the intermission, eight or ten hours before an attack, and taking care to observe, previous to using it, that the attacks of fever were either increasing in intensity, or, at least, showed no tendency to spontaneous decline in violence or duration. The cinchonine was administered only after the fourth or sixth attack had been observed. 40 patients took the sulphate of cinchonine; but at the time of M. Barby’s report only 22 had been observed to their conclusion. Of these there were 10 regular intermittents, including 8 quotidian and 2 tertian; 1 irregular intermittent; 9 remittent, including 6 quotidian, 1 tertian, and 2 typhoid; besides 2 cases of sub-continued fever. Nearly all the patients had voluminous spleens, and had been previously attacked with fever once or oftener. On an average, each patient took $\mathfrak{z}\text{i}$ \mathfrak{ss} of sulphate of cinchonine; the minimum was \mathfrak{ss} , the maximum $\mathfrak{z}\text{ij}$. *Intermittent fevers.*—The first dose arrested the fever in 3 cases out of 11, the second dose in 4, the third dose in 2 cases. The first dose sensibly diminished the following paroxysm, where it did not entirely prevent it. The two remaining cases required four doses of cinchonine. In these cases the pulse fell in three days successively to 55, 50, 48, and 45 pulsations, which was the inferior limit of frequency. The headaches disappeared completely, and the splenic dulness was in some cases reduced in extent. There was only one slight relapse. *Remittent and sub-continued fevers.*—In 9 cases the first dose was only followed by a diminution in the intensity of the febrile phenomena; but the cure was not more slow than with sulphate of quinine. The same effects were observed in the *typhoid intermittent fevers*. And in some cases of the paludal cachexia not included in these returns, M. Barby ascertained the favourable effects of the sulphate of cinchonine, which were observable at the end of a few days.

These observations, when compared with those made by other medical officers at various stations, present very considerable differences in their results, which may be due to the different climates where the experiments were made, the effects of the season, the vernal fevers being treated by some observers, the autumnal by others, etc. But if all the elements of the problem cannot be

decided, yet there seems to M. Levy sufficient evidence on which to found the following conclusions: 1st. Expectant treatment is not attended by serious inconvenience in simple intermittent fevers under proper hygienic conditions; it is to some extent indispensable in testing experimentally the substitutes for quinine. 2d. Part of the successful cases, ascribed to cinchonine as well as other febrifuges, are due to the well-known fact of the spontaneous decline of febrile attacks. Speedy cures, previous to the occurrence of an attack in hospital, and those of vernal fevers, belong to this category. 3d. The fevers of summer and autumn, although more obstinate than those of spring, present a certain proportion of spontaneous cures. Such were a part of those in the Piræus in September, 1854, cured by a single dose of 8 to 12 grains of sulphate of cinchonine; and probably the fever treated with apparent success, by means of salicine, at the hospital of Calvi, in Corsica, in 1835, were in reality spontaneous recoveries. 4th. If in malarious countries, such as Corsica, Algeria, the Piræus, and Varna, where the miasmatic effluvium is at the maximum, a large number of the fevers get well spontaneously, the proportion of such cases must be much more considerable in temperate climates, such as Paris and other towns of the interior of France, where the paludal influence is much less marked and often very weak. At the military hospital of Lille, the venerable Physician-in-Chief, M. de Chamberet, used to cure intermittent fevers with pure water, distributed to the soldiers in vials, labelled with *Protoxide of Hydrogen*! 5th. The outlay for sulphate of quinine may, in accordance with the preceding remarks, be considerably reduced both in civil hospitals and in the army; the sulphate of cinchonine will be sufficient in the treatment of most fevers which occur in spring and up till the beginning of June, and even in a certain number of cases in summer and autumn. In winter, when there are only relapses without any tendency to the malignant type, the same treatment will do, whether preceded or not by a dose of the sulphate of quinine, or mixed with a small quantity of the latter, as advised by the Council of Health for the army. 6th. No military physician has attempted the employment of cinchonine in the malignant fevers (fièvres pernicieuses); a reserve which is recommended by the results of experiments, and which ought to be imitated in civil practice. 7th. Another source of economy consists in a rational *dosing* of the drug; excessive quantities being used both in Africa and in France. Impartial observation has shown that, even in malarious countries, it is rarely necessary to raise the dose above 12 to 18 grains. 8th. Lastly, the expense for sulphate of quinine to remedy splenic engorgement is, in most cases, sheer waste. In recent cases, the treatment may be of service; but in tumours of the spleen of long standing, the quinine lavished with costly perseverance is found to produce no sensible modification of the enlargement. In conclusion, M. Levy refers to the fact, insisted on by the Council of Health of the army, that there is a singular contrast between the toxical energy of the cinchonine and its therapeutic insufficiency—a subject which deserves renewed research. In the administration of cinchona, the toxical and therapeutic effects of the cinchonine are combined with those of the quinine, and the sum of their actions forms the peculiar value of the cinchona. And if the sulphate of quinine is the resource of the physician in combating fevers, which resist the action of the sulphate of cinchonine, so also, then, are fevers which repel the sulphate of quinine, but yield to cinchona. In fevers which have frequently relapsed, and in those which have induced a cachectic condition of the system, it is the cinchona which deserves the preference.—*Edinb. Med. Journ.*, October, 1860, from *Bull. Gén. de Therap.*, May, 1860.

7. *Mode of Action of Perchloride of Iron.*—At the Academy of Medicine, a lively discussion has been going on relative to the action of perchloride of iron as a therapeutic agent. M. Trousseau, at the meetings of May 29th and June 5th, entered at great length on the doctrines laid down in a paper on the subject by M. Devergie. Practitioners, according to this memoir, may be divided into those who explain the action of such remedies in the purely chemical effect produced by their introduction into the blood, and those who assert that vital action alone is concerned in the results thus produced—that a dynamic, and not a chemical, effect is exercised by them.

According to the theory of M. Burin Dubuisson, a small quantity of perchloride of iron is sufficient to thicken (*épaissir*) the blood so much as to retard or impede the capillary circulation. This action, however, is temporary, and must not be confounded with the regeneration of the blood accomplished by that agent. M. Dubuisson considers, with M. Mialhe, that the assimilation of iron for the formation of blood-globules takes place while the iron is in a state of peroxide, and not of protoxide; that ferruginous protoxides do not act upon the albumen; that they are absorbed; that they circulate in the blood, pass to the lungs, and are converted by the influence of the oxygen inspired into peroxides, and then exercise their influence on the blood. Persalts, however, by not requiring such preliminary transformations, act much more rapidly than proto-salts do—the acid of such salts unites with the soda in the blood, and the peroxide with the albumen, forming an albuminous material at once taking part in the formation of blood-globules.

On the other hand, the partisans of the dynamic theory, while admitting the diminution of red globules in the blood during chlorosis, and their increase in quantity following the administration of iron, do not consider these facts as proving that this iron has been absorbed. They do not regard chlorosis as resulting from diminution in the quantity of iron contained in the blood. It would appear, from recent experiments, that, notwithstanding the paucity of blood-globules, the quantity of iron is not defective; and they therefore conclude that the favourable action exerted by ferruginous preparations is due to the influence they have in exciting the processes of digestion and nutrition—that is, on the digestive apparatus (M.M. Trousseau and Pidoux). According to M. Meran, of Bordeaux, who has advanced certain hypotheses on this matter, the perchloride of iron acts directly on the capillary system by its tonic and stimulant powers—contracting the coats of these vessels, so as to prevent the escape of their contained fluid, and in this way arresting hemorrhage,

Looking at both these exclusive theories, M. Devergie considers neither of them to be well founded. However much the mere chemical action of such substances on the blood may seem to account for their effects, we cannot, says he, institute a comparison between the digestive or vascular system and the apparatus of a laboratory; and, on the other hand, the rapid appearance of ferruginous preparations in the blood, and the immediate effect it produces in hemorrhage, preclude the idea of its action being exclusively of a mere dynamic or exciting nature on the digestive or vascular organs. He rather considers that these preparations act in both ways; that is, by their entering at once into the blood and improving its quality, while at the same time they exert a beneficial effect on those organs on which they seem to have a stimulating influence—thus reconstituting the blood and improving the digestion simultaneously.

The two theories here attacked, and the hypothesis advanced in refutation of them, form a question involving the whole laws of therapeutics, and by its connections extending to the general action of all medicines. M. Trousseau has all along objected to the doctrine of such remedies as this acting exclusively by their chemical properties. At two consecutive meetings of the Academy of Medicine, he has accordingly discussed the question thus opened up; and, in concluding his address before that body, epitomizes the whole drift of his observations in these words: "I have now concluded. The science of therapeutics will be nearer the truth when our ignorance of the mode of action of remedies is more candidly confessed, when each drug is more specially studied, and when experimental research is more sedulously conducted."—*Edinburgh Med. Journ.*, Oct., 1860, from *Gazette des Hôpitaux*, May and June, 1860.

8. *Thermo-Therapeia, or Heat Cure.* By ERASMUS WILSON.—For a knowledge of thermo-therapeia, medical science is indebted to Mr. Urquhart. Thermo-therapeia is the application of atmospheric air at a high temperature to the surface of the body, for the relief of pain and disease. I will endeavour to re-trace my own experience, on my first introduction to the thermæ. It was the winter time, the season bitterly cold; my inception as a "companion of the bath," took place in the private thermæ of my esteemed friend, Mr. George Witt. As an example of simplicity of construction, Mr. Witt's thermæ may be usefully

taken as an illustration. He had at the back of his house, a room twenty feet long by ten feet in breadth, and twelve feet high, with a window looking out upon a lead flat such as is common in London houses. To convert this room into a thermæ he divided it into two compartments by means of a wall which crossed it at about one-third from its further end. He had, thus, two apartments, an outer one, the cooling room or *frigidarium* of the Roman thermæ; and, an inner one, entered by two small doors (inner and outer) in the partition wall, the *caldarium*, *calidarium*, or *sudatorium*. Having left my garments in a portion of the outer apartment which served as a *vestiary*, and girt around the loins with a *cummerbund*, I entered the calidarium; the temperature was delicious, such a contrast with the exterior world. The wind and snow were raging without, while here was a paradise of 135 degrees of Fahrenheit. Within this hallowed nook anxiety, and care, and fatigue, disappeared; I stretched forth my limbs in peace and enjoyment; the brain seemed to think more lightly and pleasantly, and my ideas flowed brightly and calmly.

My friend, Mr. Witt, in the course of a few minutes was streaming with perspiration which ran down his face in rills, and dripped from his elbows and finger-ends in continuous drops, while my skin was as yet dry. I was struck also with the rich and healthy complexion of his skin; it took its hues from the free circulation of the pure arterial stream through the capillary plexus of the derma; as he drew his fingers forcibly across his chest, the white traces left by their pressure were instantly replaced by the glowing vermilion of the arterial blood. There were no gorged capillaries in that skin; no venous transformation in that cutaneous plexus; no deposits of unhealthy colouring matter either in the cuticle or in the tissues beneath; no pallor; no excess and no deficiency of fat; no choked pores; no wrinkles from loss of elasticity and contractility of the fibrous and muscular structures of the corium; no abnormal or deficient sensibility of the nerves; all was, as nature made it, perfect and beautiful. I looked for the first time in my life on a really healthy skin. How very curious and striking was the difference between my friend's skin and that of every one present; one gentleman, a finely-built handsome man, with a remarkably capacious chest, had too great a preponderance of adipose tissue, while the hue of the skin in an oblique light was a bright golden yellow. In another, the muddy tinge of the skin discovered the impure and muddy condition of the blood. The habitual use of the thermæ removes these discolorations, these indications of imperfect elimination, by drainage through the perspiratory system, and while it gives beauty to the skin, bestows health on the entire economy. After a free perspiration of half an hour's duration, I was anointed with soap and had a rub down with a wisp of white fibre called *lyff*, the fibre of one of the palm-trees commonly used in the east for the purpose to which it was now being applied. To the friction with soap succeeded a shower of warm water, then a douche of cold water, after which I was made to sit still for some minutes until the warmth of the skin was restored.

From the calidarium I passed, therefore, to the frigidarium, on this occasion, mid-winter, and a piercingly cold snowy day, truly deserving its name. I was then *cloaked* in a sheet taken from one of the pigeon-holes of the *columbarium* standing in the corner of the room, my *cummerbund* was allowed to drop on the floor, and I was made to recline upon a cane couch immediately under the open window. How cool and pleasant were the puffs of wind that played over my face and limbs; how different their impression on my skin to what they had been an hour before. I needed not the assurance of my friend that there was no fear of catarrh or bronchitis; my own feelings told me that I could resist any amount of cold, and I was obliged to suppress a longing to walk out upon the leads with no other covering than my sheet, into the midst of the sleet and wind.

After awhile I exchanged the horizontal position on the couch by the open window, to a sitting posture; the sheet was thrown off from my back and limbs; the moisture of the surface was dried up, no wiping, excepting of the head and face was practised or required, the skin felt smooth and warm, and I was permitted to dress, but with the injunction that I was to dress leisurely, lest the perspiration, which had ceased, should again be excited. It is worthy of notice, that great attention is paid to the temperature of the skin during the curriculum

of the thermæ; after the cold douche, we return to the calidarium to recover any waste of heat; and in the after cooling of the body in the frigidarium, the whole of the moisture must be dried off the skin, and perspiration must be wholly suppressed, as indicated by a peculiar smoothness and polish of the surface, before we are qualified to resume our dress. All clamminess of the skin must have ceased entirely before we resort again to our usual coverings.

Among my fellow subjects of the thermæ, I have seen numerous examples of relief from painful affections dependent on morbid composition of the blood. Several were cured of gout, of rheumatism, of neuralgia. A clergyman and doctor of divinity, who resorted to the thermæ to reduce redundancy of adipose accumulation, suffered habitually during the winter season from catarrh, bronchitis, and neuralgia, and was often laid up for weeks together with these affections. Since he has adopted the use of the thermæ, which he enjoys excessively, he has diminished in bulk; he has lost all proneness to catarrh and bronchitis, and no longer experiences the pangs of neuralgia. Recently I was much interested in seeing a case of eczema of the face treated throughout by the thermal process alone; the patient lived in the thermæ for several days, he used very high temperatures, and he succeeded completely in curing his disease. It was curious, he remarked, to observe the patches of eruption; they yielded no perspiration, and looked like so many parched up islets in the midst of the surrounding copiously perspiring skin. At about the same time a medical friend consulted me for prurigo senilis. "You know Mr. Witt; go and ask him to admit you to his thermæ," was my counsel. The next time I paid a visit to my friend's thermæ, there was my elderly patient, luxuriating in the fulness of enjoyment. That day he left his prurigo senilis behind him in the calidarium, and I believe has had no reminder of it since. He went back to his home on the coast, and now offers a seat in his own thermæ to his curious or suffering friends.

I have hinted at the curative effects of very high temperatures; and both Mr. Urquhart and Mr. Rolland have mentioned to me important results from this process. It occurred to Mr. Urquhart's mind that as *fever-heat* was represented by 112° he should be able, could he create a temperature higher than fever-heat, to supersede the stage of fever at once. Thus, taking the beginning of the cold stage, which nature seemed to struggle painfully to overcome, he was enabled by a high thermal temperature to cut it short at once and to pass over it and the hot stage to that which nature seemed desirous of reaching—namely, the sweating stage. He believes that at a certain temperature he can put a stop to the fermentative process of zymotic diseases; and, at a higher temperature still, destroy animal poisons. He suggests, moreover, a curious and important inquiry—namely, the influence on the chemical composition of the blood, circulating through the capillary plexuses of the skin, of hot air having a temperature of 160° of Fahrenheit. Not so much its influence on the healthy blood as on the blood of persons in a state of disease.

A member of Mr. Urquhart's family, a child, was accidentally burnt; the burn was distressingly painful; various applications had been made without relief; the child was accustomed to the thermæ, and desired to go into it; it was carried into the thermal chamber, and the pain of the burn was immediately assuaged. Mr. Urquhart, himself, received a severe scald; he betook himself to the thermæ; there, in a heated atmosphere, he directed upon the injured part a blast of air hotter than the temperature of the apartment; the pain became lessened, the process of effusion which results in the production of a blister was arrested; to use a popular expression, "the heat had drawn out the heat."

Looking at the thermæ in a social and political point of view, we find that it is wonderfully adapted for the preservation in health of large bodies of men, combining in itself the respective advantages of air, exercise, and ablution. Adopted by our own army, there cannot be a doubt that it would very considerably reduce the rate of sickness and death, and add to the efficiency of the men. It is applicable also in all cases where numbers of persons are collected together, as in barracks, prisons, poorhouses, factories, and schools; in large business establishments, where a considerable number of young men or young women are assembled; or in places of temporary meeting, as the House of Commons, and

clubs. It must always be borne in mind that the thermæ not only offers advantages as respects physical health, but it also conduces to moral vigour. But the usefulness of the thermæ has even a wider sphere; the Londoner, or the inhabitant of a large city, would live as healthily immured within his city walls as the rustic amidst the fields and meadows of the country. His thermæ would be to him in the place of a country house, of a horse; it would give him air, exercise, freshness, health, and life.

I might add very materially to the long list of conditions to which the thermæ might be applied with advantage, but I limit myself to a single one more: it is that of extensive works, employing a large number of men, either in operations in themselves unsalutary, or in unhealthy localities. The importance of preserving a body of working men in a state of health, and in the best condition for the performance of their duties, must strike every one, and is an object worthy a moderate sacrifice on the part of proprietors or owners. There are many localities in which miasmatic fevers abound, and constantly incapacitate the working force of large operative establishments. I believe that a few pounds expended in thermæ would correct this evil; would put the men into condition to resist the miasmatic force, and to eject the poisonous elements from the blood when they had already found admission into the organism.—*British Med. Jour.*, Oct. 13, 1860.

9. *Thapsia Plaster*.—This is a new therapeutical agent, the active principle of which is derived from the *Thapsia garganica*, an Algerian plant. It is a most energetic revulsive, the effects of which can be graduated by the duration of its application, replacing blisters in serious affections, and rubefacients in the milder ones. It induces an erythema, which is speedily followed by an abundant and salutary miliary eruption. Its action is rapid, more certain than that of croton oil, and its employment is exempt from the numerous inconveniences attributed to some other external agents. It may be employed in any cases in which revulsion is indicated, but it is especially useful in diseases of the chest, rheumatism, and arthritis; it is also serviceable in the affections of children.—*Med. Times and Gaz.*, Nov. 17, from *Gazette des Hôp.*, No. 60.

MEDICAL PATHOLOGY AND THERAPEUTICS, AND PRACTICAL MEDICINE.

10. *Treatment of Delirium Tremens by Large Doses of Digitalis*.—G. M. JONES, Surgeon to the Jersey General Hospital, extols (*Med. Times and Gaz.*, Sept. 29, 1860) the efficacy of large doses of digitalis in the treatment of delirium tremens. "Experience," he says, "has taught him that the best dose is *half an ounce* of the tincture given in a little water. In some few cases, this one dose is enough, but generally a second dose is required four hours after the first. In some cases, but very seldom, a third dose is called for; but this hardly ever need exceed two drachms. The largest quantity I have ever given was *half an ounce* at first, *half an ounce* four hours afterwards, and another *half ounce* six hours after that—making an ounce and a half in ten hours.

"As to the effects of these doses, my impression is that the action is on the brain, not on the heart. The pulse, so far from being lowered in force, becomes fuller, and stronger, and more regular, soon after the first dose. The cold clammy perspirations pass off, and the skin becomes warmer. As soon as the remedy produces its full effect, sleep for five, six, or seven hours commonly follows; sleep is the guide as to the repetition of the dose. No action on the kidneys is evidenced by an unusual secretion of urine. Sometimes the bowels are slightly acted on, but not commonly. I have never once seen any alarming symptom follow the use of these large doses of digitalis. The only case I have lost since adopting this treatment had a tumour in the brain. In three only was other treatment adopted, after digitalis had failed to procure sleep; in other words, in

sixty-seven out of seventy cases digitalis was the only medicine used, and sixty-six of these patients recovered. I do not mean that these are the exact numbers of those treated; I am certain as to the death, but I may have had more recoveries. I am well within bounds in saying seventy cases in twelve years, and that all of them were well-marked cases of delirium tremens. Slight cases of nervous derangement after drinking I have seen in great numbers; but I speak here only of such cases as required active treatment. My previous experience of the results of the treatment by opium, or some of its preparations, by antispasmodics, etc., had certainly been much less successful; the proportion of deaths was larger, and the recovery much less rapid. Again; I have treated more than one patient successfully by digitalis, who, in subsequent attacks elsewhere, has been treated by opium and died; and in many of the cases in which I have used digitalis successfully, opium had been previously given without any good effect."

Mr. J. was led to adopt this practice from observing the effects of a large dose of the medicine *given by mistake* to a patient, whose condition seemed desperate, and who recovered under effects of the article.

Dr. T. HERBERT BAKER, in a communication to the *Med. Times and Gaz.* (Oct. 20, 1860), says that his "experiments point to the fact that the tincture of digitalis, of all the forms of the drug, possesses the lowest degree of toxic power; and there can be no doubt that the quantity of tincture which may be given without destroying life is much greater than is usually imagined. Indeed, I suspect that it will yet be found that in some morbid conditions of the system the tincture of digitalis may be advantageously administered in still larger doses than have been recommended and given by Dr. A. T. Thomson, Mr. King, Dr. Clutterbuck, Mr. Jones, and others. Whether the comparative harmlessness of the tincture arises from the variable quantity of digitalis in the alcoholic solution, or whether the alcohol of the tincture somewhat modifies the action of the digitalis, is yet to be determined. Dr. Ballard's remark on digitalis as a counter-poison suggests to my mind that in the tincture we may possibly be administering at once the bane and the antidote."

Mr. WM. CARR, in a letter to the same journal, states that he has used the tincture of digitalis in large doses, as recommended by Mr. Jones, since 1858, and with the best results. "In no case," he says, "has it failed to produce relief to all the urgent symptoms—tremors, delusions, and insomnia."

"In some cases," he adds, "where the nervous system has been undermined to the extreme by lengthened indulgence, I have combined chloric ether, in doses of thirty minims, giving such every half hour, with three drachms of the tincture of digitalis, repeating the remedies until two ounces of the latter had been taken, when I have ceased for twenty-four hours; after which I have repeated the medicine in less full doses, measuring the quantities needed by the condition of my patient, the amount of sleep obtained, the degree of tranquillity enjoyed, and the state of pulse, quiet or tremulous."

11. *Treatment of Epilepsy—Belladonna—the Ligature—the Actual Cautey.*—Dr. BROWN-SÉQUARD generally commences the treatment of epilepsy by belladonna. The usual dose of this remedy for an adult is one-quarter of a grain twice a day in pill or mixture. It is very rarely indeed seen to produce any of its specific effects, as dilatation of the pupil, in cases of epilepsy. At our last visit one patient came who complained of dimness of vision, and whose pupils were evidently dilated by the drug; but this was the exception, proving the rule, as the case was not one of epilepsy or any other convulsive disorder.

In the cases in which there appear to be a tendency in the fits to appear at regular intervals, for instance once a fortnight, Dr. Brown-Séquard prescribes quinia in large doses, *e. g.*, five, ten, and even fifteen grains, to be given at intervals, shortly before the fit is expected. By this means the fit is frequently prevented, and the patient goes on to the next, or even to a longer period. In reference to these large doses of quinia, it is well known that some temporary deafness will often follow, and curiously enough, Dr. Brown-Séquard states that there is a kind of deafness which the administration of this remedy in large doses will cure.

Another therapeutical means in epilepsy is the ligature, in cases in which the aura epileptica, arising from one of the limbs, is present. Dr. Brown-Séquard has two patients, both girls, about the age of nine years, in the hospital, in whom the fits are frequently stopped in this way. The ligature is kept constantly on the arm; when the child feels the warning, the nurse of the ward tightens the bandage, and the fit is prevented. The success in these cases has been very great, and we shall, shortly, by the courtesy of Mr. Smith, the house surgeon, be enabled to place their details before our readers. It is of great consequence to have the ligature in readiness, so that it may be tightened at once. Grasping the limb tightly will do in the absence of proper means, but it is much better to keep a bandage or folded handkerchief tied on the arm ready to be tightened. Dr. Brown-Séquard has invented an apparatus to encircle the arm, and to tighten by a screw, in order that the pressure may be quickly applied.

Dr. Brown-Séquard frequently uses the actual cautery locally in a variety of nervous affections. In epilepsy, patients frequently complain of either a pain or a sensation proceeding from some part of the body. A woman, aged 20, had had fits for thirteen years; they invariably commenced with pain in the left side, just below the mamma. Dr. Brown-Séquard applied the cautery to this part in two or three places. The relief was most marked. It had not prevented the fits altogether, but it had reduced their number very considerably. Instead of having them every other day, she had them only once a week. The cauterizing iron is heated to a white heat, and is then applied suddenly to the part once or twice. It appears to cause but trifling pain, and the patients do not seem at all to dread its repetition.—*Med. Times & Gaz.*, Oct. 27, 1860.

12. *Diabetes treated by Hot-Air Baths.*—Mr. G. M. SWINHOE relates (*British Med. Journ.*, Nov. 3, 1860) the following case:—

“W. W., a mechanic, a married man, aged 40, applied to me in November last, 1859, with all the symptoms of diabetes, viz., general debility, extreme aching pain in the loins, dry tongue, great thirst, voracious appetite, constipation, dry cough, etc. The specific gravity of his urine (of which he passed from nine to twelve quarts in the twenty-four hours) was 1040; it contained plenty of sugar. His previous history was that of a moderate man, and for the last two years he had been a teetotaller. His work as a spring-maker subjected him to extreme and frequent changes of temperature. I prescribed iron and opium, and regulated the diet so as to exclude all articles containing sugar as much as possible. Under this treatment, he improved, and continued at his work until the end of the following April, when, feeling himself much better, he could not resist the temptation of a dish of potatoes, of which he freely partook, and came to me as bad as ever. I now added to my former treatment vapour baths; but these produced so much exhaustion, that I was obliged to discontinue them, and he to give up work. I now determined to try the hot-air baths, and sent him to London for the purpose. From these he derived such benefit that he was soon able to resume his work; and, having built a bath at the back of his cottage, he has been able to take them regularly, and to give up medicine for the last three or four months. He is now as strong as ever he was in his life; and, notwithstanding a total disregard of all diet regulations, his urine is now scanty, with a specific gravity of 1018, and has lost its sugar.”

13. *Diabetes.*—We have already noticed (No. for Jan. 1860, p. 229) the researches of Dr. Griesinger relative to diabetes, and we shall now give some further particulars. Dr. G. has had occasion to treat nine cases of diabetes, two of which were of the variety insipidus. Eight of the cases proved fatal, and in all *post-mortem* examinations were obtained. In order to draw out as complete a history as possible of the disease Dr. G. has collected, from various sources, 217 sufficiently detailed cases; his conclusions are accordingly based upon 225 observations.

Directing attention, in the first place, to the causes of diabetes, Dr. Griesinger narrates two cases where there might be a suspicion that the disease had been produced by the excessive use of sugar, or of saccharine aliment. A diet too exclusively vegetable, and especially if containing much starchy matter, exer-

cises a marked influence on the production of diabetes. In one case the disease seems to have been produced by a fall; in reference to this mode of causation, Dr. Griesinger found, on analysis of his 225 cases, that in 20 the disease appeared to have had a traumatic origin. In these cases the disease should rather be ascribed to the general shock to the system, than to any special cerebral lesion.

The disease was found to be distributed between the sexes in the following proportions: out of 225 cases, 172 men (76.4 per cent.) and 53 women (23.5 per cent.) were affected. With regard to age, Dr. Griesinger confirms the general opinion with regard to the rarity of the diseases in childhood and in old age; the period when diabetes is most common in either sex is from 20 to 40.

In the majority of cases, tuberculosis supervenes after the disease has existed a certain time. Dr. Griesinger has also observed among diabetic patients a great tendency to inflammations, accompanied with suppuration or gangrene. A constant diminution in the animal temperature was noticed several times. Dr. Griesinger attributes this diminution to an insufficiency of nutrition. In one case the presence of sugar in the urine and in the sweat was found to alternate.

Since the interesting researches of Claude Bernard on the function of the liver, the opinion has been advanced that diabetes might depend upon hypertrophy of this organ. But this opinion is contradicted by an appeal to facts; for in only one case was the liver found to be a little increased in size, and here there had been no hepatic symptoms during life, and after death no large quantity of sugar was found in the organ. It may be stated generally, that the progress of the disease appears to afford little support to the theory which ascribes the production of diabetes principally to the liver. Dr. Griesinger seems disposed to admit that the disease is due to a derangement of the digestive functions, which itself is subordinate to a lesion of innervation. The kidneys were not found to present any marked alteration from the healthy condition. In fact, the result of the *post-mortem* examinations would lead to the belief that diabetes is rather a functional derangement, than an affection produced by a special organic disease.

In reference to treatment, Dr. Griesinger shows the good effects of alkalies, particularly of bicarbonate of soda, administered sometimes to the extent of half an ounce a day. The author has endeavoured to discover what influence other substances exert upon the progress of the disease. Acids augment the quantity of sugar, so do alcoholic liquids. Yeast has been administered, but with no good effect. Attempts have also been made to facilitate the oxidation of the sugar by the inhalation of oxygen, of chlorine, and of ozone, but without success.

The author investigated the point, as to whether diabetic patients may be allowed to drink as much as their thirst prompts them, or whether it is better to cut off a portion. He found that water, taken in large quantity, certainly augments the quantity of urine and of sugar; that a slight diminution produces no effect; that privation from liquids produces a prompt diminution in the amount of sugar; but that the effect of privation is quite temporary, for as soon as the patient has fully quenched his thirst, he passes in the urine large quantities of sugar.

Consequently, Dr. Griesinger concludes that alkaline remedies and an animal diet are the only means at the disposal of the physician in the treatment of this intractable disorder.—*Gaz. Méd. de Paris*, from *Archiv. für Phys. Heilk.*

14. *Enemata of Sulphuric Ether for Ascarides.* By T. OGIER WARD, M. D.—Having read in one of the periodicals that injections of sulphuric ether had been of great service in ascarides, I determined to recommend its use to a patient who had long been troubled with thread-worms. It was used as directed; viz., in a dose of fifteen drops in one ounce of water, which was retained in the rectum, the patient going to bed immediately afterwards. The result was, that the patient, a lady, aged about 36, was not annoyed again for above a fortnight, and then only very slightly; and a repetition of the enema kept her free for three weeks longer; so that she flattered herself she had met with something like a cure for this troublesome complaint; and I also entertained a similar idea, having found the treatment successful in another case of ascarides in the adult. This patient, like the other, found the ether most effectual at first, but it is now quite

useless; and, with this feeling, I thought it a subject of sufficient importance to bring under the notice of the late meeting. Since that time, however, I find the thread-worms have returned, though not in such numbers as formerly; but it is a curious fact that the ether seems now to have lost its effect in this case, the lady being compelled to use it every night.

As this patient complained that she tasted the ether in a few minutes after the injection, I tried its effects upon myself one night just before bedtime. In three minutes by my watch, I perceived a strong taste of ether; and, on going to bed, my wife asked if I were unwell, and had been taking ether!

Another effect of the ether injection is, that it causes the patient to sleep very heavily, which property may be turned to good account in cases of sleeplessness in persons who cannot bear opiates. From the history and treatment of many cases, I am convinced that the common notion, that the *habitat* of the oxyuris is in the rectum, is quite erroneous. A single injection of quassia, salt, or even cold water, will completely empty the rectum of all its denizens, so that a repetition will not bring away a single individual; and yet the next night they will be as numerous as ever. Indeed, the fact that persons liable to worms are most troubled *after* the bowels have acted, may be taken as a proof that they are carried down into the rectum together with the feces; which, however, may not contain a single specimen. I am, therefore, disposed to believe that the thread-worm resides in the sigmoid flexure, or in the cells of the colon; and I am supported in this view by having often found that a repetition of an injection will bring away the ova without a single worm. If my idea be correct, our treatment should not be directed to removing them from the rectum, but to destroying them in their dwelling-places by aloes and other bitter substances mixed with the food of the patient.

I may add, as an instance of apparent communication of worms from one person to another, that the husband of one of the ladies has been more or less troubled with the thread-worms ever since his marriage, seventeen or eighteen years ago. He has used the ether once, with the effect of relieving himself for some weeks from the ascarides; and though they have returned, yet they have given him so little annoyance that he has not cared to repeat the ether injection, as he dislikes its smell and taste.—*British Med. Journ.*, Oct. 6, 1860.

15. *Action of Hydrochloric Acid upon Phthisis.*—Dr. R. P. COTTON presents a very instructive report (*Med. Times and Gazette*, Nov. 17, 1860) on the effect of hydrochloric acid upon twenty-five in-patients of the Consumption Hospital.

"Of the twenty-five patients, seventeen were males and eight females. Their respective ages varied from sixteen to forty years. Ten were in the first stage, four were in the second stage, and eleven in the third stage of the disease. In twelve instances the mineral acid was given alone; in thirteen cases it was combined, during part of the time, with cod-liver oil. The dose of the acid varied from ten to fifteen minims of the dilute hydrochloric acid of the Pharmacopœia, mixed with peppermint-water, and administered three times a day. In three cases it was tried for only a fortnight, but in all the rest it was continued for periods varying from four to thirteen weeks.

"Of the twenty-five patients, eleven *greatly improved*, six *slightly improved*, and eight received *no benefit*. Of the *greatly improved* cases, seven were in the first, two were in the second, and two in the third stage. Of the *slightly improved* patients, one was in the first, one in the second, and four in the third stage. Of those who received *no benefit*, two were in the first, one in the second, and five in the third stage.

"Sixteen patients gained in weight, eight lost weight, and in one there was no alteration. The changes in weight were particularly noticed in reference to the cod-liver oil. In six cases, although no oil was taken, there was a great increase of weight (an average of six pounds to each patient); but in all the rest, who either did not or could not take the oil, there was more or less loss of weight. Without reference to the oil, however, those *greatly improved* were found to have increased in weight, although such increase bore no direct proportion to the amount of improvement, some who had gained the least having been quite as much benefited as any of the rest.

"The improvement was in several cases very marked indeed, both locally and generally; the disease appearing to be arrested, and the patients declaring themselves 'quite well.' This was especially noticed in three cases, in one of which the disease was already in the second stage; in two of these no cod-liver oil had been taken, in one this remedy had been occasionally added to the acid. Two other persons, who had actual vomicae, also improved very decidedly, the pulmonary secretion greatly diminishing, all the general symptoms subsiding, and the patients ultimately leaving the hospital materially improved in every particular. Of the seventeen more or less improved cases, seven took no oil, while in ten it was occasionally taken in combination with the acid; in two of the latter cases the oil seemed to make little if any difference, but in at least four it appeared to contribute materially to the general result.

"In five of the patients who were obviously benefiting under the hydrochloric acid, the experiment was made of changing it temporarily for an equivalent dose of *liquor potassae*. In one of these there was no marked effect, the patient appearing to do equally well under either acid or alkali; but in the other four the change was more or less prejudicial, the patients unhesitatingly affirming that they were progressing less than when taking the acid. Much care was used in making this observation, the patient's own words being in each case recorded.

"In very few instances did the hydrochloric acid at all disagree. Now and then a little gastric pain was complained of, but in no case was it necessary permanently to abandon its use. As a general rule, the appetite greatly improved under its administration.

"For some years past I have frequently prescribed for phthisical patients the mineral acids in conjunction with gentian and other vegetable tonics; but I became anxious to examine, as far as possible, the separate influence of the acids. The frequency with which consumptive persons suffer from dyspepsia, the fact that the free acid frequently occurring during healthy digestion is the hydrochloric, together with the well-known solvent effect of this acid upon the plastic constituents of the food, pointed rather to it as the proper object of the experiment than to either the nitric or sulphuric acid. I have no reason, however, to think that either of these acids, or the compound known as the nitro-hydrochloric acid, may not be equally beneficial; but upon this point I hope to make further observations.

"After making due allowance for other influences, so favourably brought into operation at the Consumption Hospital, I cannot help coming to the following conclusions:—

"1. That the mineral acids are well suited to a large number of phthisical cases.

"2. That the dilute hydrochloric acid especially, in doses of ten or fifteen minims twice or thrice a day, is an important auxiliary to other treatment, and may oftentimes be usefully employed, either alone or in conjunction with other mineral or vegetable tonics."

16. *Uræmia*.—A paper on this subject was read before the Medical Society of London (Nov. 5, 1860), by Dr. RICHARDSON. He first pointed out the analogies which exist between uræmic poisoning and poisoning by certain common narcotic substances, such as opium and belladonna; and then passed to the description of cases in which sudden symptoms of uræmia had terminated quickly in dissolution. Thence proceeding to the diagnosis of uræmia, he passed through each phase of the disorder, particularizing the symptoms with much care. Three points in this part of the paper may be mentioned as of interest. The pupil, Dr. Richardson has observed, is usually fixed in uræmia, and in most cases is dilated; but, he added, this rule is not without exception, for he has seen the pupil contracted to a pin's point in a case of unmistakable uræmia. There is in some cases, as Frerichs has said, evidence of an excess of ammonia in the breath during the acute attack; but this is not universal, and hence some have denied it altogether. The reason of a difference in this respect in different cases is very simple. In persons suffering from kidney disease, and in whom uræmia is a probable occurrence, the breath at the best of times is charged with ammonia to an extent greater than is normal. In these cases the lung is supplementing

the kidney, and the elimination of the ammoniacal product is, in fact, the saving clause. It is when such persons take congestion of the lung, and are subjected to diminution of excretion by the lung, that the uræmic symptoms advance; and in these cases the breath is not ammoniacal during the attack. But there are other examples, where the uræmia is sudden in its appearance, owing to sudden arrest in the function of the kidney simply. Then the breath is markedly ammoniacal in the period of the acute attack. The third fact, as diagnostic of uræmic poisoning from poisoning by the ordinary narcotics, is, that during uræmic coma the patient will often rally and regain all his consciousness for a time, sinking again into forgetfulness, and even dying unconscious in the end.

The cause of death in uræmia formed the matter of another section of the paper, and was followed by observations on treatment. In the treatment of uræmic narcotism coming on suddenly, in a person not debilitated by previous disease, and not overloaded with fat, Dr. Richardson stated his belief that there was one ready and direct remedy, and that was free bloodletting. He had seen a man who had lain three days comatose and unconscious recover, under the immediate influence of loss of blood, so completely as to transact business affairs, and inquire into all that had occurred since he was struck down. Moreover, physiological reasons supported this treatment; for the bloodletting not only relieved the body from a portion of the poison, but removed the congestion of the kidney and of the other organs, and gave the permit for recovery, if recovery were possible. Thus, in animals in which artificial uræmia had been produced, the effect of frequent venesection tended greatly to prolong life. That bloodletting should not absolutely relieve in every case was reasonable, for whether relief were obtainable or not in any case would depend upon the degree of mechanical obstruction in the kidney; for if the obstruction were perfect, no treatment would be possible, seeing that no proceeding could be adopted to supplement the kidney altogether; but if in any instance there should be but partial obstruction, increased temporarily by congestion, then the act of abstraction of blood gave the only chance that remained of removing the burden from the excreting organ. The last part of the paper dwelt on uræmia in its forensic aspects. In many cases, where death is supposed to have occurred from the effects of small doses of opium or other narcotic, he (Dr. Richardson) believed that the cause was attributable to uræmia, and that so-called idiosyncrasies were probably intimately connected with renal disorder.—*Lancet*, Nov. 17, 1860.

17. *On Uræmia*.—Prof. JAKSCH holds that there are two varieties of uræmia which should be carefully distinguished; one being caused by the decomposition of urine and the absorption of carbonate of ammonia into the blood (ammonæmia), the other being the variety which accompanies Bright's disease of the kidneys. He has seen the former occur under the following circumstances: 1. In torpor and paralysis of the bladder; 2. In dilatation of the pelvis and calices of the kidney in consequence of the ureters being blocked up; and 3. In renal abscess, renal tuberculosis, and sacculated kidneys.

The following are the main differences characterizing the two forms of uræmia. We shall, to save circumlocution, use the word ammonæmia as the name of the one, and Bright's uræmia as the name of the other.

1. In advanced ammonæmia the urine discharged from the bladder manifests a strong ammoniacal odour, which Professor Jaksch has never noticed in any stage of Bright's uræmia. 2. Dropsical symptoms, either acute and febrile, or chronic and afebrile, have not been observed in ammonæmia. 3. Advanced ammonæmia is characterized by persistent dryness of the mucous membrane covering the mouth and fauces, as if every particle of moisture had been removed by blotting-paper; the membrane looks dry and shining, and the dryness even extends to the mucous membrane of the nose, the conjunctiva, and even to the chordæ vocales; these symptoms do not occur in Bright's uræmia. 4. The distinctly ammoniacal odour of the air exhaled, and of the cutaneous secretions of patients affected with ammonæmia, does not occur in Bright's disease. 5. Patients suffering from ammonæmia always show a marked dislike to meat, and especially brown meats, even if their affection has not advanced very far; a feature rarely seen in the other variety. 6. Professor Jaksch has never observed

in Bright's disease the violent intermittent rigors, simulating intermittent fever, which occur in ammonæmia. 7. In none of the cases of ammonæmia were convulsive or epileptiform attacks, nor croupy or diphtheritic exudations, noticed. 8. Disturbed vision, as produced in Bright's disease by exudation on the retina, does not appear to take place in ammonæmia. 9. Chronic ammonæmia is characterized by a uniformly pale and sallow complexion, and by gradually increasing emaciation; very acute and advanced ammonæmia is associated with very rapid wasting of the features, and muscular debility amounting to paralysis. 10. In all cases of ammonæmia which ran a rapid course there was vomiting, with concurrent or consequent diarrhœa; in chronic ammonæmia both phenomena were often entirely absent, or only occurred temporarily. 11. In ammonæmia, whether acute or chronic, Professor Jaksch has always seen death occur after sopor, varying in duration from several hours to several days.

The author of this valuable and interesting paper gives numerous cases illustrative of his views, and enters very fully into the various questions connected with diagnosis and treatment, for which we are unable to make room.—*Brit. and For. Med.-Chir. Rev.*, Oct. 1860, from *Vierteljahrsschrift für die praktische Heilkunde*, xvii., 1860.

18. *On the Resorption of Pleuritic Exudations.* By Prof. SKODA.—The resorption of pleuritic exudations frequently takes place very slowly, because the capillary vessels in the sub-pleural connective tissue are obliterated. This may be the result of shrivelling and disappearance of the connective tissue newly formed from the exudation, as then, in consequence of the arrest of the metamorphosis of tissue between the blood and the exudation, endosmose and exosmose cannot duly take place. It is not until after the lapse of months or years, when the fluid portion of the exudation has penetrated through the false membranes investing the pleura, that its resorption occurs. In the first case, internal medicines can, of course, avail nothing, as in them we possess no means of exciting the re-formation of vessels. What has been said explains the action of iodine injected in exudations, inasmuch as, by exciting inflammation, it causes the development of new vessels, and so induces the resorption of the effusion. But this view does not, perhaps, encourage us to the frequent employment of thoracentesis and subsequent injection of iodine; for this proceeding is by no means so safe as the corresponding operation for hydrocele. But, apart from that consideration, the injection of iodine or nitrate of silver into the pleural sac can answer no useful purpose; as, on the one hand, in consequence of the presence of the albuminous exudation, the caustic influence of these agents cannot reach the pleural sac, particularly as by the chemical combination which these substances form with the effusion, their power is altered and exhausted, so that the fluid must in the first instance be pumped out, which violent and sudden evacuation may be attended with evil consequences. But, on the other hand, in effusions of long standing, which have already attained to partial organization and shrivelling of the product of inflammation, the injection will be inefficacious, because the lung can now no longer fill the space previously occupied by the fluid exudation, especially as the investing false membranes must first be broken up by the lungs, which is not conceivable. But even if this should take place, a sudden evacuation could be followed by no favourable result, because necessarily there must be ruptures of the pleuritic adhesions and bursting of the compressed pulmonary parenchyma. Therefore, in a pleuritic effusion of long standing, it is only exceptionally that puncture is admissible, when the exudation is so considerable as to depress the diaphragm, to displace the mediastinum, and so to compress the lung that danger of suffocation supervenes. But how can the resorption of pleuritic exudations be induced? Experience shows that all those means which lower the pressure of the blood or augment the secretions, and therefore promote the separation of water from the blood, effect a diminution of the fluid effusion. Accordingly, venesections and diuretics may be indicated in cases of effusion; but these effects also occur spontaneously. In chronic exudations, which are already organized, such means will even be rather injurious, and the indication will be to employ remedies capable of dissolving solid exudations. Such remedies are iodine and mercury. The cautious employment of these

means may therefore be adopted, and they are particularly suitable for external application. Professor Skoda has for some years employed these means experimentally, and has often seen pleuritic exudations rapidly diminish after the use of mercurial ointment, iodine ointments, iodide of glycerin, and black oxide of copper in the form of ointment. It is self-evident that in all chronic pleuritic effusions the diet must be good, in order as much as possible to counteract their injurious effects upon the system at large.—*Dublin Quart. Jour.*, Aug. 1860, from *Vierteljahrschrift für die praktische Heilkunde*, 1860, Bd. LXV.

19. *Pathology of Milky Serum*.—Dr. C. T. COOTE, Assistant Physician to the Middlesex Hospital, relates (*Lancet*, Sept. 7th and 15th, 1860) a case of *Piarrhæmia* accompanying acute diabetes mellitus; and draws from a comparison of the facts collected by it the following conclusions:—

1. *Piarrhæmia* consists in an excess of saponifiable fat in the blood, not in the mere liberation of fat from its combinations.

2. The excess of fat in the blood may be the result of two causes—viz:—

(a.) The excessive ingestion of fat (as in *piarrhæmia* during digestion).

(b.) The diminished elimination of the same (as in hibernation and pulmonary diseases).

It is not quite clear to which of these categories alcoholism belongs. It is conceivable that its elements may be *directly* converted into fat by deoxidation; but it seems more probable that the conversion is effected *indirectly*, the hydrocarbon of the alcohol attracting to itself that free oxygen which would otherwise have been employed in the combustion of the fats of the food, and so permitting the accumulation of the latter in the blood.

3. Fat, if directly ingested, may enter the blood with the chyle through the thoracic duct; but it is clear from the present case that it may also be elaborated in, and absorbed directly from, the liver.

4. *Piarrhæmia* is not a *result* of diabetes mellitus, for either may exist without the other. Both seem to be consequences of the same derangement of the functions of the liver which overloads the blood, sometimes with an excess of sugar alone, sometimes with an excess of sugar and fat combined.

Why the liver should deal so differently in different cases with the hydrocarbons submitted to its influence it is hard to say. It seems not improbable that sugar alone is elaborated in the first instance, and that the excess of fat is the result of a deoxidation of this substance; for the conversion of sugar into fatty substances, is not only capable of being effected experimentally (as in the production of butyric acid by fermentation of sugar under the influence of casein), but has been shown to take place in the animal economy, in the formation of wax by bees fed only on sugar.¹

5. The pathology of blood milky from molecular albumen must be considered as still almost wholly negative. It is probably never an independent affection; but neither is it a mere accidental consequence of *piarrhæmia*. Its apparent relation to albuminuria seems to point to some organic change in the constitution of the plasma of the blood itself.

20. *Period of Incubation of Vaccination One Year*.—Dr. HODGES stated to the Obstetrical Society of London (Nov. 7, 1860) that in May, 1854, he had vaccinated a little boy three years of age, but the arm did not rise within the usual period. In the May following, however, a vesicle spontaneously formed, with an areola on the seventh and eighth days, gradually declining on the eleventh or twelfth; a permanent cicatrix, marked by pits, remaining, giving evidence of the genuine vaccine disease.—*Med. Times and Gaz.*, Nov. 17, 1860.

¹ Miller's Chemistry, vol. iii. p. 738.

SURGICAL PATHOLOGY AND THERAPEUTICS, AND OPERATIVE SURGERY.

21. *Dislocation of the Ulna forwards at the Elbow, without fracture of the Olecranon Process.*—EDWIN CANTON, Esq., relates (*Dublin Quarterly Journal of Med. Sci.*, Aug. 1860,) the following case of this extremely rare accident which occurred under his care in the Charing Cross Hospital. F. P., aged 40, a somewhat short, slim-built, but muscular man, while driving in a light cart at the rate of seven or eight miles an hour, was thrown out, and instinctively extended his right hand to prevent injury to his head. The weight of his body, however, caused sudden and forcible flexion of the elbow, and at the same time the forearm became twisted in under the chest. On rising, it was found that the elbow was considerably swollen, and the power of moving it entirely lost. When admitted into hospital, the forearm was forcibly flexed, and the hand supinated. The swelling, ecchymosis, and tension around the elbow were so great that it was with difficulty any of the more salient anatomical peculiarities of this part could be recognized—everything appeared, in every way, so disarranged. The skin covering the inner condyle was stretched to the utmost, and here, over a space about the size of a sixpence, it was to such a degree injured, that a compound state seemed to be momentarily threatened. The antero-posterior and lateral diameters of the joint were increased in extent, and the general swelling was so great as to present a circumference far beyond the normal size of this region. Externally and somewhat anteriorly the cuplike cavity of the radius could be indistinctly distinguished; internally, the condyle was unduly prominent; anteriorly no particular point for diagnosis could be determined on, on account of the state of forcible flexion and great tumefaction there; posteriorly, also, the swelling was very considerable, but, below it, there existed a depression favouring the view that the ulna was broken immediately below its olecranon process. No median gutter, with lateral elevations to bound it, could be felt.

Attempts were made to rectify the mal-adjustment, but without success; the efforts, however, could not be longer continued, for, it was obvious that the injury already sustained by the soft parts was so extensive—the obstacles to be overcome so resistant, and the great likelihood incurred of rendering the case one of the compound kind, forbade further trial, and it was agreed, in consultation, to place the limb at rest on a splint, and to keep the parts cool with an arnica lotion. Within the course, however, of forty-eight hours the tumefaction became still greater; a large slough was forming on the inner side of the joint, and high constitutional irritation having set in, I was obliged to amputate the limb at a sufficient distance above the articulation.

Dissection.—A very careful examination of the elbow was made, under my superintendence, by my pupil, Mr. Edgar Browne, with the following results:—

Bones.—The ulna was dislocated forwards, so that the upper surface of its olecranon process became placed in front of the capitellum humeri, and had thus assumed the position naturally occupied by the head of the radius during flexion of the forearm. The radius was supinated and maintained *in situ naturale*—as regards the ulna—by the coronary and interosseous ligaments being intact.

Ligaments.—Of the anterior ligament, the only part remaining at all perfect was a shreddy portion about the centre; all the rest of it had been torn through. The posterior, and both lateral ligaments were completely divided. The coronary and oblique ligaments were uninjured.

Muscles.—The triceps extensor was detached from all its points of insertion. The supinator radii longus was uninterfered with at its origin; but the two radial extensors of the carpus beneath it were torn away from the surfaces whence they spring. All the muscles which arise from the external condyle—with the exception of the supinator radii brevis, and anconæus—were detached from this process. The only muscle that was torn through at its origin from the internal condyle was the flexor carpi ulnaris—the olecranon and ulnar portions of it, however, continued intact. No mischief whatever had happened to any other

of the pronators and flexors. The biceps and brachialis anticus were put greatly on the stretch.

Bloodvessels.—Though much shifting of their position had necessarily taken place, no vessel of large size had been injured; the sacrifice, however, of smaller ones must have been great—judging from the large amount of blood with which all the soft textures were infiltrated.

Nerves.—The ulnar nerve was torn across where it passes behind the inner condyle. The sheath of the median was distended, and its substance permeated with blood. The other nerves uninjured.

22. *Treatment of Fractures in the Royal Infirmary.*—JAMES SPENCE, Esq., Lecturer on Clinical Surgery, gives the following account of the treatment he adopts, in the Royal Infirmary, Edinburgh, in cases of fracture:—

“In cases of fracture of the shaft of the femur, I employ the long splint, together with two short splints, fixed with slip knots on each side of the broken bone, to prevent any lateral displacement. In fractures of the neck of the femur, the long splint, without the lateral support, is used; but in many cases of old infirm persons, in whom the use of the splint cannot be continued, I have recourse to the following plan: A long narrow pillow or pad is placed between the legs, extending from above the knee to below the ankle, and the injured limb is secured to the sound one, by bandages at the knees and ankles, a broad flannel spica bandage being applied round the pelvis, and over the upper part of the injured thigh, so as to fix the thigh and pelvis. I prefer this plan to the double incline of pillows, as it not only insures greater fixity and extension, but also enables us to place the patient occasionally on the sound side, and so diminish the risk of bed-sores forming on the back. In cases where, from extensive bruising of the body or limb on the injured side, or other causes preventing the use of the splint, and where, at the same time, full extension is desirable, I apply the long splint on the sound limb, and then keep the injured limb fully extended, by fastening it to the sound one thus fixed.

“In cases of simple fracture of the bones of the leg, I find the simplest and most satisfactory plan is the use of two lateral pasteboard splints with foot-pieces, moulded to the limb, secured by slip knots, the limb being laid on the outer side, and flexed to a greater or less degree, according to the nature of the fracture. In cases of very oblique comminuted or compound fractures, or in cases of fracture of the tibia very high up, the use of Liston's splint, properly padded and adjusted, is the treatment I resort to. In fractures near the malleoli, attended with splitting of the lower part of the tibia, or separation of that bone from the fibula, in which the heel and foot are retracted, and the bones of the leg projected, and where the tendency to retraction, recurring after coaptation, is considerable, the stirrup splint well padded and placed on the front of the leg (used either alone or combined with narrow lateral splints), affords a fixed point to which we can bring forward the heel by bandaging, whilst, at the same time, it presses upon the bones of the leg, and prevents them projecting forwards. In cases of Pott's fracture of the fibula above the malleolus, attended with eversion and twisting of the foot, Dupuytren's splint, applied along the tibial side of the leg, is used as a fulcrum over which the foot is kept inverted, whilst, at the same time, slight extension is preserved. For some cases attended with retraction of the foot as well as eversion, the stirrup splint may be used with advantage, the inner horn of the stirrup being made the fulcrum over which the foot is kept inverted, whilst retraction is obviated by bandaging the heel forwards to the splint. Of course, in the corresponding fracture of the malleolar part of the tibia, with inversion, the Dupuytren splint is applied on the fibular side, to keep the foot everted. With exceptional instances, I have always employed these simple methods, so long and successfully used in this hospital; and after very extensive experience in treating fractures in Hospital, Dispensary, and private practice, I see no reason for changing to other methods, which certainly could not afford me better results, and which seem to me attended with some risk, from which these ordinary plans are free. I allude, particularly, to the use of the starch bandage, and the plaster of Paris methods of treatment, which are now much used on the Continent. In regard to the starch bandage

applied after the lapse of some weeks, when all risk of swelling, or other results of the local irritation produced by the accident have passed away, and, as a means of abridging the period of the patient's confinement, I not only have no objections to it, but have recourse to it very generally. But, used from the very first, I think it objectionable, as preventing the surgeon from observing the state of the limb, and guarding against mischief. The advantage which some claim for it, that the patient may walk about with a crutch in a day or two after the injury, I look upon as no advantage; but, on the contrary, I think the patient would be then much better in his bed, even supposing he felt the inclination to walk about. The plaster of Paris method, of course, has all the disadvantages of hiding the fractured limb from the surgeon's view, and, if I may judge from personal experience, when working with that material in taking casts, it must be anything but pleasant to the feelings of the patient, when it contracts in setting. I have seen a considerable number of cases of fracture of the leg put up in this manner in Germany, and I can easily understand that many such cases do well enough; but it is quite as likely that, in many cases, the mass of stucco in which the fractured limb is embedded, may be, very literally, a whited sepulchre. In a word, I think when we possess well-trying, simple, and successful plans of treatment, we should be very chary in departing from them. Novelty is not always progress, and, unfortunately, many novelties in surgery at the present day seem to consist in departure from simplicity of treatment."—*Edinburgh Med. Journ.*, Nov., 1860.

23. *Fractures of the Base of the Skull.*—JAMES SPENCE, in a clinical report (*Edinburgh Med. Journ.*, Nov., 1860), adverts to the "successful results of some of the severer fractures of the base of the skull, as showing that we should not despair even of such cases; for, by carefully watching symptoms and applying proper remedial measures, we may assist nature in the curative process. I would specially notice, as an example of this, the case of G—n, in which there was evidence of extensive fracture, with great bleeding from both ears and nose, injury of ethmoid, and subsequent fetid discharge from that region. The most remarkable recovery, however, of fracture of the cranium was that of the child whose head was fairly crushed by a large cope-stone (so heavy as to require two men to lift it from off the child). When brought into hospital, her head seemed flattened; the cranial bones felt loose, as if broken up; and the eyeballs protruded. The child was completely insensible from compression, and remained so for nearly three days. At first, recovery seemed almost impossible; and the treatment was necessarily limited to the application of sinapisms to the epigastrium and feet, cold cloths to the head, and enemata; yet, when she once began to rally, she recovered rapidly, and wonderfully little deformity of the head remained. On leaving the hospital she seemed quite lively and intelligent."

24. *Excision of the Knee-Joint.*—R. G. H. BUTCHER, Esq., in one of his interesting reports in operative surgery (*Dublin Quart. Journal of Medical Science*, Nov. 1860), gives the following directions for excision of the knee-joint:—

"1. *The judicious selection of the case.* The bones not being diseased far beyond their articular surfaces; while, if upon section found to be a little more than had been expected, the part should be gouged out, or an additional thin slice removed; but if to a greater extent, amputation should be at once resorted to, and, as recorded in my first memoir, with a hope of excellent success. Again, the report goes on to show that amputation may be performed some days after excision, should any unfortunate circumstance in the management of the case have arisen to demand it. In this same paper seven instances are recorded of amputation of the thigh, and all made rapid recovery, save one.

"2. *The H incision should be preferred.* The perpendicular strokes placed well back, so as to allow all fluids and discharges to drain off—far more effective and safer than any opening made in the popliteal space.

"No portion of the flaps to be curtailed, though they may be thinned of any thickened fibrinous matter or diseased synovial membrane; the latter, particularly, should be clipped away with a strong scissors. All ligamentous fibres,

both around and within the joint, should be cut through, and the extremities of the bones fairly freed and exposed.

"3. *The patella should be taken away in all cases, whether diseased or not*, and then the section of the bones, well thrust out in front, should be made with 'Butcher's saw' from behind forward, due attention being paid to the axis of the thigh-bone at the time of its division.

"4. *All bleeding vessels should be tied, or any that have sprung and retracted should be drawn out and secured*, so as to guard against intermediary hemorrhage.

"5. *While the patient is yet on the operating table the limb should be placed in the horizontal position, either by gentle and steady traction; combined with pressure of the cut surfaces of the bones backwards, or, if necessary, the division of the hamstring tendons.* Their support behind, in every case, I look upon as of great value, therefore their section must be looked upon as a last expedient towards straightening the limb.

"6. *During the adjustment of the bones great caution should be exercised, that their surfaces be throughout their extent in contact, and that no soft parts intervene.* The flaps should be then laid down, and connected by suture closely throughout their transverse division, while the lateral incisions should be brought together only at their extremities by one or two points, and the central portion of each, that corresponding to the division of the bones, should not be brought in contact, but dressed lightly with lint soaked in oil, thus securing a ready outlet for the escape of fluids. The extremity should next be cautiously laid upon 'Butcher's box splint,' padded to the natural configuration of the limb, its sides elevated, footboard applied, suitable pads introduced, and then the anterior splint laid on, taking the place of the assistant's hand, which, from the first, restrained the femur from projecting forward; then the straps buckled, the waist-band applied, and the patient may, with safety, be removed to his bed. The bed should be prepared in this way, and consist of a couple of hair mattresses laid one upon the other, evenly supported, and intervening between the upper one and the sheet a folded blanket, feather pillows for supporting the head and shoulders; the bed should be, likewise, moderately warmed, so as to prevent the patient being chilled when put into it.

"7. *The limb should not be disturbed for several days; the length of time depending a good deal on the season of the year when the operation is performed, whether it be in the heat of summer or the cold of winter.* After five or six days it may be necessary to let down the sides of the box splint, to sop up discharge, change lateral pads and soiled dressings, &c. By the apparatus named the facilities for cleansing the limb are so efficient that it may not be requisite to lift the member from its support for even so long a period as three weeks, as evidenced in my own practice. Should, however, it be considered expedient to change all the dressings, the anterior splint should be steadily held back by an assistant, and the limb pressed up to it, thus guarding against any starting of the femur forwards, or displacement laterally, when lifted from its bed. When the box is prepared, freshly arranged, the limb, controlled after the manner mentioned, should be laid down, the side splints elevated, footboard secured, and the straps over the anterior splint first tightened so as to maintain it in that position, from which it was never suffered to change. I would impress this advice still further: if the straps be unloosed for any purpose, *the hand of an assistant should steadily keep the anterior splint in its position, and well pressed back, until the artificial support is again brought to bear upon it, and fastened.*

"8. *In cases where large abscesses form in the vicinity of the excised joint, or up along the thigh, Chassaignac's drainage tubes may be used with the best hopes of success.*

"9. *The free administration of stimulants and sedatives, imperatively demanded in all cases of excision, regulated, to a certain extent, by age, sex, temperament, and habit."*

25. *Employment of Plaster of Paris Bandages for Dressing Stumps after Amputation.*—Dr. TRANEUS, of Sweden, states (*Edinburgh Med. Journ.*, Nov., 1860) that during the last two years many successful experiments have been made

at the Infirmary of Uddevalla, upon the application of fixed dressings of gypsum to amputated limbs. The dressings were applied as follows:—

“When the hemorrhage has been stayed, the threads of the ligatures are not to be confined as usual, but kept together, and led in the shortest way to the margin of the wound. Instead of plaster dressings, strips of cotton, impregnated with dry plaster of Paris, or gypsum, and dipped in water just before use, are then applied; that is, the limb may be, as usual, enveloped in a compressing roller 12 or 15 feet long, impregnated with gypsum on both sides. The tourniquet may either be removed, or, if that is not advisable, applied to the main artery, at such a distance from the wound that compression is possible. Six or eight straps, impregnated with gypsum, two or three inches broad and a foot long, are then laid transversely over the wound, crossing from one side to the other, so as to keep the edges close together, only taking care that the ligatures be first laid hanging loose over the margin under those bands. Finally, another roller, as long as the first, and impregnated in the same way, may be laid over all these, to retain the whole dressing in its place. When this dressing has dried, it is not to be disturbed for three or four days; but, when it is wet through over the sore with the discharge from the wound, an opening is to be made with a knife and pincers or scissors, at points corresponding to the seats of the ligatures, whereby the matter from the wound may be removed, and its surface, if necessary, cleansed, and the ligatures, as usual, attended to. These little openings, of an inch in diameter, can easily be closed by any common plaster or ointment, and allow of the wound being inspected daily, especially as they may be enlarged by cutting away more of the dressing during the three or four following weeks. After this time the atrophy of the limb enables the surgeon to pull off the whole dressing like a cap, when the wound is generally healed.

“The superiority of this method of dressing consists in the following, viz.:—

“1st. The dressing is agglutinative in itself and in regard to the surface of the skin.

“2d. It is not liable to be affected or injured by any discharge from the wound.

“3d. It permits of being opened with the greatest facility.

“4th. It is not at all injurious to the surface of the wound.

“To obtain the necessary gypsum sufficiently calcined, there should be procured a large piece of the gypsum commonly known in commerce, which is almost as hard as white marble, gray in colour and crystalline in texture; this should be broken into pieces of not above one cubic inch, spread in a single layer on an iron plate, in an oven, and exposed to a common oven heat, at least one and a half hour, till it becomes white and easily breaks down into powder; in this condition it should be preserved in a jar, with a ground glass stopper, and pulverized when required.

“Rub the gypsum prepared as above, upon both sides of two twelve feet long roll bandages (which are afterwards loosely rolled together), and six to eight linen bandages, one and a half foot long and two inches broad. These, with a basin of water, are to be placed as near the patient as is deemed convenient; and each bandage and stripe, just before the application, must be thoroughly wetted. One of the roll bandages is first applied, in descending turns, each turn, at least, half covering the other, as a ‘contentiv-binda,’ in, at least, two layers. The wound being a clean one, the edges, drawn together as close as possible, are fastened with ‘*suturæ nodosæ*,’ according to circumstances, but the extremities of the ligatures are not fastened as commonly, upwards on the stump, *but are laid along the line of the wound*—the reason for this will be afterwards explained. The provided stripes are now, in succession, taken at both ends, immersed in water, and their middle applied across the wound, after which, the ends are firmly pressed upwards on the stump, so that the wound is kept together by a double layer. It is preferable so to apply them, that one or two small openings are left between the edges of the stripes, for examining the state of the wound. Outside these stripes is afterwards applied a gypsum wet roll bandage, to fix the stripes, but according to the common rules of the art. Within twenty or thirty minutes, the bandage is hardened, and the patient can, without inconvenience, be placed in bed. In cases of amputation of fingers or toes, the application of gypsum bandages being simpler, requires no particular

description. The only really dangerous symptom which is now to be feared, is after-bleeding, which, if venous, will soon stop from the even pressure, partly of the 'contentiv-bandage,' and partly from the bandage surrounding the stump; but if the bleeding is arterial, and cannot be stopped by the tourniquet, ice in a bladder should be tried, and 'astringentia and sulphuric acid,' failing which, the gypsum bandages must be removed.

"An inconvenience may arise from continued severe pain, which the patient sometimes suffers, both after gypsum, as well as starch bandages. In two such cases have we been prevailed upon to remove the bandages, but without being able to discover any reasonable cause for unusual pain. In two other cases, the bandages have been allowed to remain untouched, notwithstanding many complaints, and, on the final removal, no inconvenience followed. I must add that, in these cases, no pressure took place from too tight a bandage, as the limb, from every side, being equally pressed, had, as is always the case, diminished below the normal size. The formation of an abscess is possible, but the matter should easily escape by the fine canal formed by the ligatures. In one case, we put a hank through the abscess, and, in two weeks, saw it filled up with 'granulation.'

"Although I have not seen erysipelas, mortification, or other complications take place under this system, yet, should they occur, they could be treated in the usual way, retaining, in part, the gypsum bandage. On the third day, it is usual to loosen the ligatures, as also to discharge any matter, &c., that may have gathered; but by this system, it is only necessary to cut one or two holes in the bandage, where it is moist, opposite the margin of the wound, which can be easily done by raising the bandage by pincers. If the openings have been made at the right place, the ligatures will appear in the line of the amputation wound, and can easily be drawn out. By gentle pressure, the matter can be removed as well as cleansing by injection, through these openings, which, if desirable, can be afterwards covered with a small compress, anointed with some simple salve; during the following days it may be necessary to make a couple of openings besides, for the removal of the sutures; but this should not be hurried, for the longer the bandages can remain unmoved, the surer is it that the healing 'per primam intentionem' is advanced.

"To ascertain if all is going on well, in three weeks, that entire part of the bandage that covers the face of the stump, can be cut away, the circular portion of the bandage that remains will prove a protection and support to the newly formed edges of the healing wound. Generally, in most cases, the bandages can remain unmoved from three to five weeks, when the wound will be found well healed."

26. *The Alcoholic Treatment of Wounds*.—MR. A. PRICHARD, of Bristol, states (*British Medical Journal*, Nov. 3, 1860) that his attention was called a few months ago to a pamphlet published in Paris by M. Batailhé and Guillet, bearing the title *Alcohol and Alcoholic Preparations in Surgery*, in which the authors sum up the advantages of applying 'strong alcoholic compounds to recent and other wounds in the following way, viz., that they check or prevent suppuration, and consequently phlebitis and pyæmia, and that they favour union by the first intention; and the facts brought forward, although few and meagre, induced me to give the plan a trial; and I will briefly narrate some of my cases.

The particular alcoholic preparation which was recommended was the compound tincture of aloes, or, as the French call it, *elixir de longue vie*, and it is made of aloes, myrrh, saffron and spirit; and it is said to be most valuable in contused and lacerated wounds, involving various tissues, being particularly useful in lacerated wounds of the hand, when tendinous, muscular, cutaneous, and osseous tissues are often damaged together. But, that there may be no questions about originality or priority of the discovery of this method of treatment, I will quote a paragraph on the subject from that most entertaining and valuable work, John Bell's *Surgery*, published about sixty years ago. He says: "The process of saving the hand of a workman, when thus mangled with his tools, is this: You are to take up the arteries first, then return the bones into the wound, if they project; stitch the skin over them, draw together the open

spaces with slips of adhesive plaster, and dress the outside by dipping pieces of lint in camphorated spirits and laying them along the wounds"—with a bandage afterwards, and a splint if necessary.

To this I may add, that an old book upon drugs, in my possession, says that myrrh is used with success externally "in wounds, tumours, gangrene, and rotten bones," and that it "attenuates, discusses, and resists putrefaction;" and among the veterinary medicines in Gray's *Supplement to the Pharmacopœia*, the compound tincture of myrrh, which is made of myrrh, aloes, and spirit, is called "the most common of all traumatic applications for healing wounds."

M. P. relates five cases which he treated with the compound tincture of aloes differently diluted, and states that his conclusion from them and various others in which he tried the remedy, including ulcered legs, superficial burns, contused and incised and lacerated wounds, and an ulcerated stump, is the following, viz., that it is an excellent application for recent wounds, however deep; that it checks the suppurative process in a marked degree, and to that extent not only expedites the cure, but by destroying the fetor which always accompanies the treatment of these cases, adds much to the patient's comfort; that it favours union by the first intention, by assisting in the adhesion of the deeper structures particularly, and by preventing the formation and lodgment of pus; that it is not suitable for inflamed wounds, nor where there is an erysipelatous state of the skin, nor in fact for simple cutaneous wounds, where careful apposition alone will suffice to procure immediate union.

27. *Rodent Ulcer*.—JONATHAN HUTCHINSON, Esq., Assistant Surgeon to the London Hospital, has published in some recent numbers of the *Medical Times and Gazette* a very interesting statistical report of forty-two cases of rodent ulcer, or Jacob's ulcer, as it is also called. The following are Mr. Hutchinson's deductions in regard to this affection:—

"1. That there occurs not unfrequently on one or other part of the face a form of ulceration which is characterized by an indurated edge, and by a tendency to spread to adjacent structures, without regard to difference of tissue; which is very slow in its progress; does not cause much pain; does not induce cachexia, and is never followed by enlarged glands or deposits in the viscera.¹

"2. Sections of the indurated edge of this ulcer (or of the portions of new growth which are sometimes produced about it) do not exhibit the cell-structures met with in epithelial or scirrhus cancer, but only those of organizing fibrous tissue.

"3. This ulcer differs from lupus exedens in that it never occurs in the young, and never gets well spontaneously, while lupus exedens but rarely begins after the age of thirty, and usually tends after the lapse of time to cicatrize spontaneously. The two, also, further differ, in that lupus has a tuberculated, inflamed border, without any great degree of induration; while the edge of the ulcer in question presents an extremely indurated ridge, without tubercles, and comparatively free from inflammatory congestion.

"4. The ulcer in question differs from cancer in that there is but seldom present any tendency to the production of new material, that it never causes the glands to enlarge, nor induces morbid growths in the internal viscera.

¹ In making this assertion I am borne out by all the facts hitherto recorded. Fully acknowledging, however, the near relationship of rodent ulcer to cancer, I have but little doubt that it will now and then so far deviate from its usual course as to affect the glands, and quite anticipate in the future to hear of such a case. Epithelial cancer may be said to almost never affect the internal organs, yet a few cases are on record in which it has done so. Such exceptions, however, only prove the general rule; and just as the epithelial cancer very exceptionally affects the viscera, so will rodent very exceptionally affect the lymphatics. Professor Langenbeck has mentioned to me a case in which he excised a rodent ulcer from the side of a woman's nose, who afterwards remained well for nine years, and was then attacked by cancer of the uterus, followed by secondary growths and death. Such a fact is, however, very different from one in which the cancerous infection should advance, as in other malignant disease, through the lymphatic system, from the original ulcer.

"5. Although it must be freely admitted that this disease is closely allied to cancer, and that in its inveteracy under treatment, and its tendency, if not removed, to spread deeply and extensively, it well deserves the designation of 'locally malignant,' yet it is inconvenient in practice to call it 'cancer of the skin,' since there are other forms of cutaneous cancer (the epithelial, scirrhus, melanotic, etc.) essentially different from it, and of a far higher degree of malignancy.

"6. The term 'a peculiar ulcer occurring in the eyelids,' is too vague, and also involves an erroneous statement as to uniformity of location, an objection which, also, in addition to what has been stated above, applies to 'cancer of the eyelids,' since this ulcer is met with on many other parts besides the palpebræ.

"7. To the designation of rodent ulcer given to this disease by Lebert, and adopted in this country by Paget (see *Lectures on Surgical Pathology*) no objection applies, excepting that it is more vague than desirable. Of those in use it is certainly the best, and should the disease become generally recognized by the profession under that name, the vagueness of its meaning will by custom soon cease.

"8. The rodent ulcer is most commonly met with between the ages of 50 and 60, and is equally frequent in the two sexes.

"9. It occurs but very rarely on any other region than the integument of the face, and is most common in the eyelids.

"10. It is a singular and very significant fact that no case has yet been recorded in which the rodent ulcer attacked the lower lip, either primarily or by extension, while that part is well known to be a very frequent seat of epithelial cancer.

"11. The *diagnosis* of rodent ulcer is usually easy. An ulcer with a hard sinuous edge, situated on some part of the skin of the upper two-thirds of the face, of several, or, perhaps, many years' duration, almost painless, and occurring in a middle-aged or elderly person, of fair health, and without enlarged glands—such a sore is almost certain to be of the rodent type.

"12. The *prognosis* of rodent ulcer varies with the stage of the disease and the treatment it is intended to pursue. If left to itself it will slowly but surely advance both in extent and depth, and will probably destroy the patient's life in the course of from ten to twenty-five years, death being eventually produced by the exhaustion consequent on suppuration, hemorrhages, pain, etc., and very probably aggravated by inability to take sufficient food owing to the diseased state of the mouth. If the case be seen in an early stage while complete removal either by knife or escharotics is practicable, a favourable opinion may be given as to the probable non-return of the disease. The younger the patient the more rapid will be the course of the disease, and *vice versa*; and the younger the patient the more nearly is the disease allied to cancer, and the more likely to recur after removal.

"13. The only *treatment* which the rodent ulcer admits of is local, and the best is that which obtains its freest removal with the least injury to the parts concerned. In some localities, and in some stages, escharotics, such as the chloride of zinc, may be advisable, but in most excision and transplantation of skin is the more certain and satisfactory.

"14. A widely-diffused knowledge of the true pathology of rodent ulcer may be expected to result in considerable advantage to the sufferers from that disease, since it will encourage to the early and free adoption of local measures and to the employment of excision and transplantation even in some cases which, if considered cancerous, would certainly be beyond relief by surgical art."

The following further remarks on the treatment may be quoted:—

"The rodent ulcer is to be viewed, as far as our knowledge extends, as a local disease, which spreads by continuous growth. There is not a tittle of evidence in favour of the belief that it can be influenced by internal medication. The object which the surgeon must keep in view is to remove the whole of the diseased parts, and to procure a soft, supple cicatrix. If there be any tension on the cicatrix, the irritation thereby induced will be almost certain to induce a relapse of the disease. By means of deep escharotics, such as the chloride of zinc, complete destruction of the diseased parts may in many instances be insured, and a

very healthy and pliant cicatrix is usually obtained by this remedy. In many of the favourite seats of rodent ulcer, the eyelids to wit, the use of escharotics is very inconvenient; and in these, free excision, followed by the transplantation of healthy skin from the forehead, cheek, or temple, is the best measure. The details of such operations will vary with each individual case, and do not form part of our present report."

28. *Rupture of the Rectus Abdominis.* By M. LEGUEST.—Cases of rupture of the rectus are rare, so that M. Nélaton only found it occurring in four instances out of forty-nine cases of rupture of muscle. A case related by Boyer is especially interesting as it was completed by an autopsy. A young man complained of pain in the abdomen during vomiting, and after death the two ends of the ruptured muscle were found separated from each other to the extent of an inch by effused blood, the rupture taking place at the tendinous interstice situated opposite the umbilicus and the first lower intersection. The subject of the present case was a soldier, aged 27, of good muscular development, who, a week before, while performing some gymnastic exercise, was attacked with severe pain in the abdomen, which increasing and being accompanied by swelling, he came to the Val de Grâce. At three centimetres above the pubis a hard, well-defined tumour was observed, which was not increased in size on coughing, and exactly followed the course of the right rectus; increasing in breadth towards the umbilicus from two and a half to four centimetres. After four days' rest it had diminished one-half in size, not then being more than three fingers in breadth, and in three days later only a slight hardness remained. The case thus terminated very well; but it might have been otherwise, for the epigastric artery at about midway between the umbilicus and pubis enters the substance of the right rectus, giving it branches. These or the trunk itself might become ruptured by the effort which caused a healthy muscle to yield, and thus give rise to a diffused aneurism. This would the more be to be feared as the muscle is here only covered posteriorly by loose cellular tissue and peritoneum. All authors speak of only a small quantity of blood being effused, but in this case the tumour reached from the umbilicus to within three centimetres of the pubis. It is therefore probable that some small arterial branches were divided, and that the arrest of the bleeding was due not only to the retraction of their ends but to the compression exerted by the effused blood itself. The only thing to be feared, then, was the purulent transformation of the coagulum; but rest, regimen, cold applications, and the good constitution of the patient averted this mischief.—*Med. Times and Gaz.*, Nov. 3, from *Gazette des Hôp.*, No. 76.

29. *Cure of the Deaf and Dumb.*—Considerable interest has been excited in the Parisian scientific world, by the alleged successful treatment, by a lady, of several deaf and dumb children. The facts, as detailed in the *Gazette des Hôpitaux* and other Journals, appear to be the following: About the month of August, 1855, Mdle. Cléret, a private teacher, residing in a populous locality in the suburbs of Paris, asked assistance from the Minister of Public Instruction, founding her request, among other motives, upon her discovery of a method of restoring hearing to the deaf and dumb. This means discovered by chance, she had successfully employed in a number of cases, after having proved its efficacy on herself; it consists in pouring into the ear 4, 5, 6, or 8 drops of sulphuric ether every day. After using this for 15 or 20 days, in order that its energy might be maintained, its use is suspended for a day or two, and then recommenced, when its application may be continued for a very long time, if not for an indefinite period. A committee—comprehending, as medical men, M. Lélut, president; MM. Bérard and Béhier—was appointed to investigate the matter. This duty they executed with much attention, when Mdle. Cléret was attacked with a severe illness. Having waited without much hope of her recovery, they have reported on it as being a subject as yet undecided. The following facts have been observed by them: Twenty-nine children have been treated in this manner with advantage. Two of these, who had been examined by the committee, were completely cured. Seven children, never previously under any

treatment, completely deaf and dumb, have all, especially four of them, after eight or nine months, manifested very remarkable improvement; noise and the sound of the voice being quite perceptible to them. The reporter of the committee has taken care to add, that the greatest caution had been taken to avoid all cause of error, and to prevent any illusion which might result from perceptions obtained through the other senses. Nor is this all. The committee, wishing to increase the opportunities to study the means used by Mdlle. Cléret, and desiring especially to examine other children besides those exclusively confided to this lady, deputed one of its members to take under his own care, patients to be treated according to Mdlle. Cléret's method. Nearly twenty persons were intrusted to him, principally deaf and dumb children, and some old people, whose hearing was impaired, or only existed on one side. In all these cases the result has been very remarkable. Convalescents from typhus fever have had their hearing restored by the same means. In conclusion, with the exception of two or three children, attested by authentic certificates, as deaf and dumb, and who hear well, the committee has established only some incomplete results of experiments begun and not finished, some manifest improvements definite. Such experiments are, no doubt, however, so far unsatisfactory; and, with every confidence in the report, we must regard its imperfect nature as a reason for suspending any opinion on the matter until we can be furnished with evidence of a more complete and perfect nature.

Case by Dr. Lafargue.—I took under my care a deaf and dumb boy, eight years of age. The treatment was commenced on the 27th of April last. Every day I dropt into each of his ears eight drops of rectified sulphuric ether. At first, pain was experienced in the right ear, and the amount introduced into it was reduced to four drops; but, since then, a tolerance of the remedy has been established. I now use eight drops for each ear. The improvement was almost instantaneous. At the end of two or three days the little patient began to say, "Papa," "Mamma," "Aunt," etc.; but, singularly, he pronounced all these words in a low tone. The articulation of the sounds will, no doubt, be an education of the hearing. The child hears the sound of a bell, the striking of a clock, etc. These unusual sounds interest and amuse him much. He hears better with the right than the left ear; and as the former is the organ in which pain was at first chiefly complained of, I have been led to ask myself if the curative action is proportional to the physiological effect. Experience alone can answer the question.—*Revue Médico-Chirurgicale.*

In an article on the same subject, contained in the August number of the *Journal de Médecine et de Chirurgie Pratiques*, we find the following remarks:—

Some doubts having been expressed regarding the innocuousness of the treatment by ether, we requested Dr. Béhier to tell us if the patients examined by the commission, of which he was convener, had had reason to complain of the treatment.

"By no means," replied M. Béhier. "On the contrary, we saw deaf and dumb children, perfect little savages before the treatment, become tractable and obedient as soon as the improvement of their deafness began to make them amenable to direction. Ether is a purely empirical remedy. When is this agent useful? when is it not? These are questions which I cannot answer. All I know is, that the only unfortunate result I have seen from its use has been its failure in certain cases. It causes a little pain; it is sometimes badly borne; in these cases it is sufficient to employ it at longer intervals. Although I do not use the remedy in my own practice, I could cite four new cases of complete deafness cured by the instillation of ether since the publication of my report."

M. Fonssagrives has not as yet published the result of his experiments; but some other practitioners, in reply to the appeal of Dr. Debout, have published a certain number of cases, which are, on the whole, favourable to the remedy of Mdlle. Cléret.

Still, we persist in the belief that ether will not realize the exaggerated expectations which have been entertained regarding it. If it only relieve deafness by dissolving the cerumen which encrusts the membrana tympani, it can only act by relieving a morbid condition hitherto unknown, and which unfortunately, to judge from certain statistics furnished us by Dr. Triquet, aurist in Paris, is not the most common cause of deafness.

Up to the 18th of July, M. Triquet had treated by ether 110 patients (rich and poor) affected, some with chronic catarrh of the middle ear, others with nervous deafness — all *deaf*, be it well understood, and subject to ringing in the ears. In all these cases, care had been taken to test the power of hearing before and after the treatment. The result has been this: The patients have been treated with 30 to 40 drops of ether every three, five, or seven days, rarely every day. There has always been immediate pain, without consecutive amelioration; far from it—the deafness and the ringing in the ears have augmented progressively with the number of instillations. In twenty cases the pain and redness of the auditory canal have been so violent as to have necessitated the employment of antiphlogistics. The patients (women) have suffered from headache, which has continued for weeks, and deprived them of sleep, although the treatment was suspended on the very day that the headache was experienced.

M. Triquet had thought, as the result of his first trials, that ether might be used without inconvenience to dissolve ceruminous concretions; but he is now forced to confess that, even in these cases, its uses may occasion accidents, such as otitis, with considerable swelling of the lining membrane of the auditory meatus.

On the whole, the instillation of ether in the case of 110 patients, made with all possible precaution, has not produced, according to Dr. Triquet, any perceptible amelioration, and in a considerable proportion has manifestly aggravated the condition of the patients. The author may, no doubt, be reproached with having unconsciously, in his character of specialist, deepened the shadows in the above picture; but, even charging to the account of individual susceptibility the accidents attributed to ether, we must still see here a list of 110 failures out of 110 patients. This result, it must be allowed, is not encouraging; and there is much reason to apprehend that M. Menière only expressed the truth in writing to M. Debout, that a belief in the efficacy of ether to cure deafness was one of those generous dreams which pass away with the morning light.—*Ed. Med. Journ.*, Sept. 1, 1860.

OPHTHALMOLOGY.

30. *Conical Cornea treated by Operation.*—Some months since Mr. Bowman published (see No. of this Journal for Ap., 1860, p. 554), an account of some cases of conical cornea in which vision was greatly benefited by converting the natural circular pupil into a slit. GEO. LAWSON, Esq. Clinical Ass. to Royal London Ophth. Hosp. relates (*Lancet*, Sept. 8th), the two following cases which were advantageously treated by the plan.

Case 1.—R. E.—aged 31, a tall, thin, and rather delicate man, of temperate habits. Has never had any serious illness, but for the last sixteen months has complained greatly of debility, lassitude, and a disinclination to do any work. He suffers as often as two or three times in the week from nocturnal emissions, and feels very low and nervous. A few weeks ago, he accidentally discovered that with the left eye he was unable to distinguish objects, or to read the largest type. On looking at the eye, the cornea is seen to be very conical, with a distinct small central softening. The apex is opaque. He can distinguish a hand held in front of him, but he is unable with the affected eye to count the fingers. He cannot distinguish the markings or the hands on a watch.

On the 30th of November, I drew the pupil downwards and inwards, fastening it in this position by Mr. Critchett's method of tying it with a fine ligature of silk.

Dec. 3d.—I performed a similar operation upwards and inwards.

His sight was greatly improved by this procedure. He was enabled to count fingers and to distinguish the features of friends. I saw him about three months ago; he could then count my fingers at a distance of ten or twelve feet, and with difficulty he could make out the time on my watch. The conicity has certainly materially diminished. The right eye was, however, becoming slightly affected, requiring him to use concave glasses.

Case 2.—W. M.—, aged forty-two, a butler; a healthy but delicate looking man, with a very anæmic appearance; married, and has two children. Twelve

years ago he says that he could see well and read any ordinary type. He has now conical corneæ of both eyes; but in neither very severe. The conicity of the left is the greatest. With the left eye he cannot count fingers; but with a powerful concave glass he can do so with difficulty. The apex of the cone in this eye is slightly opaque. His object in seeking relief was that in following his employment he could not tell when he had filled the wine-glasses, and was frequently apt to pour the wine over on to the cloth. I performed an operation on this patient, in January last, exactly similar to that in Case 1. Shortly after he had gone back to his situation, he wrote to me saying that he had derived very decided benefit from the operation. A week ago this patient called on me; and then I questioned him concerning the state of his vision. He said he thought he saw near objects better, but there was a great improvement in the way he saw those more distant. He was now able to pour out wine without spilling it; and in my presence, with his best eye (the right one) covered, he filled a wine-glass with water to the very brim without spilling a drop. He could distinguish the markings and hands on my watch. The conicity of the cornea was clearly much diminished, and the opacity had almost disappeared.

31. *Extraction of a Portion of Coal which had been in the Eye Ten Years, with Recovery of Perfect Sight.*—Mr. NUNNELEY records (*Ophthalmic Hospital Reports*, No. 11) the following very remarkable case:—

“When a hard substance enters and remains in the eyeball, in spite of whatever may be done, in the great majority of cases, sooner or later, the organ is lost, for if it be not destroyed by acute suppuration, commonly irritation is kept up after the more immediate active symptoms have diminished, and the ball becomes soft and ultimately shrinks and collapses, even though the foreign body be small and become imbedded. This wasted condition not unfrequently occurring after the lapse of some time.

“In February last, J. A., a stout middle-aged man, a collier by trade, applied to me. He stated that ten years ago, while blasting in the pit, the shot exploded too soon, scattering the fragments of coal with great violence; that by some of these the left eye was struck, some entering the ball; that for six or seven weeks he suffered great pain, was nearly blind, and was unable to work, but that gradually the pain and inflammation ceased, and the sight of the eye became as good as it was before the accident; but that ever since there have been two or three black specks to be seen in the eye. He has continued uninterruptedly at his work until five weeks before his application to me, when the eye and side of the face were violently struck by the fall of a large hard substance while at his work. The eyelids were bruised and swollen; but as he was not blasting nor using the pick, and saw the size of the piece which struck him, he is quite certain no particles entered the eye, and that it was simply a hard blow. However, since then he has suffered intense pain in and about the eye, great inflammation, and an inability to open the lids or to work.

“I found every indication of acute irritative ophthalmia; there was intense photophobia and lachrymation; both conjunctiva and sclerotic were greatly injected; the iris was dull and inactive; the cornea, particularly at its lower half, was rough and hazy, where also was a dense opaque white patch, with two or three conjunctival vessels going to it, and behind this spot could be indistinctly seen, in the anterior chamber, a dark substance about the size of a swan shot, but what it might be, whether a clot of blood, dark fibrin, or a piece of coal, in consequence of the opacity of the cornea, it was impossible to say. Imbedded in the cornea, towards its upper and outer part, were two small portions of coal, and in the sclerotic conjunctiva near to them two or three very minute particles. As the man was quite certain no coal had entered the eye for ten years, and equally so that during this period, until the receipt of the blow five weeks ago, the sight of the eye was perfect, and it was evident that the particles in the cornea were imbedded in the tissue which was clear and healthy about them, the case was treated as one of traumatic inflammation. When I saw him after a few days he was very much relieved—the vascularity was lessened, the opacity of the cornea was much diminished; and as the dark spot in the anterior chamber had disappeared, I supposed it must have been a small coagulum which

had been absorbed. I did not see him for a fortnight, when I found the eye as much inflamed as it was when I first saw him. He said that it had continued to improve until the three previous days, since when he has suffered greatly. He now said that he felt certain that the coal in the eye was the cause of all the symptoms, and begged me to remove it. None was to be seen except the particles in the cornea; and though, from the fact of the cornea about them being clear and healthy, I felt certain they were not causing any irritation, at his importunity I tried with a needle to extract them, but found, as I supposed, that they were deeply and completely imbedded in the structure, with a layer of clear, healthy, firm cornea passing over them, while the lower part of this was soft; I therefore declined to further meddle with them. Again in a few days he presented himself, saying he was certain there was a portion of coal deeper in the eye, which moved about, as, when the pain had violently returned, he had seen it by looking in the glass. I declined to accede to his urgent request that I should open the eye and search for what I could not see, but directed him at once to come to me should he again see the portion. In two days afterwards he called, bringing with him his wife; both of them declared that before leaving home they had seen the piece, though none could be then seen. They were directed to wait in town for a few hours, and should it again come into sight, immediately to return. After a few hours they did so, when evidently a portion of coal occupied the same place in the anterior chamber in which I had seen the dark substance on the man's first visit. I at once sent him to the Eye Infirmary for operation, where I also saw it; but on his being placed upon the couch it had again disappeared. I now made him get on and off the couch quickly and shake the head suddenly several times, which brought the particle into view. A section of the cornea was instantly made, when with the aqueous fluid escaped the portion of coal.

"The next day the wound in the cornea had closed, its curve was restored, the inflammation and pain were much less, and the cornea was much less hazy. On the following day he was so much better that he returned into the country. In three weeks he went to work in the pit, the eye being perfectly well, and the sight as good as ever. The small portions in the upper part of the cornea remaining quiescent, as they probably will do, they having had nothing to do with the recent symptoms.

"In this case, in all probability, the coal had been fixed in the posterior chamber, exciting while lying immovable there no mischief after the subsidence of the first irritation, but that by the concussion of the blow received five weeks before I saw him it had been detached and movable, becoming at once a cause of irritation by floating through the pupil from one chamber into another when disturbed by the motions of the body or head, and, on entering the anterior chamber and resting upon the anterior surface of the iris, a source of mischief only to be allayed by its removal from the eye. The sudden and complete restoration of the eye after so long a residence of a hard substance within its interior, and the active disease caused by it during the last ten weeks, is an interesting fact."

MIDWIFERY.

32. *Cases of Sudden Birth with Dropping of the Child.*—Dr. OLSHAUSEN related some cases of this description to the Berlin Obstetrical Society. 1. Rosalie D., aged 23, a primipara, came to the Lying-in Establishment, after having had weak pains for about twelve hours. While ascending the wooden staircase the child suddenly fell from her, striking its head against the stairs with a noise which was audible in an adjoining chamber. The infant showed all the signs of being full-timed, but it only weighed five or six pounds. The funis, which was very gelatinous, was of an average length, and was torn through at three inches from the umbilicus, and no bleeding followed. At the time of the birth a true *caput succedaneum* was observed on the posterior part of the right parietal bone, stretching to the occipital; and when this had disappeared a cephalæmatoma gradually appeared at the same part of the parietal

bone, and stretching from it, two fissures could be felt. The child, was at first very well, but afterwards became emaciated, and died when eighteen days old. At the post-mortem, at the seat of the cephalæmatoma, the periosteum was found separated from the bone, having effused blood beneath it, and a fissure, penetrating through the whole substance of the bone, was found extending $1\frac{3}{4}$ inches in length, from near the middle of the sagittal suture to the right parietal tuberosity—the middle of the fissure being the site of the cephalæmatoma. On the inner surface of the cranium an internal cephalæmatoma exactly corresponded to the external one. The cranial bones were moderately firm. There was no extravasation on the surface of the brain, but both it and its membranes were highly hyperæmic.

2. Alwine F., aged 21, having already borne a child, was seized with weak labour-pains which had only lasted two hours and a half, when, while she was on her way to the Institution, the child suddenly fell on the pavement. The funis was torn through at its middle, and at first there was considerable hemorrhage. The child, though full-timed, did not weigh much more than five pounds. A fissure was plainly felt stretching from the tuber parietale towards the sagittal suture, and having at its termination a slight depression of the bone. The fissure and depression were less perceptible as the child grew up, and at the age of six weeks could no longer be felt.

3. W. S., aged 26, having already had a child, was so suddenly delivered of a child in the Institution that it fell upon the wooden floor. The very gelatinous funis was torn through so closely to the umbilicus that it was ligatured once only with difficulty. No bleeding had occurred. The child died on the eighth day from phlebitis umbilicalis; but neither the skull nor the brain exhibited any signs of injury.

4. Ernestine M., a primipara, aged 22, had had only ten pains, of which but four were severe, when the child suddenly fell on the floor. It was a small one, and quite healthy, and exhibited no signs of injury on the head. The funis was torn at five or six inches from the umbilicus, but no bleeding ensued, although it remained ten minutes untied.

In the two cases in which fissure of the cranium existed, the fissure took, as F. Weber says it always does, the direction of the osseous fibres from the centre of ossification towards a suture. In the first case death could not be referred to the injury, as the child, living eighteen days after its occurrence, died of atrophy, without having exhibited any cerebral symptoms—the cerebral hyperæmia found after death being frequently met with in new-born infants. All the children were small, but in one of the cases the mother had a contracted pelvis. *Med. T. and Gaz.*, Sept. 1, 1860, from *Monatsschrift für Geburtskunde*, July, 1860.

33. *On the Doctrine of William Hunter in regard to Retroversion or Retroflexion of the Gravid Uterus.*—Dr. W. TAYLOR SMITH read an interesting paper on this subject before the Obstetrical Society of London (Nov. 7, 1860). After giving an account of the way in which our knowledge of this displacement of the uterus has been acquired, and the opinions of ancient and modern authors, but particularly of William Hunter, upon this subject, Dr. Tyler Smith proceeded to lay down his own views, and especially to dispute the Hunterian doctrine that the chief and exciting cause of complete retroversion is retention of urine and distension of the bladder. He then went on to say: "My own attention became specially directed to the subject of retroversion of the gravid uterus in the following manner. I attended a lady—a patient of Sir Ranald Martin—who, in the unimpregnated state, suffered from complete retroversion or retroflexion. She left this country, with the uterus retroverted, to join her husband in India. She soon became pregnant, and went the full time. The question suggested itself to me—What was the condition of the uterus in this case after impregnation occurred? And I resolved to take any opportunities which might occur to me of answering it. I have now seen a considerable number of cases in which the retroverted uterus has become impregnated, and have carefully watched the progress of gestation under these circumstances. The result has been a conviction that the most common cause of retroversion of the gravid uterus is not to be found in the state of the pelvis, or the condition of the bladder, but in the occurrence of impregnation in the retroverted uterus, and in the tendency of the

organ thus impregnated to grow and develop itself during the early months of pregnancy, in the retroverted or retroflexed position. When an ovum is deposited in the retroverted uterus, the enlargement of the organ causes a greater sense of weight and pressure in the pelvis than ordinary pregnancy. The os uteri approaches the pubis, and the fundus projects towards the hollow of the sacrum. The fundus is found to enlarge considerably when examined from time to time by the finger. At length, unless the pelvis is of very large size, the bladder and rectum are pressed upon so as to interfere with their functions, and difficult micturition and defecation, especially the former, are the results. Owing to the retention of the gravid uterus within the pelvis, there is little or no increase in the size of the abdomen. There is usually a great amount of pain and discomfort in the lower part of the back, and the sympathetic affections of pregnancy are frequently more severe than usual. Abortion very frequently occurs from the mechanical irritation of the uterus." After making some further general observations to prove the strength of his argument, Dr. Tyler Smith proceeds to say: "In conclusion, I may observe that it seems to me the great use of the knowledge of the mode in which retroversion of the gravid uterus occurs will be in the prevention of the full retroversion—or strangulation, as I have ventured to term it—of the gravid uterus in the pelvis. As long as retroversion was supposed to take place suddenly and mysteriously, little could be done to avert it; but if, as I believe, the displacement dates from the very beginning of pregnancy, in the great majority of cases, we may do much by position, and attention to the bowels and bladder, to prevent any dangerous symptoms; and, aware of the condition of the uterus beforehand, we shall be more ready to give prompt mechanical assistance when it becomes necessary to pass the hand into the vagina to carry the fundus above the brim. When retroversion has existed in early pregnancy, but has been relieved spontaneously or otherwise by the ascent of the fundus, labour takes place without any unusual difficulty. We ought, however, in the management of the puerperal state, to endeavour to prevent a return of the uterine displacement. The occurrence of pregnancy is rather favourable than otherwise to the cure of retroversion. In the latter months of pregnancy the fœtus acts as an intra-uterine pessary; the organ is strengthened, and in the return of the uterus to the size of the unimpregnated state by the process of involution we have a better chance of curing retroversion than under any other circumstances. The abdominal bandage should not be tight enough to force the uterus into the pelvis. The patient should be encouraged to lie on her right or left side, inclining to the prone position, but avoiding recumbency. The bladder should be frequently relieved, and any violent straining during defecation avoided. She should remain in bed or on a couch longer than usual, and before resuming her ordinary duties the condition of the uterus should be ascertained; and if any tendency to a return of retroversion exists, an air-pessary should be worn in the vagina as long as may be necessary to insure a right position to the uterus. Several other cases of retroversion of the unimpregnated uterus, followed by retroversion in the gravid state, have fallen under my observation, besides those related in the present paper; but as they would only be a repetition of those already detailed, I will not trouble the Society with the particulars of them. What has happened in my own practice must necessarily have occurred in that of others; and probably it is only necessary that the matter should be understood, for the production of a number of well-authenticated cases of the same kind by those engaged in obstetric practice. I must now leave it to the Society to decide whether the facts and observations which have been adduced do not prove that the Hunterian theory of gravid retroversion is no longer tenable; and whether we must not in future look upon retroversion of the unimpregnated state, which is well known to be a common affection, frequently admitting of impregnation, as the principal cause of retroversion of the gravid organ. In raising this discussion, I would yield to no one in veneration for the name of William Hunter, as being undoubtedly one of the greatest and most honoured names in obstetric science."—*Med. Times and Gaz.*, Nov. 17, 1860.

34. *New Sign of Post-partum Detachment of the Placenta.* By JOHN CLAY, Senior Professor of Midwifery in Queen's College, Birmingham.—The rules usually given in obstetric manuals and text-books for the management of the placenta

after the birth of the child are—to wait for a pain; or to carry the finger along the cord to the os uteri, and if its root can be felt, it may be fairly assumed that the placenta is thrown off, and may be easily extracted by gentle traction of the cord, with the aid of external manipulation. If these instructions be faithfully carried out, can we rely upon the facts elicited as infallible proofs that the placenta is separated from the uterus? Pain may mislead, as it frequently arises from other causes than contractions of the uterus; and even if the insertion of the cord can be felt, it is not always conclusive on this point, as the root of the cord may sometimes be felt when the uterus is in a flaccid condition, by using moderate traction on the cord, and yet the placenta be not thrown off. Besides, the patient often lustily complains of the smarting pains caused by the frequent examinations deemed necessary to ascertain the fact; and often she positively forbids such a mode of interference.

Four years ago I was led, from these causes, to investigate the subject, with the view of improving, if possible, upon the old mode of managing the deliverance of the after-birth. I thus ascertained certain facts from which I came to the conclusion that a very simple sign existed by which the separation of the placenta, after the birth of the child, might be indicated; and, having tested it in upwards of nine hundred cases, I hope I may be considered to be fairly entitled to lay the results at which I have arrived before the profession.

Before dividing the umbilical cord, I always apply two ligatures, and make both sufficiently tight to prevent the occurrence of hemorrhage. If the maternal part of the cord is now examined, it will be found to be in a flaccid condition, and almost free from blood; but if it be again examined, at an interval, say from one to three minutes, it will be found to have acquired increased specific weight, and that the vessels are more or less filled with blood. The one fact may be ascertained by poising the cord on the fingers; the other by slightly grasping the cord near the vagina, with the thumb and fore-finger of the left hand, and with the fingers of the right hand suddenly compressing it, when a well-marked sense of fluctuation is perceived underneath the fingers of the left hand—a kind of resilience similar to the feeling produced when an elastic tube filled with fluid is suddenly compressed.

When the placenta is thrown off, or sufficiently detached to give rise to a tolerably free hemorrhage, the cord loses its increased specific weight and the hydrostatic property just mentioned. These phenomena occur so invariably, that *the loss of the previously acquired hydrostatic properties of the cord after the birth of the child constitutes the sign of detachment* previously referred to.

The whole of the phenomena are manifested in three stages, viz: 1st, a state of flaccidity; 2d, a state of repletion; 3d, a state of flaccidity.

If the umbilical cord be tightly grasped by a spasmodic contraction of the os uteri, or by irregular contractions of the body of this organ, the *loss* of the particular hydrostatic properties may be delayed for a brief interval; but in a few seconds the spasm subsides, and those phenomena are produced which indicate the separation of the placenta, and that this structure may be safely extracted. These signs are not, of course, always equally marked in every case, often requiring experienced tactile management in order to detect their presence. When the uterus is in a flaccid condition, the phenomena are manifested in a very slight degree, but are still perfectly reliable. On the other hand, when the uterus is contracted, with some degree of firmness, on the placenta, they are so well marked that the most inexperienced may readily detect them. In cases of partially adherent placenta, the disappearance of the hydrostatic properties, after being once fully developed, and the failure to deliver the placenta by the usual manipulations, have always indicated to me the necessity of promptly adopting artificial detachment by the introduction of the hand. In twin cases, if the cord is firmly tied, I have invariably found that the signs persisted until the birth of the second child. In one case, where the hydrostatic properties disappeared, after being well marked, before the birth of the second child, I found, on examination, that the corresponding placenta was detached, and I at once removed it, which otherwise would probably have been suffered to remain. Neither mother nor child incurred any risk.

It sometimes occurs that the placenta is separated simultaneously with the

birth of the child. In this instance the first series of phenomena may be absent, and it may be prudent to wait before proceeding to extract the placenta, although it may be generally effected with safety.

The practical value of the application of these facts to obstetrics is obvious, as by merely compressing the cord in the manner previously indicated the precise time of separation may be easily ascertained, the placenta at once extracted, and the patients thus freed from those frequent annoying examinations usually employed. The prompt delivery of the placenta, on the first efforts of the uterus, is very important, as this organ contracts then more efficiently, and the risk of hemorrhage is not so great, and it may be fairly assumed that the puerperal convalescence is not so protracted as under a more dilatory proceeding.

To students, or inexperienced practitioners, it might be a safe instruction to impart—not to interfere in the extraction of the placenta so long as the hydrostatic properties herein defined are persistent.—*Dublin Quart. Journal of Med. Science*, Nov. 1860.

35. *Cases of Inversion of the Uterus*.—Dr. BRANDT related to the Berlin Obstetrical Society two cases of inversion of the uterus which he had met with. He was called to the first on account of a fearful hemorrhage which had immediately followed the birth of a third child. He found the woman almost pulseless in a pool of blood, various means for arresting the hemorrhage, including the plug, having been tried in vain. The plug having been removed, it was at once discovered that an inversion was the cause of the nearly fatal hemorrhage. Reposition was accomplished with ease, and the bleeding at once ceased. The woman, though still anæmic, was enabled to leave her bed on the ninth day, and the lactation was normal. The second case was a primipara, aged 30, and a hemorrhage which had followed the removal of the placenta had continued forty-eight hours, notwithstanding attempts to arrest it, when the author saw her. She was then anæmic and nearly speechless, her feeble pulse beating 140. After putting her under the influence of chloroform, reposition was accomplished without further loss of blood, the organ contracted and the patient was relieved of the pains she had complained of. Some ergot was also afterwards given, and next day the contraction of the organ was found to be quite normal. The further progress of the case, however, was unsatisfactory, as the patient fell into a typhoid state, with subsequent œdema and metastatic abscesses; and at the period of the report, two months after delivery, she was still in a precarious condition. Dr. Brandt, in regard to the production of the accident in these cases, stated that, although it was the interest of the midwives to hasten the termination of the labour, he had no proof that they had acted improperly.

Professor Martin referred to two cases which had occurred in his own practice, in one of which the inversion seemed to have been produced by the traction of the cord made by the midwife. In the other case, which was an old one, the cause was doubtful. Indeed, it was only a partially-inverted uterus, and had been supposed to be a polypus. The diagnosis was made, first, by estimating the length of the organ between the two hands placed within and outwardly, and then by the introduction of the uterine sound, which could be carried around the prolapsus part, but passed in nowhere to a greater depth than an inch. Only palliative treatment was resorted to. Dr. Mayer referred to two cases formerly related by him to the Society, in which he was certain the occurrence was not produced by any external means as he was present at the labours; and the expulsion of the placenta was left to Nature. He attributed it to the shortness of the funis. He referred likewise to a third case which had been also spontaneously produced. With regard to the diagnosis of old cases, he had found the sensibility of the part a never-failing aid. A polypus is completely insensible; but the inverted uterus, is possessed of so much sensibility that if we scratch its surface with the nail, the patient is enabled to tell us the kind of action performed. Still, the diagnosis is sometimes excessively difficult, and he remembered a practitioner of high renown having well nigh passed a ligature around the inverted fundus. Dr. Kauffmann referred to a case in which reposition was accomplished after three-quarters of an hour's effort, through an

os uteri contracted to half an inch. It was not attempted until a day and a half after delivery, and was followed by a gangrenous metritis. As the question whether, when there is no longer any hemorrhage, it is better to leave the uterus in its abnormal position or undertake its reposition, he is an advocate for always attempting the operation. An inverted uterus, independently of the sterility it gives rise to, may become the source of numerous evils which may endanger life itself. The cases of spontaneous reposition, which have been observed as late as six months after delivery, speak highly in favour of the attempt being made.

At a subsequent meeting of the Society Professor Langenbeck laid before it a preparation of a completely inverted uterus taken from a woman forty-five years of age, who having suffered from severe uterine hemorrhage during two years, came to his clinic under the idea that this depended upon uterine polypus. The diagnosis was attended with considerable difficulty, but it was at last determined after careful examination per anum that the affection really was an inverted uterus. The patient, worn out with the hemorrhage, soon afterwards died. The tumour proved to be a completely inverted uterus of nearly the normal size, having attached to its fundus a sarcomatous lobulated tumour. From the abdomen the finger could be passed through a funnel-like passage into the inverted organ. The tubes and ovaries lay in front, and were not involved within the inversion. Dr. C. Mayer observed that it was an error to suppose that this woman had never borne a child, for he had learned that she had had a child twenty-three years before: and this strengthened him in his conviction that a tumour of the fundus uteri never was the cause of inversion. It is only after delivery has occurred that the tissues are disposed to become inverted. To the objection that the woman had only suffered from hemorrhage during the last two years, he opposed a case he had met with of very old inversion in which the only inconvenience at first was excessive menstruation, the patient being otherwise quite healthy. Among the great number of cases of uterine polypi and tumours he had seen and operated upon, he had never met with an instance of inversion being produced either by the tumour, or by the operation performed for its removal. He maintained, however, that the polypus should always be operated upon *in situ*, and not first drawn down. Dr. Martin, while agreeing that in general the change of texture induced by pregnancy is a necessary preliminary, believed that there are too many recorded cases of inversion having been caused by fibroid tumours to doubt the possibility. Dr. Ebert stated that while engaged a few years ago in the compilation of a large work on inversion of the uterus, he could not find a single case recorded in which its production, in consequence of a tumour, could be regarded as indubitable. He believed, however, that it was possible an incomplete inversion occurring after delivery might easily be overlooked, and become, with the progress of time, developed into a complete one. Professor Virchow had great doubts as to the possibility of the spontaneous production of inversion. The fact of such occurrence often taking place in the intestinal canal proved nothing, as that is a soft, relaxed tissue in a state of constant peristaltic movement. *A priori*, indeed, we might well expect that hemorrhagic polypi would easily induce inversion, seeing the rapidity of their growth, the large size they attain, and the severe expulsive pains they give rise to; but in all his numerous autopsies he has found that the uterus only sinks deeper in the pelvis so as to give the polypus a point of support in the vagina and thus take off traction at the fundus. In doubtful cases he is, therefore, inclined to the opinion that the incomplete inversion produced after delivery, has increased at a later period until complete inversion has been the result. Dr. Kristeller referred to the case related by Baudelocque, in which inversion occurred in a girl eighteen years of age, whose hymen was unruptured, as a proof that the accident may arise independent of parturition. Dr. Gurlt pointed out the improbability of the affection in the present case having been so long overlooked, had it been produced as suggested as a consequence of a delivery which had taken place twenty-three years ago. By searching he has been enabled to collect eight cases of inversion combined with polypus of the uterus. In one of these cases the inversion was incomplete, and in the other seven complete; and in five of these cases the

inverted uterus, together with the polypus, was removed by ligature. In one case the polypus was removed and the uterus replaced. The conclusions he comes to are—1. That a complete inversion of the uterus in consequence of the existence of a polypus, though a very rare occurrence, may take place. 2. That so far from the distension of the uterus in consequence of pregnancy being previously necessary, four of the subjects of the cases here referred to were in the virgin state. 3. The inversion under the operation of labour-like pains may sometimes be rather quickly produced. Dr. Martin referred to a case in which *inversio uteri chronica* was combined with fibroid. A woman who three years previously had borne her last child, exhibited, after increasing hemorrhages, an inversion of the uterus with a tumour of the fundus. Dr. Jürgens, of Riga, applied a ligature, which was gradually tightened during a fortnight, when the lower part was cut away. The preparation of the part is preserved in the Dorpat Museum; and a fibroid tumour the size of an egg is observed to be attached to the fundus uteri, whence it is easily separable, the uterine substance around having too undergone hypertrophy. The division was made near the cervix, and the woman recovered.—*Med. Times and Gaz.*, Oct. 27, 1860, from *Monatsschrift für Geburtskunde*, Bd. xv. and xvi.

36. *Puerperal Epidemic at Scanzoni's Midwifery Institution at Würzburg, during February, March, and April, 1859.* By Dr. O. VON FRANQUE.—The establishment is of quite recent institution, and is placed in one of the healthiest parts of the town, being surrounded by gardens, and well exposed to the air. It is well constructed, and upon an average contains 30 pregnant women (besides a few others suffering from disease), from 350 to 360 births taking place annually. During the three months now referred to there were 99 deliveries, and the forceps were applied four times, and turning was resorted to once. Of these 99 women 30 became the subjects of puerperal fever, 9 of them dying. Besides these one of the women died of phthisis, and one from eclampsia. Of the 102 children born, 8 were born dead, and 9 died subsequently.

With respect to the epidemic itself precursory indications were met with at the end of 1858 and the beginning of 1859, for without their assuming the character of puerperal fever, mild forms of endometritis and peritonitis, especially the first, were observed. They were, however, purely local manifestations of short duration and favourable termination. These slight affections disappeared towards the end of January, true and severe puerperal fever appearing at the beginning of February. About this time, too, irregularities in the parturient process were of frequent occurrence. These consisted in deficiency of pains, and still oftener in irregular spasmodic contractions, spastic contraction and rigidity of the os uteri. In some of the fatal cases this last condition was the cause of that excessive prolongation of the labour, which, independently of other complication, is a powerful predisponent to the disease. Another condition often observed during the prevalence of these epidemics was not wanting here, viz., hemorrhage occurring speedily after labour. Almost all the women delivered during these three months had more or less considerable hemorrhage, dependent upon defective involution and contraction of the uterus. The organ remained large and soft, showing not the slightest disposition to contract. Puerperal affections exhibit themselves under two principal forms: viz., with hyperinosis of the blood, and with primary dissolution of the blood. The latter form was only observed in any considerable degree in two cases, which were very acute, both proving fatal. It is remarkable that the most acute of all the cases, in which death occurred within twenty-four hours after delivery, occurred at the beginning of April, when the epidemic had already given signs of diminishing. The cases connected with a hyperinotic condition of the blood pursued a less rapid course. The first appearances of disease were manifested on from the second to the fifth day, commencing either in the form of a localized endometritis or peritonitis, or more commonly still, of the two together. There were twenty-eight cases of this form, of which number seven proved fatal; these seven cases remarkably resembling each other in the nature and course of the diseased process set up. In one of the seven puerperal mania occurred during the height of the febrile action on the third day, and the patient died on the ninth day.

The treatment adopted consisted in local bleeding, cataplasms, mercurial frictions, warm baths, and small doses of calomel and opium. The post-mortem appearances were very similar: viz., a large relaxed uterus with its cervical portion softened, and its inner surface lined with diphtheritic or gangrenous deposit; fibro-purulent exudations in various parts of the cavity of the abdomen; an enlarged spleen; and a dark, fluid blood in all the veins, the heart, and the cerebral sinuses exhibiting, therefore, the signs of a dissolved condition of the blood, which, however, in these cases, was not primary, but the result of the continuation of the diseased process. In the milder cases, cataplasms were only employed, and phosphoric acid given with the beverages; local bleeding, and especially warm baths, being resorted to when the local pain proved excessive. The majority of the patients were dismissed from the wards after undergoing treatment for ten or twelve days. There were also cases of febrile action without any special local manifestation; and others in which, together with a moderately rapid pulse, more or less prostration, and a general feeling of malaise, there was an abnormal enlargement of the abdomen without even strong pressure made upon it giving rise to any pain. The involution of the uterus was performed with remarkable slowness; it remaining relaxed and to be felt above the pubis for a long period. All these cases terminated in recovery. One remarkable fact is, that in certain cases of labour, occurring during the height of the epidemic, in which, on account of the difficulty and prolongation of the labour or the exhaustion of the patient, the worst prognosis was delivered, no ill effects whatever resulted. In fact, during the height of a violent epidemic, certain individuals who are not predisposed to puerperal disease may go through the severest labours, involving the most difficult operations, quite unscathed; while others, the whole course of whose labour has been perfectly normal, become the victims of the severest form of the disease.

As in other epidemics not only did the pregnant and puerperal woman suffer, but the same influence was exerted upon the fetus and child. All the children who were born dead, or died soon after birth, exhibited the plainest signs of the diseased condition of the blood, of which they had become the subject while *in utero*. The blood was dark and fluid, the spleen was enlarged, and the umbilical arteries almost always contained pus.

As to the cause of the present epidemic, none other can be assigned than the prevalence of certain atmospheric influences, the intimate nature of which are unascertainable. If to this it be objected as a more probable circumstance that the disease may have resulted from the miasmatic influences generated within the walls of the institution itself, the reply is that such a conclusion can scarcely be admitted with respect to so newly built and well-contrived an establishment which has never been overcrowded with patients. Moreover, puerperal diseases prevailed at the same time not only in Würzburg, but also in its vicinity, which were not, it is of importance to observe, treated by the same practitioners who were in attendance at the Institution. To these facts may be added the greater prevalence of hemorrhages, and the greater mortality from puerperal diseases which took place at this time. An influence which has often proved very mischievous in Lying-in Hospitals during epidemics, viz., the presence of numerous male individuals, did not come into operation here. Individuality too, exerted no influence; for the feeble and the strong and healthy-looking were alike attacked; and, in fact, the fatal cases occurred among the most strong and powerful women, while the feeble suffered comparatively little.—*Med. Times & Gaz.*, July 14, 1860, from *Scanzoni's Beiträge*, bd. iv.

37. *Case of Periodical Hydruria.* By Prof. SCANZONI.—A Russian lady, 30 years of age, and of strong bodily frame, who had always menstruated regularly, and had borne six living children, was seized four weeks after her last confinement, in 1856, with a sudden and profuse discharge of limpid, uncoloured, scentless fluid, which at the end of three days spontaneously disappeared. Four weeks later a very slight menstrual discharge appeared, which only continued for a few hours, and was followed by another rush of the watery discharge, which as before lasted for three days. From this time the menstruation became very irregular, ceasing sometimes for two or three months, and only lasting when it

did come on usually for half a day, the small quantity of discharge being also remarkably pale and fluid. But immediately after the cessation of menstruation, the watery discharge began to appear with regularity, so that during two years it manifested itself nearly every four weeks. No pain or other symptoms either preceded or accompanied the discharge. The quantity of this clear fluid was always very considerable, and was estimated by the patient at from six to eight quarts (*maass*). Its discharge was almost uninterrupted, but sometimes it passed out with temporary increase. After many attempts at treatment in Moscow, she repaired to Paris, and consulted many practitioners, and among others Jobert, who, as well as his predecessors, regarding the affection as a hydrometra, besides giving various internal medicines, applied the actual cautery three times to the vaginal portion of the cervix uteri! As after a six months' treatment she found herself nowise improved, the patient consulted another practitioner at Kreuznach, and thence she came to Scanzoni at Würzburg. With the exception of an inconsiderable enlargement of the uterus, and a slight degree of anæmia, he could discover nothing abnormal in her appearance. After a while he had an opportunity of seeing her during one of the discharges; and, examining her again, he convinced himself that there was no material enlargement of the cavity of the uterus. He now became very doubtful whether the fluid was really secreted from the uterine mucous membrane, as all whom she had hitherto consulted believed it to be, and an analysis of some of the fluid by Scherer proved it to be nothing but *very aqueous urine*, since the analysis furnished unmistakable urea and uric acid. After nearly four days' duration, the discharge ceased of itself; the patient, much astonished at the conclusion he had come to, remaining six weeks longer under Scanzoni's treatment. During the whole of this time he ordered her to drink the Wildungen chalybeate water, and at the very next expected period the watery secretion did not reappear. Before her departure he recommended a long-continued use of iron. He saw her first in the summer of 1858, and in March, 1859, she wrote him word that for the last five months she had had none of the discharge, while menstruation had become more abundant, and more prolonged.

In seeking for an explanation of this extraordinary case, Professor Scanzoni believes that he is correct in regarding it as a hypercrinia of the kidneys, depending upon a hyperæmia of the urinary system, induced by the condition of the menstruation. The history of the case exhibits the casual connection between the excretion of urine and the menstruation; and it is not to venture too much in ascribing the spare and brief menstrual discharge to a diminished congestion of the genital organs, a hyperæmia of the kidneys, due to no ascertainable cause, taking place simultaneously. That this disturbance of the relations of the circulation may have been accompanied by some anomaly of innervation on the part of the urinary system, is rendered probable by the involuntary and uninterrupted flow of urine which took place, and which could only be explained by a paralysis of the neck of the bladder, however temporary this might have been. Lastly, we have to remember the anæmic condition of the patient; for although it existed in a slight degree only, it is highly probable that it influenced the circulatory disturbances of the organs in question, and, in part, the watery condition of the urine. The case, at all events, is deserving of our notice, contributing as it does to our knowledge of the influence which the menstrual process may exercise on the most various functions, and teaching us care in our diagnosis, the absence of which, in respect to this patient, must be considered as very blamable.—*Med. T. and Gaz.*, Oct. 20, 1860, from *Würzburger Medicin. Zeitschrift*, Bd. i.

38. *Intra-Uterine Smallpox*.—DR. ALEXANDER R. SIMPSON communicated to the Obstetrical Society of Edinburgh, some time since, an interesting case of this. The mother of the child was attacked with variola on the 5th of February; the eruption appeared on the 7th, began to fade on the 16th or 17th, and the patient so far recovered as to be able to be out of the house on the 23d of the month. She had fairly recovered, and the eruption had faded away, and left only its yet vascular scars behind, when, on the 25th or 26th of the month, twenty or twenty-one days from the time of the commencement of her own

attack, she began to miss the foetal movements; and this marks the date of the onset of the disease in the foetus. On the 4th of March, six or seven days after the foetus thus seemed to have become affected, its heart-sounds were still distinct and clear; but on the third day afterwards they were no longer to be detected. On the seventh or eighth day from the date of the commencement of the disease, the foetus had died. In short, after the disease had run a course of three weeks' duration in the mother, and had expended itself in her constitution, the foetus became affected by it, and died on the seventh or eighth day afterwards, although it was not expelled from the uterus till the end of another fortnight.

The body of the child presented a few pustules sparingly distributed on different parts of the body, but enough were to be seen sufficiently distinctive to show that the foetus had suffered from smallpox, and had died at the seventh or eighth day of the eruption. The death of the foetus, Dr. S. thinks, was not to be attributed even chiefly to the smallpox, but to the condition of the placenta, which had undergone fibrous degeneration.

Dr. BRUCE, at a subsequent meeting of the same society, showed a preparation of a foetus about the sixth month, which had suffered from intra-uterine smallpox. The mother had had an attack of smallpox at about the fifth month of pregnancy.

Dr. MYRTLE remarked that he had had a patient who was attacked with typhus fever in a very aggravated form when about four months pregnant. She was just recovering when she was seized with smallpox, in a form but slightly modified, although she had been vaccinated in infancy. Dr. Myrtle thought the child must necessarily have died; but it was born at the full time, plump and healthy, and without any trace of having been the subject of any intra-uterine disease. It had to be vaccinated, however, twelve times before the vaccination finally took effect.

Dr. FIGG, of Bo'ness, had had a case nearly analogous, where, however, the pregnant female succumbed to an attack of smallpox, and almost in dying gave birth to a dead child, on which no trace of the disease could be detected. Mr. Goodsir had even examined the skin microscopically, but no evidence could be found that the foetus had been affected by the smallpox.

Dr. MOIR had also had a patient attacked with smallpox in a late stage of pregnancy. She gave birth prematurely to her child, which Dr. Moir vaccinated the day it was born. The vaccina took, and the child lived, and escaped the smallpox.

Dr. BRUCE had a patient seized with smallpox when about three months pregnant. The eruption did not come out well; the patient aborted, and afterwards sank and died. Dr. Bruce had tried to vaccinate the woman, as smallpox was then prevalent, but the vaccination failed.

Dr. WILSON had seen two cases where patients near the full term of pregnancy had been attacked with smallpox, and in due time gave birth to children who showed no trace of the disease, and were easily vaccinated. Dr. Wilson had also seen a child born with marks of intra-uterine smallpox on its face and other parts of the body. The mother had passed through an attack of the disease shortly before. Vaccination of the child was attempted, but it failed.—*Edinb. Med. Journ.*, Nov. 1860.

HYGIENE.

39. *Influence of Tobacco-Smoking on Public Health.*—Sir CHARLES HASTINGS read a paper on this subject before the National Association for the Promotion of Social Science. He commenced by observing that it might be considered one of the functions of the Association to point out the evil effect on public health of certain active agents in daily use by the community. The one to which he would especially draw attention was tobacco. Tobacco is at present extensively consumed by all classes; it is a very active narcotic agent; its empyreumatic oil acts most deleteriously on the nervous system; and when it is concentrated, death results from it. The author then quoted the account of the chemical nature of tobacco, as given by Professor Johnstone, of Durham, "that the chemical

constituents of tobacco are three in number: a volatile oil, a volatile alkali, and an empyreumatic oil. The volatile oil has the odour of tobacco, and possesses a bitter taste. On the mouth and throat it produces a sensation similar to that caused by tobacco smoke. When applied to the nose it occasions sneezing, and when taken internally it gives rise to giddiness, nausea, and an inclination to vomit. The volatile alkali has the odour of tobacco, an acrid, burning, long-continuing, tobacco taste, and possesses narcotic and very poisonous qualities. In this latter respect it is scarcely inferior to prussic acid, a single drop being sufficient to kill a dog. Its vapour is so irritating, that it is difficult to breathe in a room in which a single drop has been evaporated. A hundred pounds of the dry tobacco-leaf yield about seven pounds of nicotine. In smoking a hundred grains of tobacco, therefore, say a quarter of an ounce, there may be drawn into the mouth two grains or more of one of the most subtle of all known poisons. The empyreumatic oil is acrid and disagreeable to the taste, narcotic and poisonous. One drop applied to the tongue of a cat brought on convulsions, and in two minutes occasioned death. The Hottentots are said to kill snakes by putting a drop of it on their tongues. Under its influence, the reptiles die as instantaneously as if killed by an electric shock. It appears to act nearly in the same way as prussic acid. Experience proves that a large proportion of those who smoke or chew tobacco, do so under the conviction that it is always innocuous in its effects, and often beneficial. Now, this is a mistake which the every-day observation of medical practitioners can attest. For amongst the patients who consult us for various nervous and stomach complaints, it will be found that tobacco smokers form a large proportion. Indeed, we find, unexpectedly sometimes, on inquiry, that the habit of smoking is the source of very distressing ailments, which immediately or gradually subside on omitting the use of this drug. It is grievous to observe that this habit is prevailing among young people, upon whom its effects are most likely to be prejudicial. Strikingly illustrative of this position is the fact, which has been very recently made public, that in the competitive examinations to which young persons are submitted in the military schools in France, the smokers of tobacco occupy the lowest place."

One of the most severe cases of epilepsy which Sir Charles had ever seen was in a boy of twelve years of age, who had been for two years a tobacco-smoker; he recovered only on being prevented from continuing the habit. It could, no doubt, be said, and it was true, that thousands pursue this practice without producing epilepsy; but many of these suffer from nervous and digestive disorders.

It was, then, important that the Association should disseminate sound views on the action of tobacco, and should show that this drug cannot be used indiscriminately. An admonition from such a body would come with more force than from the medical profession, whose monitions could only find way among the sick and those needing medical care, while the opinion of the Association would permeate the community at large. Sir Charles Hastings then quoted from the opinion of Sir B. Brodie on the effects of tobacco on the nervous system.

The various institutions now formed and supported for the purpose of diffusing useful knowledge among the labouring classes, ought to be available to assist in this work, if their managers could be made awake to the importance of the question; but, in many instances, these societies are not aware of the baneful action of tobacco on the frame. If they were, smoking-rooms would not form a part of the establishment, whereby the onward progress of civilization is proposed to be insured. It is a sad reflection that it should be considered necessary to insure the attendance of members at a society whose professed object is to advocate civilization by diffusing art and science, that there should be the means supplied for indulging in the evil habit of smoking, as in the clubs of the aristocracy. This Association may at any rate raise a warning voice against such erroneous proceedings, which must doubtless tend to enervate our population, and eventually to produce a degenerate race.

Sir Charles Hastings ended his paper with the following conclusions:—

That the effects of tobacco-smoking are chiefly dependent upon an empyreumatic volatile oil, and other active principles, whose direct tendency is to act injuriously on the nervous system and digestive organs. That tobacco is extensively

consumed by the community, and its use ought to be discouraged. That this Association emphatically records its conviction that societies formed for the purpose of promoting useful knowledge amongst the working classes, should on no account provide smoking-rooms for the members.—*British Med. Jour.*, Oct. 6, 1860.

MEDICAL JURISPRUDENCE AND TOXICOLOGY.

40. *Poisoning by Atropia—Indications of the Alkaloid in the Urine.*—Dr. Roux (de Brignolles) was called on the 21st April, 1860, at 9.30 A.M., to a lady thirty years of age, who, in a fit of despair, had designedly swallowed a solution of atropia, which had been prescribed by a distinguished oculist of Paris, for the purpose of preventing the formation of adhesions of the iris. The formula of the prescription was as follows: Atropia, three grains; distilled water, ℥iv; alcohol, ℥j; dilute acetic acid, one drop. One part of this solution had been employed as directed, and Dr. Roux calculated that at least three-fifths remained, representing nearly two grains of atropia. The patient was in bed. The face and conjunctivæ much injected, and the pupils so dilated that the iris was reduced to the size of a grayish thread. Sight was not completely lost, but objects were confused, and of a red colour, and the patient complained of being enveloped in a dense mist. She had nausea; and, by the aid of large quantities of tepid water, abundant vomiting was produced. Giddiness and tendency to sleep, scarcely controlled by fear of death and regret at her conduct; coldness of the extremities, which were also cramped; pulse 150, small and depressed. The treatment consisted in the giving of small cups of strong black coffee every quarter of an hour; the application of cooling lotions to the forehead, and of sinapisms to the extremities. The first doses of coffee were rejected by vomiting, and at eleven o'clock a quantity, of the ordinary strength, was given by the lower bowel. Shortly afterwards the nausea ceased, the patient became a little calmer, and the stomach retained some mouthfuls of coffee. At one o'clock, however, the delirium and agitation were extreme. She wished to rise, was evidently unconscious, and the mouth could only with difficulty be opened; but deglutition appeared easy: there was tenesmus; frequent desire to urinate; and the urine, which was carefully collected, was clear and transparent. Dr. Roux, recollecting that M. Bouchardat had recommended a solution of iodine in iodide of potassium as an antidote to belladonna, and as a precipitant of other alkaloids, determined to prescribe it alternately with the coffee. Unfortunately the lady was pregnant, and iodine is much feared by some as an abortive; but Dr. Roux, nevertheless, prescribed the antidote, and gave, according to the formula, a cupful every half hour: iodide of potassium, fifteen and a half grains; iodine, two grains; distilled water, seventeen fluidounces. The treatment was attended with the best effects; and at seven o'clock the use of the antidote was suspended, but the coffee was still given.

Next morning, 22d April, the patient rose. She was pale and sunk. She had only slept one hour, and since 4 A.M. had had two loose stools. The pupil was still much dilated, and vision was much confused, objects still being indistinct, and of a slightly reddish tinge; muscular debility considerable; pulse irregular, small, and still frequent.

The patient stated that the solution of atropia had a very acrid taste, and that, a quarter of an hour after having swallowed it, she was affected with nausea and ocular flashes. She lost all feeling in her legs and arms, and then she called for assistance.

Dr. Roux was anxious to test the urine for atropia, and for this purpose availed himself of the test proposed by M. Bouchardat, and by which that chemist had determined the presence of various alkaloids, such as quinia, morphia, solania, daturia, atropia, in the urine of animals fed on the poisonous solanaceæ. Drop by drop the following mixture was allowed to mix with the urine, viz: iodine, fifteen and a half grains; iodide of potassium, thirty-one grains; distilled water, two ounces. At first the urine became of a deepish green tint;

and then, as an excess of the test, was added, a fine powder of a greenish yellow colour, and in very small quantity appeared at the bottom of the fluid. Two hours had elapsed since the urine had been collected, and Dr. Roux fearing that possibly from the heat a little of the urea might have been transformed into carbonate of ammonia, a drop of muriatic acid was added, and a quarter of an hour after, there was a flocculent reddish yellow precipitate. Dr. Roux finally mixed with some normal urine a solution containing one-fifth of a grain of atropia; when to this the iodine solution was added, the liquid became clouded, and of a greenish colour, and after a time a greenish yellow precipitate fell, as in the first experiment, but much more copious. These experiments led Dr. Roux to conclude that this test of M. Bouchardat's is of such delicacy, that it may detect smaller quantities than even one-fifth of a grain of atropia in the urine.—*Ed. Med. Journ.*, Oct., 1860, from *Gazette des Hôpitaux*, 31st May, 1860.

41. *Tannin as an Antidote to Strychnia*.—As the result of many experiments performed on rabbits and dogs, Dr. Kurzak comes to the conclusion that tannin promptly administered is the best antidote in poisoning by strychnia. From twenty to twenty-five times the quantity of tannin is necessary; but even a larger amount should be administered, as the contents of the stomach, and especially gelatine, may absorb a portion. Tannin is the more eligible a remedy, inasmuch as it is easily procurable in the shape of gall-nuts. A portion may be rapidly reduced to powder and administered in water, while an infusion or decoction is prepared. For every grain of strychnia at least two and a half drachms of the gall-apples should be given. It will, indeed, be most prudent to administer a still larger quantity, especially when vomiting occurs. The experiments made by the author with *green tea* show that this also possesses a certain amount of efficacy; but, as it requires to be administered in such large doses, it becomes itself almost a poison. It can, therefore, only be of use when a very small quantity of strychnia has been taken, or as a mere adjuvatory. *Coffee* exerts still less effect. *Oak-bark*, containing 8.5 per cent. of tannic acid, may be advantageously used when the oak-apples are not accessible; and various other substances containing tannin, as acorns, horsechestnut-bark, green walnut-shell, &c. Vegetable acids must be avoided during the treatment of strychnia poisoning by tannin, as they favour the solution of the resulting precipitate. The same caution applies to alcoholic drinks. As the experiments have shown that active efforts increase or even induce the convulsions in strychnia poisoning, every care in treating the accident must be taken to avoid all such movements or any powerful stimulation.—*Med. T. and Gazette*, Sept. 29, from *Zeitschrift der Aerzte zu Wien*, No. xi.

42. *On the Danger of Green Paint in Artificial Leaves and Flowers*.—Dr. ARTHUR HILL HASSALL calls attention (*Lancet*, Dec. 1, 1860) to the employment of arsenite of copper (Scheele's green) in the manufacture of the green leaves and flowers so generally worn during the past year or two on bonnets, headdresses, wreaths, and other similar ornaments.

"The use," he says, "of emerald green for the manufacture of artificial leaves and flowers has been for the most part resorted to only within the last two or three years, and I am informed that there are from two to three hundred leaf-makers, all using this poisonous substance. The condition of these people, mostly young women and girls, but including some men and boys, is wretched in the extreme. They all labour, and many in a severe form, under symptoms of arsenical poisoning. The poison is diffused throughout the atmosphere they breathe, and is of course inhaled; further, it acts as a local irritant and escharotic on the hands and other parts of the body to which it becomes mechanically applied. The symptoms may be arranged into the constitutional and local; the former consist of general derangement of the health, debility, nervousness, headache, thirst, sickness, loss of appetite, and diarrhoea; the latter in soreness of the throat and gums, œdema about the eyes, soreness of and running from the nose, and in ugly-looking sores on the hands, face, neck, and other parts of the body, to which the poison finds access.

"Now, for all these evil effects no necessity whatever exists. In the first

place, it is more than probable that a suitable substitute for the arsenical pigment might be found; and in the second, even if its use could not be dispensed with, it is certain that the more injurious and distressing consequences might be avoided by the adoption of certain simple and obvious precautions.

"Hitherto reference has been made only to the effects of the employment of the pigment on those engaged in the manufacture of the leaves and flowers. It is highly probable, however, that the wearers of these ornaments do not altogether escape from the serious consequences to which their poorer sisters are subject."

He relates the cases of two men engaged in the manufacture of these flowers, who suffered from the poisonous effects of the pigment, and states that eight young women and two boys, working in the same room with these men, were all sufferers. "They all had sores; the girls, at the back of the neck, on the sides of the nose, and on the hands; their eyes were also affected, and there was running from the nose. To such an extent was their health affected, that they were compelled from time to time to give up their work and return home."

Dr. H. further states that "arsenite of copper is employed for a great variety of purposes besides as a dye for colouring paper-hangings. In a letter inserted in the *Lancet* some months since, I pointed out that it was constantly used in the colouring of articles of furniture and dress. I also showed that another green pigment, probably scarcely less injurious than the arsenical preparation, was still more constantly employed to colour carpets, table-covers, curtains, clothes, dresses, and numerous other articles. This pigment is known as false Brunswick green, of which three shades are prepared, and it consists of Prussian blue or indigo with CHROMATE OF LEAD."

43. *Medical Report of a Case of Poisoning by Arsenical Paper-Hangings.* By J. B. METCALFE, M. D.—Clarence W. King, son of W. T. King, Esq., of Beresford Lodge, Highbury, aged three years and a half, first manifested symptoms of being unwell on the morning of Thursday, November 1. He refused his breakfast, complained of chilliness, and was sick; at 10 A. M. he was seized with convulsions, for which he was put into a warm bath, and the attack soon passed off. On my arrival at 11 A. M. he was in a semi-comatose state, his countenance placid, surface of body warm; the bowels had acted several times, the stools passing off involuntarily, watery, bilious, and very offensive; pulse quick; tongue white; there was no abdominal tenderness on pressure. On visiting him again in the evening, I learned that slight convulsive twitchings of the facial muscles had been observed; that he had been more feverish, but was again less so; he had not been sick; the bowels were less freely relieved.

Thus far there were no symptoms of so grave a character as to lead to the supposition that they were attributable to any extraordinary cause; worms (to which the child was subject), or any other irritant, might have produced them. It was on the following day, at 7 A. M., when having been sent for on account of another child having been seized with convulsions, accompanied by violent shrieks, and severe dysenteric discharge from the bowels, that I noticed the great change which had taken place in the little fellow; he was almost in a state of asphyxia, the surface cold, pulse feeble, countenance livid. The application of warmth, administration of ammonia, and frequent supplies of warm milk, after the lapse of a little time produced somewhat of reaction; but in the afternoon he was again seized with a severe tetanic convulsion, and from that time alternations of repose and convulsion (amounting sometimes to perfect opisthotonos) continued until the little sufferer sank at 12 o'clock, thirty-eight hours after the commencement of the attack.

It is worthy of note that although all the symptoms were due to arsenical poisoning, as will appear in the sequel, there was no violent sickness; only once at the commencement of the attack was the child sick, and on one occasion on the second day when beef-tea was given instead of the milk.

A *post-mortem* examination was made on the following day, with reference to which it may be briefly remarked that the appearances were indicative of a healthy state of system; the child was fat and well nourished. For the appearances presented in the stomach and those parts which were forwarded to Dr. Letheby, the reader is referred to the report which is appended.

Chemical Report. By H. LETHEBY, M.B.—On Friday, November 2, I received from Dr. Metcalfe, of Clapton Square, Hackney, a sample of green flock-paper, two napkins soiled with the dejections from a boy aged three years, and a bottle containing an evacuation of a girl aged two years; and on the Sunday following I received a jar containing a child's stomach, unopened, a piece of liver, and a piece of large intestine; also a bottle of food.

1. The *flock-paper* was of a dull pea-green tint. It had no gloss upon it, and the size which held the pigment to the paper had been so far decomposed by moisture and air as to permit the colour to be brushed off by the slightest friction. The flock patterns were of a deeper green colour, and were in some places barely attached to the paper, on account of the destruction of the size. A piece of paper measuring 6 inches square (36 square inches) weighed 41.47 grains; and furnished, as the mean of three experiments, 12.99 grains of an arsenical green pigment, and 5.80 grains of carbonate of lime. The arsenical pigment was found to consist of 8.44 grains of arsenious acid, and 4.55 of oxide of copper; it was therefore nearly pure arsenite of copper, or Scheele's green.

2. The *dejections in the napkin* from the boy were deeply tinged with bile, and became of a deep blue-green colour with hydrochloric acid, but they did not furnish a trace of arsenic or copper.

3. The *evacuation from the girl* amounted to 2.5 fluidounces. It had a deep yellow colour from the presence of bile, but it did not furnish any evidence of poison, although, from the discoloration of the copper, I at first thought that arsenic was present.

4. The *stomach* was unopened, and its contents were secured by ligatures at both orifices. Externally it did not present any remarkable discoloration or sign of irritation; but on the inner surface, at the œsophageal end, there were small patches of a red colour, made up of the peculiar petechial spots which are so characteristic of arsenical poisoning. Diligent search was made for particles of arsenite of copper, but none were found. The contents of the stomach measured five fluidrachms. They were somewhat thick, like gruel, and had a dark chocolate-brown colour. On analysis, they gave a minute trace of copper, and very distinct evidences of arsenic.

5. The *piece of liver* weighed 945 grains. It did not present any abnormal appearance; but, on analysis, it yielded traces of both arsenic and copper.

6. The *piece of intestine* was somewhat redder than natural, and showed marks of irritation in the mucous coat, but it did not furnish a trace of poison.

7. The *food in the bottle* was perfectly free from poison.

The results of these investigations leave no doubt in my mind as to the cause of the child's death. The poisonous pigment existing in so large a quantity on the paper, together with the circumstance that the slightest friction would remove it, accounts fully for the origin of the poison; and the presence of it in the dead body answers the question as to the cause of death.

It may not be out of place to remark that the quantity of the poison on the paper is sufficient, under circumstances like the present, to be a source of serious danger: for a piece of the paper six inches square contains enough arsenic to destroy two adult persons; and, from the many cases of a like character which have been referred to me, I cannot hesitate to believe that the use of such papers is extremely hazardous. Within the last fortnight I have had occasion to examine two specimens of green papers which were suspected to be the cause of illness, and in each of them arsenic was present to a large extent—in one case it amounted to nearly ten grains on a surface six inches square; and it almost invariably happens that the poisonous pigment is but loosely attached to the paper, and is easily brushed off by slight friction. When our artificers and manufacturers will learn caution in respect of the use of such poisonous pigments I know not, for already the danger of it has been sufficiently discussed. Nor is there the excuse that, in order to meet the demand for bright and permanent colours, there is any necessity for the employment of mineral pigments. The French, who are our competitors in this matter, have long since abandoned the use of such pigments, and are yet able to outstrip us in the brilliancy of tint. It is high time that our manufacturers should imitate their example.—*Lancet*, Dec. 1, 1860.

AMERICAN INTELLIGENCE.

ORIGINAL COMMUNICATIONS.

Strangulated Inguinal Hernia, successfully treated by Large Doses of Opium. By O. M. DOYLE, M. D., of Walton's Ford, Pickens, S. C.

On the 24th of July, 1860, I received an urgent call to see a negro man belonging to a gentleman near by, on the "Georgia side." On my arrival I found the patient very prostrate; pulse frequent, skin clammy, and covered with cold perspiration. I was told that he had been engaged as a labourer on a railroad, *thirty-seven miles* from home, and that he had had a "swelling in the groin" for eight or ten days. Three days previously to my seeing him he had become unable to work, and his employers had sent him home. He had been two days making the journey, being compelled to stop frequently from sick stomach. He had vomited blood several times, and had not evacuated his bowels for three or four days. Examination showed him to be suffering from strangulated hernia.

Taxis was resorted to without success, and then the means usually recommended, as nauseants, warm baths, venesection, &c., were tried with the same result. I then had the patient placed on his hands and knees, and employed taxis, but with no better effect.

I then had him supported in a vertical position, with his feet upward, in order to have the advantage of gravitation, and once more employed taxis, but again I was doomed to disappointment. He was now sinking rapidly, having had stercoraceous vomiting several times.

It now seemed to me that an operation afforded the only chance of relief, and I asked for a consultation. Accordingly, Dr. T. S. Miller, of Retreat, S. C., was sent for. While awaiting his arrival, I gave the patient opium with a double object. 1st. To support and keep him quiet. 2d. I had seen a case reported in which it had induced sufficient relaxation to admit the hernia to be reduced. I administered two grain doses of pulv. opii every two hours, until three portions were given. He was then becoming stupid, and was allowed to remain perfectly quiet, after flexing the thighs, and having him warmly covered with blankets. He remained in this condition about an hour and twenty minutes. His respiration was then becoming very slow, and almost stertorous. He now made a sudden start to move, and I heard, very distinctly, a sharp slapping sound. I then removed the covering, and examined for the tumour—it was gone!

A quantity of strong decoction of coffee—a moderately antiphlogistic course of treatment for a few days—and the proper adjustment of a truss, constituted the after treatment.

I shall use opium in the next case.

On Pelvic Measurements. By W. H. BYFORD, M. D., Prof. of Obstetrics in the Med. Dep. of Lind University.—The subject of pelvic measurement is of so much importance, obstetrically, that various modes and instruments have been devised and invented for its correct accomplishment at different

times; yet there is no one that gives exact results for all parts of the cavity and straits, and probably all combined, as they are now used, will only enable us to arrive at proximate instead of absolute certainty in this respect. I have examined the writings of all the authors of books and articles in the periodicals within my reach, and the result is as I have stated.

Under these circumstances, I hope it will not be regarded as presumption for me to give a plan which I have taught for the last three years, and demonstrated to the medical classes who have honoured me with their attention. These demonstrations have been upon the cadaver entirely, and I have only tried it once upon the parturient patient; but I believe the common sense of the plan will commend itself to the profession sufficiently to insure a trial. The instrument I use is Baudelocque's calipers. The instrument should be constructed with flexible and movable shanks, so that the convexity of their axis can be altered, or that they may be entirely straightened, or one flexed more than the other. The scale for measurement between the shanks cannot be relied upon, in the altered flexures of them, for anything but a rule by which we may readjust the instrument after withdrawing, so that we may measure the distance between the balls upon the extremities, and thus find how far they were separated when in the pelvis. Bearing this in mind, when we wish to measure the antero-posterior diameter we place the balled extremity of one of the shanks on the upper part of the inner surface of the symphysis pubis, and the other opposite to it upon the external surface, observe the position of the shanks upon the scale, remove the instrument, replace the shanks upon the scale as they were while applied, and then measure the distance between the points. This is the thickness of the pubis and soft parts covering it, inside and out. Now place the point of one of the shanks upon the promontory of the sacrum, and the other upon the spinous process of the last lumbar vertebra, observe the marks upon the scale, withdraw the instrument, readjust the shanks upon the scale, and measure the distance between the points, when you will have the thickness of the pelvic wall of the superior strait. Now place the point of one shank on the front of the pubis at its upper edge, and the other on the spinous process of the last lumbar vertebra, and this will give us the thickness of both anterior and posterior walls, and the plain of the superior strait; by subtracting the two former from this whole measurement, we get the exact measurement of the strait itself. Now by introducing one shank into the vagina, and placing it upon the middle of the linea ilio-pectinea, and placing the other upon the great trochanter outside opposite, we have the thickness of the side wall of the superior strait. Place one point on each great trochanter, and we have the aggregate measurement from one side to the other. By deducting the thickness of the walls, thus ascertained, from the whole sum, we have the exact length of the bilateral line of the superior strait.

It seems to me that there is nothing more simple and exact than the above mode of arriving at correct measurements of the superior strait. The same *mode* of proceeding will give as satisfactory results if applied to the cavity or inferior strait. I hope that members of the profession, who have charge of institutions where they can test the validity of the above mode of measuring the pelvis, will give it a trial, and publish their experiments and conclusions.

Additional Instances of Anomalous Conformation of the Human Lung.
By JOHN H. PACKARD, M. D., of Philadelphia.—In the number of this journal for April, 1860, I reported a case of complete division between the

lobes of the left lung, the usual fissure running clear through to the root of the organ.

Since that time I have twice observed the same condition; once in a man between thirty and forty years of age, who had died of chronic dysentery, and again in a boy seven years old, who had succumbed in the desquamative stage of scarlet fever. In the former case it was impossible to ascertain the state of things on the right side, from the existence of tuberculous infiltration of the lung, with universal pleuritic adhesions; but in the latter the upper lobe of the right lung was entirely separate from its lower portion.

As it can hardly be supposed that such a deviation from the usually described arrangement of the pulmonary tissue would have escaped the notice of demonstrators and writers on anatomy, this seems to me merely one of those curious successions which are so often met with in matters of the kind.

DOMESTIC SUMMARY.

Morphia Salts.—Dr. EDWARD R. SQUIBB has given (*Am. Med. Times*, Nov. 3, 1860) some interesting remarks on these preparations. He states that he “has observed, within the past ten years, a gradual augmentation in the doses of these salts, or more particularly of the sulphate, which is, to say the least, very remarkable. The standard works on materia medica and therapeutics state the dose at one-eighth to one-fourth of a grain; and they consider one-sixth to one-fifth of a grain as equivalent to one grain of good opium. But opium itself has undoubtedly diminished in sedative value of late years, thus keeping pace with the general disposition in trade to dilution and adulteration; so that a grain of powdered opium, equal to one and one-fifth grains of the moist drug, is now not more effective, probably, than one grain of the moist drug was formerly. As good Smyrna opium yields never less than nine per cent. of morphia salts, it follows that ten grains of the opium contain nearly one grain of morphia salt, and hence that one-tenth of a grain of morphia salt would be the quantity present in one grain of good opium. But the morphia is not the only narcotic principle of opium; and, therefore, admitting that the other narcotic principles together equal the morphia in sedative effect—an admission which is certainly beyond probability—we have one-fifth of a grain of morphia salts as representing one grain of opium. Thus the books, if brought up to the present standard of opium, would probably give one grain of *powdered* opium instead of one grain of opium, as the average anodyne or sedative dose, and one-fifth of a grain of salts of morphia as the therapeutic equivalent. On looking over the treatment of various acute diseases, where no tolerance or habit exists, it is now rare to find any practitioner giving salt of morphia in any such doses. Three prominent papers are now called to mind as having been published within a few months past, wherein the sulphate of morphia was used in acute diseases, namely, sporadic puerperal peritonitis, articular rheumatism, and pneumonia—all by distinguished therapeutists. In the treatment of these diseases it is not uncommon to give one grain of the sulphate at first, to be followed by half a grain every four or six hours, and continued for seven, ten, or even sixteen days. (See Prof. Austin Flint on Pneumonia and Pericarditis, in *N. O. Med. News and Hosp. Gazette*, for Sept., 1860.) Now, the smallest probable equivalent to this in the best moist opium would be six grains to start on, and then three grains every four or six hours. Other therapeutists exceed this; and one, Dr. A. L. Hudson, of Rush Medical College, thinks that one grain of ‘morphia’ (alluding, probably, to the ordinary sulphate) is about equal to two grains of opium. These examples may not represent the common practice with salts of morphia with thorough fairness, yet they are not, with the exception of the last, selected from extreme or heroic practice; and the writer’s observation of the practice of good sound authorities leads directly to

the inference that there must be some cause for this practice to be looked for in the commercial salts of morphia of the present day.

"As it is easily demonstrable that opium has not increased in sedative power, but has probably decreased within the time mentioned, it is fair to infer that it is the salts of morphia that are at fault, and that the deficiency is due to bad pharmacy. When one-sixth to one-fourth of a grain of salts of morphia was found to be equivalent to a grain of opium, the salts of morphia were brown and granular, and presented a very different appearance from the beautiful white crystalline preparations of the present day; and it is a matter of legitimate and important research to determine how far the screwing and torturing processes of modern pharmacy, whereby to get the largest yield and greatest beauty of product, are chargeable with a depreciation of real value in obtaining a fictitious one. Both the druggists and pharmacutists would now-a-days instantly reject morphia salts that were of a brown colour, and it is probable that most physicians would also object to such as being impure. It is, nevertheless, a fact that when morphia salts are extracted from opium, in the most simple way, and that which interferes least with their normal condition, they are of a brown colour, and that the after processes by which they are rendered white are well calculated to change the character and even the constitution of the delicate alkaloid. Pasteur and others have recently shown conclusively that heat and other apparently feeble chemical agencies are quite capable of converting alkaloids into isomeric substances of similar appearance but very different reaction. Thus strychnia can be easily converted into a substance of precisely the same ultimate composition, which, although crystalline and soluble, is absolutely inert in its effect upon animals. It is known also that morphia, quinia, and veratria, are all subject to metamorphosis by heat, either without, or with but slight change of composition, and that the new products differ in such points of chemical behaviour as to lead directly to the inference that they would no longer be therapeutically the same. Hence it is altogether probable, yet certainly not yet proved, that the salts of morphia of commerce are partially changed in the processes of extraction and bleaching, whereby not only a fictitious market value is given to them, but whereby, also, their therapeutic power is much decreased. When opium or cinchona bark solutions are first properly depurated, and the alkaloids thrown down from them, and these alkaloids then converted into salts without bleaching, the salts are of a brown or dirty white colour. The amount of colouring matter and all other impurities, however, in such preparations does not amount to more than one or two per cent., and may therefore be totally disregarded in a medical point of view, since but the hundredth part of each grain would be inert. On the other hand, it is not probable that any process of bleaching could be adopted which would not introduce more than twice as much inert matters, or render twice that proportion of the original alkaloid ineffective as a medicinal agent.

"There is a great deal of this fictitious value sought and obtained for medicinal substances and preparations which is neither legitimate nor harmless, while it subserves the very bad purpose of often misleading those who can and do judge only by appearances and sensible properties, not only to the acceptance of inferior medicines, but to the condemnation of such as might be better worthy of confidence, wherein the labour and skill might have been bestowed rather upon the material and composition than upon the appearance."

Quinine, a Prophylactic of Intermittent and Remittent Fevers.—Our esteemed friend, Dr. H. W. DESAUSSEURE has recorded (*Charleston Medical Journ. and Review*, July, 1860) some very interesting observations on the power of quinia to protect the constitution against attacks of fever in those habitually exposed to malarious influences. A case, he states, which came to his knowledge years ago, first drew his attention to this subject. "Subsequent facts," he remarks, "obtained from experiments on the African coast, have confirmed the experience derived from this individual case; and I think that I have been able to collect a sufficient number of data to render the opinion plausible, if not conclusive, that quinine possesses the power of protecting the white man from attacks of intermittent and remittent fever, or its collaterals, when exposed for

even long periods to malarious influences; and, moreover, that its daily use is in nowise injurious to health, nor does its habitual use render the system insusceptible of its remedial powers."

Dr. DeSaussure relates, among other cases, the following in support of his views:—

"An overseer agreed to take charge of several rice plantations in one of the sickliest regions of rice culture, undertaking to spend the summer months on one of the plantations. He made no inquiry as to the health of the one chosen as his residence; it was selected from its convenient locality. When warned of the danger of his residing there in summer, he said he would never have the fever. His own confidence in his capacity to resist malarious disease seemed unlimited. The result fully justified this confidence. He lived ten years or more in that neighbourhood, spending every summer on the plantation, varied only by an occasional visit to the healthy pine land, where his family resided during the summer. He visited his rice fields without hesitation at any hour, day or night, that his business required. He never had an attack of fever during that time. I saw him after he had been there several years; a finer specimen of robust health would have been difficult to find.

"It was ascertained, on inquiry, that it was his habit to take quinine daily, during the summer, before leaving his house; the quantity he did not know, for he never weighed it. He died finally of apoplexy, which any one who saw him would have predicted as the probable cause of his death. His entire and complete confidence in his ability to resist fever in so malarious a region is strong evidence that he had been in the habit of using quinine, and was well satisfied of its prophylactic virtues. This case, in conjunction with the statements from the officers of the Niger expedition, would appear to prove that quinine may be used under exposure to malarious influences for an indefinite period, not only without compromising the general health of the individual or injuring the constitution, but as surely protecting the system from the inroads of malarious disease."

Dr. D. further states that he was "called in August to see one of the contractors on the Charleston and Savannah Railroad, labouring under a very severe attack of remittent fever, contracted during superintendence of his contract between the Ashepoo and Combahee Rivers, notoriously a very unhealthy region. During his convalescence he informed me that he would have to return to his work on the road, where he had a large number of hands employed (150); that they were negroes brought from healthy regions in North Carolina, and he expected all of them to be more or less sick, as they were entirely unaccustomed to a malarious climate. I advised him to take quinine daily himself, and to give it to all his hands, white or black. Late in the fall I met him in the city; he looked healthy and well. He thanked me for the advice I had given him; told me he had carried up some pounds of quinine; had used it himself daily, and compelled all his employees to take it also; that he himself had never had another attack of fever; that his health was better than it had ever been, and that not a single one of the 150 hands he employed had been attacked by fever. In fact, he said: 'The only case of sickness I have had was in a negro who came from North Carolina sick.'"

Persulphate of Iron in Post-Partum Hemorrhage.—Dr. GEO. MENDENHALL relates (*Cincinnati Lancet and Observer*, Nov., 1860) a case of post-partum hemorrhage, in which frictions over the fundus and body of the uterus, the introduction of the hand into that organ, ice, and ergot, all successively employed, failed to bring on contractions of the uterus and arrest the discharge. About three ounces of a saturated solution of persulphate of iron were then injected into the uterus through a catheter, the hand being retained for a few minutes in the womb so as to facilitate the application of the solution over the inner surface of the organ.

"It produced," Dr. M. states, "no pain, and increased the contractions of the uterus but very slightly. The blood in the uterus and vagina were coagulated in a manner that can only be produced by this preparation of iron. The hand was withdrawn, and watch kept over the condition of the uterus and the dis-

charges from the vagina. From that moment not another drop of fresh blood was discharged from the uterus and vagina. The patient was bandaged, reaction came on, and she recovered without an unpleasant symptom.

"For the next forty-eight hours the discharge consisted entirely of the disintegrated blood, which had no doubt been in the vagina and uterus prior to the injection. This was succeeded by, and intermixed with, a serous or sero-mucous discharge, tinged in colour by the persulphate, which gradually became of the natural colour that the lochia assumes upon the cessation of the presence of the red globules of the blood. *After the injection there was not at any time a particle of fresh blood, or a tinge of it, in the lochia.*"

Sulphate of Quinia and Ferrocyanuret of Iron in Rheumatic Dysmenorrhœa.—Dr. J. B. SNELSON states (*St. Joseph Med. and Surg. Jour.*, Nov. 1860) that he has employed the sulphate of quinia with the ferrocyanuret of iron, for several years in rheumatic dysmenorrhœa, with very satisfactory results. He commences the treatment by emptying the alimentary canal by purgatives; during the menstrual period he uses the warm bath, and gives opium combined with camphor and ipecac to relieve the pain. After the period has passed, he commences with a pill composed of two grains of sulphate of quinia and an equal portion of ferrocyanuret of iron, to be taken morning, noon, and night. These are to be continued during the intermenstrual period.

Excision of the Clavicle.—Dr. WM. M. FUQUA records (*Maryland and Virginia Med. Jour.*, Nov. 1860) a case of this in a healthy man, 40 years of age, in whom the bone was carious, the result he thought of injury. When seen by Dr. F., the outer two-thirds of the bone were enlarged to a considerable extent, with an unhealthy cicatrix along its middle third. The inner extremity was diseased to within three-quarters of an inch of its sterno-clavicular articulation, as was found after removal of the bone. Dr. F. having determined to remove the bone, the patient was placed under the influence of chloroform. "The shoulders being elevated, an incision, beginning at the sterno-clavicular articulation, was carried along the line of the clavicle to its outer extremity. The pectoralis major was then severed from its attachments and turned back. Along the upper border of the bone a few fibres of the sterno-mastoid were divided and the dissection proceeded superiorly until the platysma and trapezius were detached. On turning these back, the bone was exposed; then disarticulated it at its extremity (outer). In like manner, the inner extremity was exposed; not all of it, however, was found diseased. It was determined, therefore, to remove only the carious part. This was done by passing a chain saw beneath the fragment, and with a few strokes it was removed. Having cleansed the wound thoroughly, the extremities were sutured, whereas the intervening portion was held together by adhesive strips. He was then put to bed.

Oct. 10. Expresses himself as feeling badly; face flushed, pulse 90; slept none last night. R.—Morph. sulph. gr. $\frac{1}{4}$.

Oct. 11. This morning, he says, he is much better. Shoulder painful and considerably swollen; removed sutures; union had taken place at either extremity. Daily the wound injected with dilut. chlorinated soda, and adhesive strips applied.

He is now walking about, the wound having almost entirely healed.

Compound Dislocation of the Ankle-Joint; Complete Recovery after Reduction, without Ankylosis.—Dr. R. W. GIBBES, Jr., M. D., reports (*Charleston Medical Journal*, Nov. 1860) a case of compound dislocation of the ankle-joint which is remarkable for the very complete recovery which took place; there being no ankylosis, a very rare result in such an accident.

The subject of it was a negro, twenty years of age, who was struck, Dec. 16, 1859, by a heavy beam, on the outside of his right leg, causing a complete luxation of the foot outwardly. The sole was said to have been turned out and upwards, and the whole of the lower end of the tibia to have been protruded. When seen by Dr. G., about half an hour after the accident, the patient was suffering a great deal of pain, "the foot was in its normal position, but the internal malleolus, with about an inch and a half of the tibia, exposed; the inferior lip of the wound,

which consisted of nothing but skin and cellular tissue, being firmly hitched inside of the joint between the astragalus and malleolus. This had occurred during a spontaneous reduction in raising or removing the patient for a short distance, and made the wound present a tremendous gap, its superior lip extending from about the middle of the limb anteriorly nearly back to the tendo-Achilles. The internal lateral ligament was completely severed. The periosteum did not appear injured, but I removed with my fingers two or three loose pieces of articular cartilage found attached to the malleolus, and then, after stretching and twisting the foot a little outwards, I succeeded without much difficulty in disengaging, with the index-finger, the inferior lip of the wound from under the malleolus. It slipped up readily so as to cover the denuded tibia, and meet the superior lip at a distance of two inches above the point of the bone. The fibula was fractured at the usual place, two and a half to three inches above its inferior extremity. About half a pint of blood had been lost, and considerable oozing continued, which was, of course, increased by my manipulations. A thick compress wrung in cold water was immediately placed inside and above the malleolus, to prevent any further amount of blood escaping or entering the joint, and being held firmly there, the wound was neatly closed by three points of the interrupted suture (silk). A second large compress was then applied over the first one, extending several inches above the wound, and the limb, with the foot turned inwards, bandaged closely to a pasteboard splint reaching nearly to the knee, as in a case of simple fracture of the fibula."

The limb was placed upon pillows, laid on its external side in a semiflexed position, and cold water ordered to be constantly applid over the dressings.

Dec. 17. Passed a good night without anodyne.

18th, afternoon. Not so comfortable; dressing removed; considerable portion of wound united; two anterior stitches taken out, and a wet compress and bandage applied, without any splint; sole kept turned inward.

20th. Some fever, and a good deal of heat and tumefaction about the leg; fourteen leeches applied on the inside of the wound, followed by flaxseed poultices, which afforded relief.

"22d, 9 A.M. The leg is erysipelatous for some distance above the wound, great heat, pitting and painful on pressure, with fever, restlessness, &c. &c. *R.*—Discontinue poultices, apply douche of cold water repeatedly, with wet cloths in the intervals, and keep the foot a little above the level of the body. At 4 P.M. I returned, not knowing but that amputation would be necessary, and expecting at least to have to make deep incisions into the leg; was agreeably surprised to find a wonderful improvement; resolution evidently taking place in the cellular tissue. *R.*—Continue cold applications, and give a full anodyne at bedtime."

23d. Wound discharging more freely; patient better.

24th. Improving steadily.

March 24. Patient has been getting about on crutches for the last fortnight, and although he does not use the ankle-joint in walking, yet Dr. G. can flex and extend it freely, and almost without pain. "Noticing fluctuation about an inch below the sinus, and behind and above the malleolus, a small opening was made with a lancet, from which half a teaspoonful of thick pus escaped. I had no probe with me, but inserted a tent of lint.

"27th. The old opening has closed up, while the new one is discharging freely; and on examining with the probe, I detected a portion of loose bone, about the size of a large grain of corn, which was readily extracted with artery forceps. It presented a smooth articulating surface on one side, and must have proceeded from the posterior part of the malleolus; passive motion is practised from time to time.

"April 27. The openings have both entirely healed, the ankle is hardly at all larger than the sound one, and he begins to use the joint very perceptibly in walking; the foot, of course, swells at times, after much exercise.

"June 2. It is just five and a half months since the accident. I saw Henry to-day, and find that he has abandoned his crutches for more than a week, and is walking with a stick only, there being but a slight limp in his gait.

"Soon after this he went to work at his trade. I saw him to-day, August 23,

and examined the ankle carefully. It seems in no respect inferior to the other one, either in appearance or in the performance of its functions; the movements of flexion and extension, as well as the lateral motions of the tarsus, being equally perfect in both."

Frequency of Accidents or Irregularities during first Labours.—Dr. RICHARD McSHERRY states (*Maryland and Virginia Medical Journal*, Oct. 1860) that, in looking over his notes, he could not but observe, with some surprise, to how great an extent primiparæ are more liable to accidents than multiparæ. In his own practice he has had notable irregularities or disturbances to contend with in more than 33 per cent. of his primiparæ, whilst in multiparæ this has happened in only 10 per cent.

Spina Bifida treated by Injections of Iodine.—Dr. EMIL FISCHER reports (*North American Medico-Chirurgical Review*, Nov. 1860) two cases of spina bifida treated by injections of iodine, by Prof. Gross, at the surgical clinic of the Jefferson College. Both terminated fatally.

Tartro-Citric Lemonade.—Prof. J. LAWRENCE SMITH expresses (*American Journ. of Pharm.*, Sept., 1860) his surprise that the tartrate of soda should have given place as a purgative to the citrate of magnesia, a preparation which he very justly considers as obnoxious to very many objections. Among these he enumerates "the not unfrequent irregularity of its operation, sometimes not acting as promptly as desired, at other times with too great and continued energy, requiring anodynes to arrest its operation. Again, owing to the manner in which it is made, and the want of uniformity in the composition of the commercial carbonate and calcined magnesia, the amount of free acid in the solution varies much, when made at different times by different operators, and with different lots of materials. There being sometimes two or three drachms of free acid present in a bottle, and besides, under all circumstances, the mixture must be quite acid in order to retain for any length of time the citrate of magnesia in solution. Mitscherlich and Bence Jones have both made experiments on citric acid, and they consider it a poison analogous to oxalic acid.

"Yet another objection to citrate of magnesia is the certainty of its undergoing decomposition, resulting in the deposition of an insoluble citrate of magnesia, a change that takes place very rapidly when the bottle is opened.

"With these facts before me, I compounded a preparation of tartrate of soda with lemon syrup and water (at first I introduced a small portion of citric acid, calling the mixture tartro-citric lemonade.)

"It is free from the objections of the citrate of magnesia, is a prompt and certain purgative, without excessive action, and uniform in composition, does not undergo decomposition even after the bottle is opened, even more agreeable to the taste and less costly than citrate of magnesia."

The following is the formula for this preparation:—

"Sal soda	21 lbs. 14 oz. avoirdupois.
"Tartaric acid	15 " "
"Sugar (white)	24 " "
"Water to make	25 gals. "

"It is then put into strong twelve ounce bottles, and thirty-five grains of bicarbonate of soda added to each bottle, and immediately corked and fastened by twine or wire.

"This preparation has been used in Louisville for about six years, and is gradually extending over various parts of the west and south."

SUPPLEMENT.

FISKE FUND PRIZE ESSAY.

Diphtheria ; its Nature and Treatment, with an Account of the History of its Prevalence in various Countries. By DANIEL DENISON SLADE, M. D., of Boston, Mass. (Continued from page 174.)

AN important question next arises as regards the nature of diphtheria. *Is it infectious?* Upon this point, as we might naturally suppose, there is a wide difference of opinion. M. Bretonneau maintained that the exudation of diphtheria possessed a special virulence, and that the disease may be not only propagated by the application of the secretion from an affected surface to sound parts, after the manner of smallpox, but that, like syphilis, diphtheria cannot be communicated from a diseased to a healthy person in any other way. He says :—

“Innumerable facts have proved that those who attend patients cannot contract diphtheria unless the diphtheritic secretion in the liquid or pulverulent state is placed in contact with the mucous membrane, or with the skin on a point denuded of epidermis, and this application must be immediate.

“The ‘Egyptian disease,’¹ is not communicated by volatile invisible emanations, susceptible of being dissolved in the air, and of acting at a great distance from their point of origin. It no more possesses this quality than the syphilitic disease. If the liquid which issues from an Egyptian chancre, as visibly as that which proceeds from a venereal chancre, has seemed under certain circumstances to act like some volatile forms of virus, the mistake has arisen from its not having been studied with sufficient attention. The appearance has been taken for the reality.”²

In support of his opinion, M. Bretonneau has collected a few cases. One is that of M. Herpin, who was surgeon to the hospital at Tours. A child attacked with diphtheria, who had transmitted the affection also to its nurse, was placed under his care. Upon visiting it one day, and during the process of sponging the pharynx, by access of cough, a portion of the diphtheritic matter was ejected from the mouth, and lodged upon the aperture of the nostril of M. Herpin. This he neglected to remove, and the consequence was a severe diphtheritic inflammation which spread over the whole nostril and pharynx. The constitutional symptoms were extremely severe, and the prostration so great, that convalescence occupied more than six months.³ Dr. Gendron, of Chateau de Loire, received on his lips portions of diphtheritic exudation, expelled by a patient during a fit of coughing. Laryngeal inflammation came on with much violence, but his life was saved by taking prompt and decided measures.

In 1826, M. Bretonneau was summoned to the Ecole Militaire, in which diphtheria was prevailing; although many cases of the disease occurred, none proved fatal after his arrival. He states, however, that a boy affected

¹ Considered by Bretonneau as identical with diphtheria.

² *Traité de la Diphthérie.*

³ *Arch. Générales de Méd.*, Jan., 1854.

with frost-bites of his foot, happening to use a bath that had been employed for a diphtheritic patient, his great toe at once became the seat of painful diphtheritic exudation.

M. Lespiau¹ also gives an account of a soldier who, during the epidemic of the disease at Avignon, used the teaspoon of a diphtheritic patient, and in consequence contracted the disease in his mouth; and of another, who was attacked with diphtheria the night after sleeping with a patient.

Similar cases have been cited by authors in favour of the theory of contagion by inoculation. On the other hand, Prof. Trousseau failed in experiments which had in view the inoculation of himself and two of his pupils with diphtheritic matter, and Dr. Harley, of London, was not more successful in similar experiments on various animals.

M. Bretonneau goes so far as to maintain that the facts which he relates, and other details in his possession, corroborate the assertion that the atmosphere cannot transmit the contagion of diphtheria, which is only transmissible by inoculation. This opinion, however, is not supported by the experience of other writers, neither does it accord with the facts deduced from the recent epidemics. In fact, it may be very clearly shown from the evidence already collected, that contagion plays the principal part in the propagation of diphtheria.

M. Isambert,² in his paper upon the epidemic of malignant sore-throat, which occurred in Paris in 1855, gives the following as his experience:—

“Diphtheritic affections sometimes appear sporadically; they also often seem to be endemic, as well as epidemic and contagious. As predisposing causes, we may consider that the lymphatic temperament, a feeble constitution, privation, &c., all exert a decided influence. Youth is much more exposed to the disease than any subsequent age. Locality and overcrowding have a positive effect; so also do cold and changeable seasons.

“Epidemic influences are much the most powerful. As to the contagious nature of the disease there can be no doubt, since many physicians have contracted the disease. The opinion of M. Bretonneau that diphtheria is not transmitted by the atmosphere, but is always the result of inoculation, is altogether too exclusive. With M. Trousseau, we cannot reject infection at a distance as one of the means of propagation possessed by diphtheria.”

With regard to the influence exercised by the moisture of the air, by the temperature, and by the particular locality, M. Trousseau thus expresses himself—

“In the villages of the Loire, remarkable for their salubrity and for their excellent position, I have seen diphtheria prevail to a terrible extent, while the villages of Sologne, situated in the midst of marshes, remained exempt; and, again, hamlets bordering upon ponds depopulated by the epidemic, while others enjoyed a complete immunity.”

The observations of M. Empis led him to favour the idea of contagion. While at the same time he recognizes, as a character of diphtheria, “the property which it has of being generalized in the economy, like the diseases *totius substantiæ*,” a property which, as he observes, may be best appreciated when the disease is studied epidemically.

Carnevale and M. Aurelius Severinus, as well as Franciscus Nola, admit the contagious properties of diphtheria, as well as almost all the writers upon the subject of the seventeenth century.

Dr. Sam'l Bard, as we have before remarked, considered the “suffocative angina” which he describes, as infectious.

¹ Mémoires de Méd. et Chirurgie Militaire, Paris, 1854.

² Archives Générales de Méd., 1850.

"The disease I have described, appeared to me to be of an infectious nature, and as all infection must be owing to something received into the body, this, therefore, whatever it is, being drawn in by the breath of a healthy child, irritates the glands of the fauces and trachea as it passes by them, and brings about a change in their secretions. The infection, however, did not seem, in the present case, to depend so much on any generally prevailing disposition of the air as upon effluvia received from the breath of infected persons. This will account why the disorder should go through a whole family and not affect the next-door neighbour."

To come now to the observations of more modern writers upon this point, we may cite the remarks of Dr. Ranking in his admirable lectures on diphtheria,¹ to which we have before alluded—

"My own conviction is, that it is infectious to a limited degree; by which I mean that when patients are accumulated in small, ill-ventilated rooms, the disease is likely to be communicated; but I do not fear that, like scarlatina or erysipelas, it may be propagated in spite of all sanitary precautions, still less that the infection can be conveyed by the clothes or persons of those who visit or superintend the patients. That it commonly spreads through the family once invaded is to be attributed, in some degree, to the persistence of the same cause as originated the first case. What that cause is, it is difficult to determine.

Stench and poverty and crowded rooms have ever been the sad heritage of the agricultural labourer, but diphtherite is only of recent origin. Doubtless these insanitary adjuncts to a labourer's life predispose him and his children to the assaults of any epidemic malady, but the true and specific cause of diphtheria is a something superadded, and which our senses cannot appreciate."

In an article by Dr. Edw'd Ballard, of Islington (*Med. Times and Gaz.*, July 23, 1859), the following facts are given in support of the infectious character of diphtheria as it came under his observations:—

"1. Infectious diseases habitually spread in families they invade. Out of 47 families there were only 15 in which the other members all remained healthy. Of course it may be argued, in opposition, that all the members of a family are equally exposed to the operation of local causes of disease.

"2. As a rule, it spread in the houses it invaded chiefly among those members of the several families who were most closely in communication.

"3. In no case where separation from the sick person has been effected early in the disease, have I noticed that it has spread to the separated individuals. In one case where communication had been allowed for three days before separation, a child was seized with diphtheria on the sixth day of removal from home.

"4. The following special instances (of which we give one) may be adduced of communication of the disease from one house to another:—

"Jane J., æt. 10 years, resided at Islington, with her mother, an aunt, and three sisters. On May 1st and 2d she was on a visit at the house of an uncle, whose daughter, Jane's cousin, was kept at home because she was believed to have a cold. On the 2d, this child exhibited decided symptoms of diphtheria; the attack was slight and she recovered.

"On May 6th, a servant in this house was taken ill with a severe attack of diphtheria, and was removed to St. Bartholomew's Hospital, where she died. On the 2d, Jane returned home, was taken ill on the third with diphtheria in a severe form, and died on May 9th. Her mother and a sister, aged fourteen years, were both taken ill on May 11th. She had not been so much with her daughter as other members of the family up to the 8th, when she sat up with her all night. The tonsil sloughed, and there was a complete cast of the trachea expectorated. She died on the 18th. The sister, who was also attacked on the 11th, slept with her mother, and, when not at school, was continually in and out of Jane's room, sitting there sometimes for hours together. She died on May 14th, asphyxiated.

¹ Lancet, Jan. 15, 1857.

Another elder sister, who slept with Jane and the aunt, suffered from nothing but a slight sore throat."

The results of injuries instituted at fifty-seven houses where fatal cases occurred, with respect to local causes of disease, were as follows:—

"In more than half the houses, then, which were examined, there was some defect or other in the sanitary arrangements or in the surrounding conditions of the patient. In the greater number of the houses thus deficient, the fault was discovered in the state of the drainage."

On the other hand, some writers of experience maintain that diphtheria possesses no contagious properties whatever, and others accord to it but comparatively feeble ones.

M. Daviot,¹ in a memoir on diphtheria, says—

"Pharyngeal diphtheria is purely and simply an epidemic disease. Like other diseases which assume this character, it only manifests itself in those localities and individuals which have the most affinity for it. Springing from an alteration in the constituent elements of the atmosphere, an alteration unknown in its essence but appreciable in its effects; it is propagated through the medium of that fluid. . . . A great number of persons were struck by the epidemic a few days after arriving in the infected places, and without having communicated with any patient."

M. Daviot denies, as regards an epidemic which he describes, that it generally happened that all or the greater number of the members of a family were attacked at once, and states that it was quite as common that only a certain number of persons living under one roof were affected, and that the successive attacks took place at considerable intervals. M. Daviot thinks that such results can only be accounted for by—

"Similarity of organization and predisposition in individuals placed under the same hygienic circumstances, and, therefore, subject to the same morbid influences. . . . Will any one contend that the contagious principle could have six months, a year, or even more of incubation before its development? Such an explanation is contrary to all probability, and does not require to be repeated."

M. Daviot did not meet with an instance where diphtheria was communicated by personal intercourse. He remarks that neither the attendants nor those who cauterized the throats of affected children contracted the disease. He concludes that pharyngeal diphtheria is not in itself contagious, and that it only appears to be so when associated with eruptive fever.

Dr. Crighton,² of Edinburgh, records the results of 45 cases of diphtheria occurring in his practice. Of these, 25 were males and 20 females; out of this number 9 proved fatal, or 1 in 5. Of these, 6 died of asphyxia with membranous exudation in the air-passages, and 3 by pure asthenia. They were instances of faucial diphtheria. In one case, aged 21 months, vulval diphtheria occurred. The mean age of the fatal cases was within a fraction of seven years.

"In only two cases was there anything like proof of contagion, and, from all that I have seen of diphtheria, I believe that, although it would be incorrect to separate it from the list of communicable diseases, yet it is very feebly so compared with many others. I may mention one instance which struck me particularly, where, in a large family of six or seven children, and chiefly under the age of twelve, a child had the disease in a very severe form, and although he was

¹ Memoirs on Diphtheria (New Sydenham Soc.), London, 1857.

² Notes on an Epidemic of Diphtheria. By R. W. Crighton.

never isolated during the day from the others, but lay on a sofa in a room where I generally found several of them at my visit, they all escaped."

In a letter from Dr. Monckton (*Med. Times and Gaz.*, Feb. 26, 1857), after much experience in the disease during epidemics which prevailed in the county of Kent, he says:—

"No decisive instance of its communicability has come before me; on the contrary, I have seen it attack individuals only, in a family of liable persons, much more frequently than I think scarlet fever would have done. My own conviction is, that diphtheria is epidemic, endemic (*i. e.* largely affected by locality), and non-contagious, or, if contagious at all, vastly less so than scarlet fever, from which last it is very distinct."

Space would fail us, if we attempted to bring forward more than a very small portion of the argument and evidence which have been offered in favour of the contagious or non-contagious properties of diphtheria. There are, however, one or two points which we may consider further.

In connection with certain epidemics, especially in France, there were frequently observed cases of cutaneous diphtheria, which, from their persistence and superficial site, seemed peculiarly to favour transmission by contagion. As a general rule, it was never developed unless when the epidermis was raised or removed; and the observations of M. Trousseau, and others, have incontestably proved that the diphtheritic affections of the skin are of a nature identical with those which have their seat in the mucous membrane of the larynx and fauces.

Now, although those who favour the idea of contagion find in the phenomena of cutaneous diphtheria strong ground for the support of the theory of inoculation, there are facts which would seem equally to oppose it. For example, it has been observed in these epidemics, that the false membrane upon the skin not only presents itself in those not previously affected with faucial diphtheria, but it not unfrequently attacks remote parts, such as we should suppose were inaccessible to inoculation, as, for example, the folds of the groins in children, and the spaces between the toes. "A single well observed fact of this kind is sufficient to cast a doubt on the theory of inoculation."

Again, it sometimes happens, according to M. Trousseau, that diphtheria, especially when it occurs as a sequel to measles and scarlatina, is complicated with an eruption of bullæ of *rupia simplex*. These often become the seat of cutaneous exudation. As the bulla becomes flaccid from the absorption of its contents, instead of the formation of a thick brownish crust, it is observed that a firm concretion can be felt beneath the still entire epidermis.

The influence which meteorological and cosmic conditions exert in the production of diphtheria, is no better understood than is the relation existing between these same conditions and the production of other epidemic diseases.

Bretonneau, for example, had the idea that his diphtheria needed a damp atmosphere for its development. In the recent epidemics, both in France and in England, many instances are recorded where the disease prevailed in very dry and high situations. And in our own country, similar observations have been made. Dr. Wooster, in a monograph on diphtheria as it prevailed in California, explains how far the views of Bretonneau are applicable to the disease as it presented itself to his notice. He says:—

"In our climate the air in summer becomes so dry, that if an ordinary soft, wooden pail or bucket, be half filled with water, and set in the sun in the open

air for six hours, and then two quarts of water be added, it will leak through the joints of the shrunken staves, above the surface of the first portion of water. A miner uses a bucket to bail water from a hole all the forenoon, and, although it is perfectly saturated with water, yet if he leaves it in the sun while he goes to his dinner, when he returns it will often fall to pieces as he attempts to take it up.

"This is the kind of air in which the disease has occurred with unequalled fatality in this State. In this city I cannot ascertain that a case has occurred in that part of the town built over, or near the waters of the bay, or on the salt marshes near it. But I have seen cases in the high part of the city, and on bluff headlands extending into the bay, points that from their elevation and constant exposure to a strong breeze, would be thought inaccessible by any morbid effluvia."

In this connection, we cannot refrain from citing the somewhat poetical, but at the same time very truthful remarks of Mr. Ernest Hart.¹

"It was observed of diphtheria in France, and it is equally characteristic of its course in England, that it did not obey any known climatic or meteorological laws. It descended upon Tours, in the rear of the Legion of La Vendée; it broke out in crowded and ill-ventilated barracks, and it spread throughout the town. It visited, alternately, the open hamlets of the rural departments and the crowded courts of the great cities. It raged in Orleans and in Paris, through the Sologne and in the Loiret. It reached the sea-side, and fell with violence upon the infant population of the city of Boulogne. It appeared to be equally independent of all atmospheric conditions. Was a theory formed that its intensity depended upon the solar influence, and that the heat of the summer months lent fresh force to its destructive attacks—soon it raged with greater violence in the winter months, and during the cold season. Was a connection traced between the localities of its invasions, and the marshy ill-drained character of the land—the next season it was found to ravage dry and elevated stations with equal rage. It has been no less careless of the limitations of heat, cold, dryness and moisture, since it has established a camp in this country. * * * It has swept across the marshy lowlands of Essex, and the bleak moors of Yorkshire. It has traversed the flowery lanes of Devon, and the wild flats of Cromwell that are swept by the sea-breeze. It has seated itself on the banks of the Thames, scaled the romantic heights of North Wales, and has descended into the Cornish Mines. Commencing in the spring months, it has continued through the summer, and if extremes of temperature have appeared to lend it fresh vigour, and the heat of the dog-days, or the severe frosts and sleet of winter have fostered its strength, yet moderate temperature has not greatly abated its influence, and it has struck a blow here and there through all the seasons."

Without doubt, diphtheria, as well as other diseases of a similar character, follow general laws, and in many cases we are obliged to confess our entire ignorance as to the exact nature of those laws.

But if we cannot ascertain the influences which govern these epidemics, perhaps on closer investigation we may discover certain individual or hygienic circumstances which may affect them either as direct or as predisposing causes. Thus, as a general rule, we shall find that diphtheria is more frequently associated with the ill-ventilated, contracted hovels of the poor, seizing by preference upon the unhappy subjects depressed by poverty and its attendant evils. Yet these are not the exclusive conditions for the development of diphtheria. We find in the various reports of these later epidemics that the disease has made its appearance, and carried off its victims, in the abodes of refinement and wealth.

"Zymotic in its nature, it tends to fasten upon whomsoever is debilitated by previous disease, or by a constitution naturally feeble and artificially effeminized,

¹ On Diphtheria, its History, &c., by Ernest Hart, London, 1859.

or whose vitality is lowered by the depressing influences of luxury, indolence, and inactivity; and the habitual defiance of physical and hygienic laws, which is so frequent an element in fashionable life. Hence individual causes come into play, and introduce this associate of the poor into the palaces and mansions of the great, which they so often fringe. Diphtheria finds there its victims pale and anæmic, or grossly sanguineous, and unhealthily excited.”¹

Finally, all we can affirm is, that, as a general rule, all anti-hygienic conditions of any kind favour the invasion of diphtheria, as well as of other similar epidemic diseases.

An element in the nature of diphtheria is of recent discovery. We refer to the presence of albuminous urine in the disease. The first observation upon the relation of albuminuria to diphtheria appears to be referable to a case reported by Mr. Wade, of Birmingham, to the Queen's College Medico-Chirurgical Society in December, 1857, and afterwards published in his *Observations on Diphtheria*.² Shortly after this, during researches on this disease at Paris, MM. Bouchut and Empis made a similar discovery. Albuminuria did not exist in every case examined, but it was seen in twelve out of fifteen cases. Both of these observers attach great importance to this renal complication, as affording an anatomical explanation of the cause of death, when this cannot be attributed to either of the other modes, viz., death by asphyxia or general poisoning. In fact, it was considered by them to indicate the infectious nature of the disease, in this respect resembling purulent infection, which is accompanied by a similar alteration of the urine. On this point M. Bouchut arrives at these conclusions. “Albuminuria in the absence of scarlatina or asphyxia (dependent on laryngeal obstruction) is a sign in diphtheritic diseases of a commencement of purulent infection, and coincides with a very great gravity of the disease.” These conclusions he founds on these observations, that both in diphtheria and purulent infection there are, 1st, alteration of the colour of the blood, which assumes a bistre tint; 2d, masses of pulmonary apoplexy, more or less numerous, similar to those which precede the development of metastatic abscesses; 3d, ecchymoses of purpura on the skin, or the serous membranes and the viscera. MM. Bouchut and Empis are of opinion that there is nothing farther necessary to establish the connection between these diseases than the presence of visceral abscesses, or purulent collections in the serous membranes in addition to the preceding alterations.

In all the children under their care for diphtheria, the urine was analyzed both by heat and nitric acid. When albuminuria was present, the urine contained a very large proportion of salts, which rendered it cloudy and of a milky appearance at the moment of emission. At first the heat caused the salts to be held in solution, then at a higher degree of heat, the albumen was precipitated. In three cases, the precipitate was very large, in the remainder, it was moderate in amount.

Together with albumen, Mr. Wade usually finds in the urine tube casts and renal epithelium, the former being either “small waxy casts,” or “epithelial casts.” He is of opinion that albuminuria produces a diminution in the total amount of solid excreta, that is, that the special functions of the kidney are suspended, whereby symptoms arise which are indicative “of the retention within the system of those matters which should be excreted.”

Our author has not informed us at what period of the disease he has

¹ On Diphtheria. By Ernest Hart, London, 1859.

² Observations on Diphtheria, by W. F. Wade, B. A. &c. &c., 1858.

first detected albuminuria, neither does he give any observations tending to show how far the progress of the disease is affected by this condition of the kidneys, which would seem to be indicated by the presence of "casts," &c., in the urine.

In a paper, communicated to the *British and Foreign Med.-Chirurgical Review*, Jan. 1860, Mr. Sanderson, upon the basis of eight cases, is not inclined to admit either of the doctrines advanced by Mr. Wade. He says :—

"In eight cases in which I have had the opportunity of making repeated observations as to the condition of the urine, the only ones which occurred to me since my attention has been directed to the subject, it has been albuminous in all."

Dr. Sanderson, having given a brief report of each of these cases, goes on to remark :—

"Although in several of the cases above related the cessation of albuminuria was clearly coincident with the amelioration of the patient, and the disappearance of the most alarming symptoms, it is not less certain that in one or two others albumen existed in large quantities in the urine, although the cases maintained a mild character throughout. From this it may be inferred that albuminuria is not in itself so alarming a symptom as M. Bouchut is inclined to imagine."

As it appeared of importance to Dr. Sanderson to ascertain whether the existence of albuminuria coincides with the solid excreta of the urine, he directed his experiments to that end. He offers, however, only one satisfactory observation, the following :—

CASE VI. W. D., male, aged 30. Albuminuria first observed about the eighth day; disappeared three days after; abundant.

General character of symptoms.—Extremely grave; excessive prostration; intense adynamia, with nervous agitation and busy delirium. Concretion not examined.

Result—recovery.—Slow convalescence, with extreme muscular weakness.

During a period of about nine days the albuminuria continued. During this time observations were made as to this condition of the urine. Without giving the two tables of analysis, we come directly to the result.

"At the acme of the disease when the urine was intensely albuminous, when there was complete anorexia, and the ingesta were reduced to a minimum, the quantity of urea excreted in a period of twenty-four hours was about twice as great as that excreted during a similar period when convalescence was established, and he was eating with an appetite the ordinary diet of the hospital, with extras.

"The above facts show that diphtheria agrees with the other pyrexia in being attended with a marked increase in the excretion of urea, and that the existence in the kidney of the condition which is implied by albumen and fibrinous casts in the urine, does not necessarily interfere with that increase in the elimination of nitrogenous material. There is, therefore, no reason to apprehend the occurrence of uræmia as a consequence of the renal complication in diphtheria; this complication not being the cause of the dyscrasia, but merely the index of its existence."

With reference to the presence of albuminuria in diphtheria, there have been but few accurate observations made, and, in fact, until comparatively recently, it was thought that one distinctive mark of diphtheria over other kindred diseases was, that there was no albumen and no dropsy present. Certain it is that albuminuria has manifested itself throughout almost the

entire course of grave cases of diphtheria, and which have yet terminated favourably. On the other hand, cases have occurred which have proved fatal when it has been absent.

There can be no doubt of the serious character of this renal complication, but further research and observation are necessary before we can ascribe to it any settled prognostic value.

In this connection we may also speak of the remarkable after symptoms of diphtheria, which have been observed by almost every practitioner who has had even a limited experience during the epidemics of the last few years, and which have been particularly referred to by MM. Trousseau and Bretonneau, and also by M. Faure.

After apparent recovery from the immediate effects of the disease, in many cases there still seems to be lurking in the system the morbid poison, whose special affinity is for the nervous system. Thus, prominent among the sequelæ of diphtheria, is paralysis in its various forms, more frequently local than general, also otalgia, amaurosis, headache, ophthalmia, &c. Epidemics of this last have been observed in Germany, and described by Graefe, in 1854,¹ and in France by M. Jobert in 1857.²

The most frequent form of paralysis has been that of the soft palate. The symptoms are a nasal twang in the speech, incapacity for suction, and the regurgitation of fluids by the nostrils. This form was thought both by M. Trousseau and others to be local in its origin. But further observation has led them to change their views.

M. Trousseau makes the following clinical remarks:—

“The pathology of the paralytic affection was, for a long time, altogether misunderstood both by himself and others. In consequence of its being more frequently local than general, in other words, the palate and pharynx being more usually affected with paralysis than the system generally, he was for a long time under the impression that the loss of power was dependent upon the inflammation of the coats of the nerves supplying these parts, and on infiltration producing pressure on their motor muscles. A more extensive experience, however, of the general character of the paralysis which accompanies and follows diphtheritic affections, caused him to change his views, and he now believes that loss of power and sensibility is the direct consequence of the peculiar diphtheritic poison acting generally on the system, and strangely modifying the blood. He further stated a fact which has often come under his observation, that many children who have been subjected to the operation of the tracheotomy fall victims to paralysis of the epiglottis and larynx.”³

But it is to Dr. Faure that we are more particularly indebted for the most complete account of these remote consequences of diphtheria. He describes this peculiar condition of the system

“As a state characterized by a gradually increasing loss of power, showing itself especially in all those functions connected with muscular movement. In some instances, several sets of organs are affected, in others only one, while again in others, the whole system is involved in the general debility. But whatever are the variations in this respect, there is no definite relation between the severity of the primary symptoms of diphtheria and that of the sequelæ. The primary symptoms, though very formidable, do yet by no means of necessity prove fatal, while, on the other hand, the comparative mildness of the attack will not justify an absolutely favourable prognosis, since death sometimes follows where everything had seemed to warrant the most confident expectation of recovery.”

¹ Gazette Hebdomadaire, 1856.

² Archives Générales, 1857.

³ Med. Times and Gazette, Jan. 27, 1859.

Several cases are given by M. Faure, in illustration of the various phases of this condition, and he sums up as follows :—

“Some time after an attack of diphtheria, from which the patient has so completely recovered that no trace of false membrane is left behind, the skin grows more and more colourless without apparent cause, so that at length it assumes almost a livid pallor. Severe pains begin at the same time to be felt in the joints, the patient loses power over his limbs, and soon sinks into a state of indescribable weakness. At the same time, the disorders that appear in different functions show that the various organs which should minister to them are involved so far as they are dependent upon muscular power. In this respect, however, the phenomena are not constant, for sometimes it is one set of organs, and sometimes another which suffers most from this weakness. Very generally, in consequence of the want of muscular power, the patient becomes unable to sit upright, or does so with great difficulty, while the legs cannot bear the weight of the body; all the movements grow uncertain, tottering, hesitating and apparently purposeless. Very remarkable disorders show themselves also within the throat, for the velum is completely paralyzed, and hangs down like a flaccid lifeless curtain, which interferes with speech and deglutition. All the muscles of the jaw, neck, and chest are partially paralyzed in consequence of which mastication is rendered difficult, and the food can be neither easily moved about in the mouth nor readily swallowed. Vision is impaired, squinting is not unusual. The sensibility of the skin is much diminished, in the limbs it is sometimes completely lost, though morbid sensations, such, for instance, as formication, are sometimes experienced. Œdema of the various parts often occurs, and occasionally parts here and there lose their vitality, and become gangrenous. No general reaction occurs; fever is rare. The features grow duller and more and more expressionless, though a foolish smile sometimes crosses them, or now and then a ray of intelligence appears. Some patients have frequent fainting fits. As the condition goes on from bad to worse, the weakness becomes extreme, and death at length follows some fainting fit, or takes place when exhaustion has reached its uttermost; life, as it were, quietly, almost imperceptibly, passing away.”

Such are some of the most common sequelæ of diphtheria. It is not to be understood, that in these cases a fatal termination is necessary, nor that the symptoms are necessarily so severe as have been depicted by Dr. Faure.

Further observations will undoubtedly clear away much that is obscure upon these singular after-effects of the disease. Even in our present state of knowledge of them, they certainly furnish materials towards the solution of the question—the identity or non-identity of diphtheria and scarlatina.

These cases are to be treated on tonic principles. The nervine tonics are especially indicated. In cases of local paralysis, astringents, feeble cauterization, and electricity may be employed.

Summary.—Our knowledge of the nature of diphtheria may be summed up in the following words :—

Diphtheria is a *specific* disease. This fact is shown by its origin, its progress, its manner of termination, and its sequelæ.

Its diagnostic sign is the formation of an aplastic membranous exudation upon any portion of the cutaneous or mucous surface which is exposed to the contact of the atmosphere.

It is propagated by infection and contagion, and is both epidemic and sporadic in its invasion.

Its characters plainly indicate that it belongs to the category of *blood diseases*.

It is not allied either to cynanche trachealis, or to scarlatina.

The treatment is to be directed to the control of the exudation, and to the support of the constitution by means of tonics, stimulants, and by a nutritious diet. Of this we shall speak next.

Treatment.—Like all diseases which have prevailed epidemically, and which have appalled by their severity and fatality, or perplexed by their novelty, diphtheria has been subjected to a great variety of treatment. It is only within the last few years that anything like unanimity has existed in the profession in regard to this important point. Not to go back further than the period of Bretonneau's memoir on this subject, we shall find that an activity of treatment prevailed which would scarcely coincide with the ideas of the present day. Bleeding, both local and general, blisters, certain local applications to the pharynx, rapid mercurialization, formed the treatment in all cases. Mercury, in fact, was considered as the sheet anchor by a great majority of medical men. To quote the words of Dr. Sam. Bard: "But although I consider mercury as the basis of the cure, especially in the beginning of this disease, I do not by any means intend to condemn or omit the use of proper alexipharmics and antiseptics." Although a few practitioners may still make use of this therapeutic agent, it is now generally agreed that such is the asthenic nature of the disease at the present day, that depletion is not borne well in any form, neither is the action of mercury defensible either in theory or practice.

As we are as yet unacquainted with any specific capable of arresting the course of diphtheria, our treatment must be directed simply to the conducting our patient in his progress through the disease. In the first place strict attention to certain hygienic rules are necessary. The most scrupulous cleanliness of person and surroundings, free and uninterrupted ventilation should be insisted on. If there are other children in the family where the disease breaks out, the well ones should be sent away, or at least should be kept out of the room where the infected individual lies.

As regards external local applications in the very early stages of the disease, if there is much heat and engorgement about the throat, cold, wet compresses may for a time give relief. As the disease progresses, warm fomentations, and emollient applications generally, may be substituted. Blisters are to be avoided, both on account of their adding, by their irritation, to the engorgement and to the cellular infiltration, and on account of themselves putting on a diphtheritic or sloughy appearance. As everything in the aspect of the disease, from the first, indicates that the powers of life must not be lowered, but on the contrary that the tendency to prostration must be averted in every way, neither leeches nor local bleeding are admissible, except perhaps in very rare exceptional cases. In certain epidemics, there is also danger that the wounds might take on a sloughy character.

Many practitioners commence the treatment of diphtheria with the administration of an emetic or a purgative. Under certain circumstances an emetic may be advisable, particularly when there is an early tendency to croupal symptoms. For the purpose, full doses of ipecac are preferable. Anything like purging, however, is to be sedulously avoided on account of the asthenic nature of the disease. The bowels may be moved by simple enemata, or by some mild laxative.

There are occasional cases of diphtheria so mild in character that local applications to the fauces may be sufficient, but as a general rule it may be conceded that the disease requires a tonic and sustaining treatment, par-

ticularly is this often the case at a late period of the disorder. In cases at all severe, the tendency is to depression and to death by asthenia, unless earlier terminated by asphyxia.

Stimulants and nourishment should be commenced with early, and persisted in systematically. The amount, of course, must depend upon circumstances, but in order to insure efficiency, they should be varied, should be given in small doses at regular and frequent intervals, and if rejected by the stomach should be given in the form of enemata. So also with respect to children, when they are frightened and distressed by painful attempts at swallowing, and absolutely refuse everything, we have the same resource.

Injections of beef-tea, with brandy and quinine, may be employed, and thus life may be not unfrequently sustained, when otherwise it would inevitably have been extinguished.

With regard to the particular form of internal tonics, there is a variety of opinion. There are some which, perhaps, promise a greater chance of success than others, among which we may mention quinine, tinct. ferri chloridi, and chlorate of potash. But as each of these has powerful advocates in its favour, we imagine that, provided the strength of the patient be sustained, it is of little importance by which of these tonics it is accomplished.

The tincture of sesquichloride of iron seems now to be preferred by the great majority of practitioners, on account of its unquestionable usefulness in the more asthenic forms of disease. The dose is from 10 to 15 drops, in water, every three or four hours.

"Of the many internal remedies which have been advised, we do not know of any on which so much reliance can be placed as on the tincture of sesquichloride of iron, with chlorate of potash, chloric ether and hydrochloric acid in the form of mixture, sweetened with syrup, full doses being employed according to the age of the patient, and frequently repeated. A free use should be made of generous wine, beef-tea, coffee, eggs, in combination with brandy and wine, milk, and whatever other form of nutriment the ingenuity of the surgeon or the fancy of the patient can suggest."¹

Quinine may be administered in mixture with or without the dilute hydrochloric acid, or in the form of pill; the dose and frequency of repetition must be governed by circumstances. If the chlorate of potash be preferred, it should be given in doses of from four to eight grains, according to age, in a bitter infusion with two to five drops of the dilute hydrochloric acid.

We come now to speak of the auxiliary measures to be adopted in the treatment of this disease, and first, of the local applications to the fauces. The propriety of these has been called in question by some writers, on the ground that the disease is a constitutional one, and, therefore, that they can be of no service. But we must answer to this, that there can be no more reason why the local remedies are not as applicable to this affection as in other constitutional diseases, for example, as in syphilis, scrofula, carbuncle, &c. In an excellent paper by Dr. Bristowe,² on the treatment of diphtheria, the following reasons are given for discarding heroic applications to the fauces:—

"1. That the throat affection is merely a local evidence of a constitutional disease, which is unlikely to be arrested in its progress by any treatment directed to the secondary manifestations only. 2. That the throat affection rarely kills, except by involving organs, such as the trachea and deeper tissues of the neck,

¹ Lancet, Sanitary Commission.

² Med. Times and Gazette, Sept., 1859.

which are beyond the region of the possible influence of such agents. 3. That if the theoretical correctness even of such treatment be admitted, the application of remedies to the surface of a thick false membrane, with the hope that they may affect the subjacent mucous tissue, is not only clumsy, but, as regards the object intended, practically useless; and that the prior forcible removal of the membrane from the entire surface, in order to their efficient employment, is unjustifiable in the early stage, even if possible, and is likely only to be followed by increased inflammation, and reproduction of false membrane. . . . Of course, if a gangrenous state of the tonsils, or any other local complication, supervenes, such topical applications as are commonly had recourse to in like conditions of the throat should be employed."

While we concur in the remarks of Dr. Bristowe so far as regards the forcible removal of the false membrane, particularly in the early stages, the experience of almost all medical men of the present day bears witness to the efficacy of the application of caustics or escharotics to the throat.

On the other hand, some writers maintain that the disease at the outset is a local one, which rapidly brings on a general *intoxication*. This would be a still stronger argument—if we granted this to be true—for these very local remedies, if applied in season, might prevent a further extension of the disease.

There are a multitude of substances which have been employed as local applications to the fauces, each of which have their special advocates. During the last four years the nitrate of silver, either solid or in solution, has been perhaps more extensively used than any other substance. This, when used early in the disease, seems in many cases to check the progress of the exudation; yet it does not answer the purpose altogether, and further experience has somewhat diminished confidence in it. Indeed, in some instances it is a question whether the free application of this caustic does not rather add to the evil.

"I have mentioned that I thought that the indiscriminate mopping of the fauces, as it is called, with solutions of nitrate of silver, was frequently attended with injurious results in this disease, principally, I believe, for this reason, that, owing to the struggles of the little patient, it is impossible to apply the caustic solution with that precision which the case absolutely requires. Thus, it is applied to parts which are entirely free from disease. I have been told of cases where the inside of the cheeks has been covered with it; in coughing, a portion of it has been expelled upwards through the nose, corroding the susceptible surface of its mucous membrane; and, again, other portions of it have seemed to pass downwards into the pharynx and œsophagus; and I am not sure that, during the convulsive struggling of the patient in resistance, some of it may not also enter the larynx, where it may possibly initiate those inflammatory changes in the mucous membrane of the air-passages which are too frequently the harbingers of death in this disease."

Still, if carefully and properly used, nitrate of silver in many cases is undoubtedly of benefit. If in solution, it is to be applied by means of a probang or brush, swabbing over the diseased surface quickly, at the same time thoroughly. The strength of the solution should be from 30 to 60 grains, and perhaps higher, to the ounce of water, according to circumstances. For children, a full-sized camel's-hair brush is best. The child should be placed on the lap of an attendant, and the head firmly fixed. If he will not open the mouth, the nostrils should be closed for a few moments, and as he opens the mouth for breath, the jaw should be at once depressed, and then, the tongue being kept down by the finger, the fauces are brought well into view, and the solution thus thoroughly applied. The utmost

¹ Observations, &c., by F. A. Bulley, F. R. C. S., Med. Times and Gaz., Apr. 1859.

gentleness and patience should be exercised; at the same time, firmness, for upon the effectual accomplishment of this proceeding the success of the treatment will greatly depend. This should be repeated every three or four hours, so long as it is necessary.

The nitrate of silver may also be employed in the solid form, but this we should not advise, particularly in the case of children. During the struggles of the little patient the crayon might become broken, an accident which has happened, and fragments fall into the œsophagus or larynx, giving rise to serious lesions. Moreover, the nitrate of silver in this form has the disadvantage of creating a more decided eschar than does the solution, simulating the diphtheritic exudation, and thus hindering the perception of the progress of the disease.

The tinct. ferri chloridi is an excellent substitute for the nitrate of silver, and is now generally preferred by a great majority of practitioners both in this country and in Europe. This may be applied by means of a brush or sponge, or in a gargle of the strength of two drachms to eight ounces of water.

The hydrochloric acid may be useful in some cases, and has also been extensively advocated. It is to be applied in a similar manner to the other substances of which we have spoken. In the case of children, the addition of honey to the acid is desirable. This is a favourite topical remedy of M. Bretonneau. He says:—¹

“At the commencement of the epidemic at Tours, topical remedies suggested themselves. The beneficial effects of hydrochloric acid soon gained for it an exclusive preference. In the use of this acid, it is preferable to employ it in full strength, at long intervals, than to return to less energetic applications more frequently.”

Another gargle, which is very efficacious, and which has also the advantage of correcting the fetor of the breath and the secretions of the throat, is a solution of the chloride of soda, in the proportion of one drachm to six ounces. This may either be employed by itself, or combined with other applications. The same may be said of the chlorate of potash. The combination of chlorate of potash and hydrochloric acid with the tincture of the sesquichloride of iron is strongly to be recommended, especially in the croupal cases, the chlorate of potash having an undoubtedly antidiphtheritic influence, where time exists to bring it into play.

Numerous other applications to the fauces have been advocated and successfully employed. Among these may be mentioned, strong solutions of sulphate of copper; the chloride of sodium, either by itself or combined with vinegar; gargles of tannin, capsicum, &c.; Monsell's salt, in powder. Of this last substance, Dr. Beardsley, in his paper upon the epidemic at Milford, Connecticut, to which we have previously referred, writes:—

“Monsell's salt was found to be the most efficacious and valuable of all topical remedies, affording in some instances decided relief. Its active astringent property rendered it peculiarly appropriate, and well adapted to obviate that relaxed and enfeebled condition of the throat which attends the advanced stage of the disease.”

In cases where there is much tonsillitis, we may employ the inhalation of steam, mucilaginous gargles, warm fomentations, &c. These often afford marked relief, and are useful adjuncts to the other treatment.

M. Bouchut² advises the ablation of the tonsils early in the disease, not only for the purpose of removing the exudation which appears on them, and

¹ *Traité de la Diphthérie.*

² *Gazette des Hôpitaux, 1858.*

which he considers the localization of the disease, but also of facilitating respiration. Such a proceeding we should consider decidedly inadvisable, to say the least, for the following reasons : In the first place, the exudation is almost sure to re-form upon the cut surface; next, there is a great risk of severe hemorrhage; and finally, any cutting operation, however simple, had better be avoided, if possible, especially upon young children, and in a disease so asthenic in its character.

The removal of the tonsils in this disease might possibly be practised upon an adult, when there is great tumefaction, and for the purpose of facilitating respiration, and for this purpose only.

When the nasal fossæ have become implicated, various solutions should be injected through the nostril. MM. Bretonneau and Trousseau recommend a solution of alum, or the insufflation of the same substance in powder. We should advise, however, a solution of the chloride of soda, in the strength of two drachms to eight ounces of water, to which two ounces of glycerine may be added. Frequent injections of warm water and soap may also be thrown up, in order to cleanse the parts and remove the offensive odour.

Injections of nitrate of silver, sulphate of zinc, and, in fact, any solution which is applicable for the fauces, will answer a good purpose for injecting the nasal fossæ.

But when in spite of all means of treatment, energetically and judiciously employed, the disease progresses steadily onwards, and the larynx and trachea are invaded by the exudation, giving rise to symptoms of imminent danger, then the important question of tracheotomy must be entertained.

Without going into a history of tracheotomy, or a recapitulation of the arguments on the one side or the other, we most unhesitatingly say that, under the circumstances above mentioned, this operation is a resource which we are in duty bound to employ for the safety of our patient, and in view of what experience teaches us is otherwise certain death. It is not that by so doing, we increase his chances for life solely, but in the case of an unfavourable termination, we render his last moments less distressing.

It has been urged that the operation of tracheotomy is not warrantable in those cases of croup which are the result of the extension of the diphtheritic exudation to the larynx, the patient not merely dying from asphyxia, but sinking likewise from a constitutional infection; on the other hand, it has been urged that there is even a better chance of success of the operation than in true croup, the membrane being less apt to spread to the bronchia.

For ourselves, we can see no validity in any arguments which have been adduced either in favour of or against tracheotomy in diphtheria, which would not be equally applicable to the same operation in cases of croup, and in this opinion we are supported by statistics.

In this connection we would refer to the remarks made by Dr. Fuller, in the course of a paper read to the Royal Med.-Chirurgical Society, in 1857, with a view to a correct appreciation of the subject. Dr. Fuller began by referring to the difference existing physiologically and pathologically between idiopathic inflammatory croup, and the diphtheritic form of the disease which commonly prevails in France, and he pointed out that the objection usually urged against French statistics of tracheotomy in croup, viz., that diphtheritic cases are much more favourable for the performance of the operation than are the croup cases usually met with in Great Britain, has no foundation in fact. By reference to 483 cases in which tracheotomy had been performed for the relief of croup in France, he showed that the operation had been eminently successful in the hands of French surgeons,

and he reminded the society that inasmuch as the condition of the throat externally and the nature of the accompanying fever in diphtheritis are by no means favourable to the operation, the success which has attended it can be explained away only on the supposition often put forward by English writers, that in France the disease seldom extends into the trachea and bronchi, and is rarely accompanied by bronchitis or pneumonia. The fallacy of this supposition was, however, shown by reference to the recorded results of the post-mortem investigations of 311 cases of croup in France, and he also showed that in regard to its pathological effects, diphtheritis, when accompanied by croupal symptoms, does not, as compared with inflammatory croup, present any greater prospect of success for the operation than it does in the character of its accompanying fever, or the condition of the throat externally.

Granting then that the two diseases, inflammatory croup and diphtheria, stand on an equal footing as regards the applicability of the operation, let us briefly consider a few of the objections which have been brought against tracheotomy.

First, it is urged that the small amount of success which has hitherto attended the actual performance of this operation in croup renders it an expedient to which it is scarcely justifiable to have recourse. In answer to this objection, if we refer to the statistical inquiries of different countries, we shall find that it has no foundation. Thus, in France, where tracheotomy has been resorted to in cases of croup on an extended scale, although the rate of mortality has on the whole averaged about seventy-six per cent. of the cases operated upon, yet in about 680 cases in which the operation was performed, the mortality only amounted to sixty-eight per cent.

According to M. André, during the year 1856, there were 54 operations of tracheotomy for croup at the Children's Hospital in Paris. Out of these there were 39 deaths and 15 recoveries, or over 27 per cent.

The proportion of recoveries obtained by M. Guersant in a very considerable number of operations during the last three or four years was about one-third.

In a summary drawn up by M. Bouchut, he says :—

“Although the success of tracheotomy is not very striking, yet the results are such as ought to encourage its adoption. Thus M. Bretonneau performed the operation in 20 cases, and out of these 6 were successful. In my own practice, 160 operated upon, 5 saved. M. Velpeau saved 2 in 10. M. Petit, 6 operated upon, of which 3 were successful. Thus out of 176 cases, we have 16 which terminated favourably.”

M. Chaillou, in the *Journal of Practical Medicine and Surgery*, gives the following statistics as regards the operation in cases of confirmed croup. In eight years, 380 operations of tracheotomy were performed, of which 86 were successful. The proportion of cures remained about the same, 1 in 4 or 5, an encouraging result when by far the greatest number of patients were operated upon in the last stages.

The statistics of tracheotomy at the Hôpital des Enfants in 1855 showed ten cures and thirty-eight deaths, out of forty-eight cases, or one patient saved in five. Since this period this ratio has very much improved, owing to a more extended experience in the mode of performing the operation, and in the necessary after treatment.

In Great Britain the recorded results of the operation afford a fair amount of success. In 22 recorded cases in 1857, no less than eight terminated satisfactorily.

Dr. Fuller, above cited, reports five cases of croup for which tracheotomy was performed, in two of which life was saved. The results of the operation in England are, for some undetermined cause, far less favourable than those which have been obtained in France or in this country.¹

The most recent statement of the results of tracheotomy in France, is that of MM. Roger and Sée,² which yields 126 recoveries to 446 operations, or 27 per cent. during the last seven years.

A much larger amount of statistics might be added to those which we have brought forward, but sufficient have been offered to prove the propriety of tracheotomy in this disease under certain circumstances.

It has also been objected that the operation was a very difficult one, and that in itself it was a very dangerous one. In answer to the first of these objections, we will say that, in the case of young children, it is often a difficult proceeding, and requires a greater amount of operative skill and care than is commonly supposed. These circumstances, however, should scarcely be held to militate against our having recourse to the operation when the necessity of the case demands it.

But that tracheotomy is in itself a very dangerous operation, the tendency of the evidence on this point goes to disprove. Thus, M. Trousseau³ has collected the records of ninety-six cases, in which tracheotomy was performed for the removal of foreign bodies in the windpipe, and in seventy-three of these a complete cure was effected, the rate of mortality after the operation thus amounting to about twenty-four per cent. of the cases operated upon.

Dr. Gross, in his *Treatise on Foreign Bodies in the Air-passages*, has collected the particulars of 176 cases in which foreign bodies had accidentally gained entrance into the air-passages. In 68 of these, tracheotomy was performed, and the mortality reached only 11 per cent.

To be sure, we must take into consideration, when making a comparison between the results of tracheotomy when performed for the removal of foreign bodies, and those of this operation for the relief of croup, that, in the former case, the tissues operated upon are generally healthy, whereas, in the latter case, they are the seat of certain morbid changes. Yet notwithstanding this, there is not sufficient danger in the operation itself, under any circumstances, to deter us from performing it.

Dr. West, after speaking of the more favourable results obtained in France than in England as regards this operation, owing, as he thinks, to its performance often in the former country when other means might have been tried which would probably have controlled the disease, says:—

“Still, if these facts detract something from the apparent value of the operation, they at least show that in itself it is not attended by serious danger; and recent statistics prove that, in as far at least as the diphtheritic form of croup is concerned, there is no sort of connection between an increased frequency in the performance of tracheotomy and a higher mortality from the disease.”⁴

The gravest objection which is brought against the operation, is, that it is apt to induce severe bronchitis, or at least to greatly aggravate any previously existing inflammation of the lungs or bronchial tubes. In answer, we say, that, although there is some ground for this accusation, on the other hand, it must be remembered that these very inflammatory conditions are the almost invariable complications of croup, however treated, and that

¹ See Appendix D.

² Gazette Hebdom., Nov. 1858.

³ “Discussion at French Academy,” by M. Trousseau.

⁴ Lectures on Diseases of Childhood, &c., 1859.

they do not ordinarily follow tracheotomy when resorted to in other circumstances, as for the removal of foreign bodies, for acute laryngitis, or for œdema of the glottis.

Other objections still have been brought against the propriety of tracheotomy in cases of croup and diphtheria, which we could satisfactorily answer did space permit. We can only add, in conclusion, that there do not appear to us to be any evils attendant upon the operation which counterpoise the indisputable benefits to be derived from it.

A few words upon the proper period for performing the operation. Tracheotomy has been, and is still considered by a great portion of the profession, especially in this country and in Great Britain, as the very last resort. Within the last few years, however, the opinion of those best able to form a judgment has materially changed. A middle period should be selected for the operation. We should not wait until the case is desperate, the patient in a complete state of prostration, in fact moribund; nor, on the other hand, should we attempt the operation too early, before other remedies had been fairly and completely tested. But we are to resort to the operation "so soon as ever we feel that our remedies are too tardy to overtake the disease."

There are some circumstances relating to the proper management of the operation and to the after-treatment, which greatly influence the results of tracheotomy, at which we must hastily glance. The first of these concerns the size of the tracheal tube, the importance of which was first insisted upon by M. Trousseau. This gentleman explains the occasional sudden and apparently causeless disappearance of the amendment which at first follows the operation by the inadequate size of the canula, which is frequently employed, and which does not provide for the constant and permanent admission of a sufficient quantity of air. In illustration of this fact, M. Trousseau says:—

"Take a quill, and closing your nostrils, endeavour to breathe entirely through it; at first you breathe easily enough, but soon your respiration becomes laborious, and at length you are fain to throw away the quill, and with open mouth once more to fill your lungs completely. Now precisely this is what happens when an opening of inadequate size is made into the trachea, air enters readily, and without the interruption which the spasm of the glottis occasioned; but it does not enter in sufficient quantity, and hence the return of the symptoms and the patient's death."

Acting on this principle, M. Trousseau makes a larger opening into the trachea, and introduces a larger canula than was formerly used; and this practice is now gaining ground, especially with us in the United States.

Another necessary precaution has reference to the necessity of insuring to the patient after the operation, a warm moist atmosphere, which may be easily effected by filling the room with steam from some simple apparatus; and to keeping the room at a fixed temperature, and, though well ventilated, free from all draughts. The neck also should be surrounded with several folds of muslin, so as to cover the orifice of the tube. Great care should also be taken to keep the canula free, and as upon this one thing the whole result of the operation may depend, it should not be intrusted to unskilful hands, but to a medical student, or to some competent person, upon whom full reliance can be placed. This is a point which has not attracted the attention which it deserves, for not unfrequently cases occur where death suddenly takes place from the stoppage of the tube, the persons in charge fearing to do what the occasion of the moment demands.

Medical treatment must not be suspended after the operation. The same measures which were considered useful before the operation must be steadily persevered in. Great stress has been laid by a few writers in our country upon the importance of throwing nitrate of silver injections into the trachea. These we certainly advocate, as cases have come under our observation where very beneficial results have followed their employment.

The period at which the canula ought to be removed is also an important point, on account of the irritation of the edges of the wound which its long-continued presence is apt to produce. M. André, in his statistics before alluded to, has endeavoured to ascertain the proper period for this purpose. His observations were made in 17 cases. In 1 it was taken out on the fourth day; in 5 on the sixth day; in 2 on the seventh; in 3 on the eighth; in 1 on the eleventh; 1 on the thirteenth; 1 on the fourteenth, and 1 after the fourteenth. From the fourth to the fifth is the time recommended by M. André. After the removal of the canula the wound is to be covered with a bit of gauze, and the edges touched daily with the nitrate of silver, and dressed with a little spermaceti or other ointment. The tube is to be replaced if dyspnœa recur.

In addition to what has been already said on the treatment of diphtheria, it may not be inappropriate to give a summary of the treatment recommended by some of the principal practitioners in Europe.

Mr. Ranking, in his lectures on diphtheria (*Lancet*, January, 1859), recommends the tinct. ferri chloridi, 10 to 15 drops every three or four hours, and the same to be applied locally with nourishing diet.

Mr. Hart, of the "*Lancet* Sanitary Committee," advises a tonic treatment, tinct. ferri chloridi, chlorate of potash, &c.; and as local treatment, the nitrate of silver, 30 to 60 grs. to the ounce, or the muriatic acid.

Dr. Kingsford (*Lancet*, Nov. 1858), in simple diphtheria, uses a calomel purge in the commencement. Then chlorate of potash, with dilute hydrochloric acid in a decoction of bark, and mopping the throat two or three times a day with the compound solution of alum.

In the severe forms he uses wine and nutritious diet freely, and gives tinct. ferri chloridi, with chlorate of potash, 10 to 30 drops of the former with 10 to 30 grs. of the latter, every two or three hours according to circumstances. Nitrate of silver to the throat—wine and nutritious diet freely. If much difficulty of deglutition, enemata of strong beef-tea, and port-wine every two hours, the quantity to be injected not to exceed two or three ounces at a time. Mercury, he thinks, contraindicated except as a cachectic at the beginning.

Dr. Perry, of Kent (*Med. Times and Gaz.*, March, 1859), gives oil of turpentine, ten drops every second hour, to a child of from two to six years of age, and alternates this with five grains of carbonate of ammonia every two hours. Besides this, the child takes port-wine, porter, and beef-tea, or wine with the yolk of an egg, *ad libitum*. He thinks that mercury hastens the fatal result.

Dr. Cammack (*Lancet*, Oct., 1858) gives a calomel purgative where symptoms of laryngitis appear, and a decoction of cinchona with hydrochloric acid. A gargle of salt and vinegar for the mouth and throat, which he also injects up the nostrils when they become affected. He uses the solid nitrate of silver to the exudation. He is convinced that the malady is herpetic.

The editor of the *Lancet* (October, 1858) thinks the disease is not a new one, but believes it to be a form of scarlet fever. He gives ammonia and beef-tea early, and keeps the skin softened by the steamed blanket. A warm

blanket wrung from hot water is to be put around the patient, and this to be enveloped in dry blankets, and the patient to be sweated for an hour and rubbed rapidly dry, and again covered with dry blankets. "Keep him up with ammonia and good nutritious broths."

Mr. Thompson (*Brit. Med. Journal*, June, 1858) advises thorough applications of nitrate of silver to the throat, a stimulating gargle of nitrate of potash, and capsicum or solution of chlorinated soda. Mild but continued counter-irritation over the upper part of the chest appeared of great service. General treatment he thinks of little use. Stimulants were often required in the early stage.

M. Roche (*L'Union Médicale*, July 26, 1859) places great reliance on the following treatment. Having first freely cauterized the false membrane with lunar caustic, he injects every hour against the fauces a solution of common salt not of sufficient strength to create nausea. The tincture of iodine he also employs as a topical application.

Mr. Ramskill (*Lancet*, February, 1859) makes use of an infusion of chamomile to wash and syringe out the throat and nares of children, to which he adds a few drops of creasote, or of the liquor calcis chlorinatæ. Internally he gives chamomile with muriatic acid and ether and quinine, at the same time good nourishment and stimulants.

Dr. West, in his *Treatise on Diseases of Childhood*, adopts this mode of treatment. A drachm of the nitrate of silver to the ounce of water is applied to the throat either by means of brush or probang. If necessary afterwards, the strong hydrochloric acid diluted with from four to ten parts of honey. One or, at the most, two applications of the stronger caustics in the twenty-four hours suffice. These are preferable to the weaker ones, as these latter must be applied frequently, which distresses the child. For this reason he does not make use of the tincture of iron, nor does he employ insufflations of powdered alum or calomel. The mouth may be kept free from the secretions which are apt to accumulate in it by syringing it every three or four hours with a lotion of the chloride of soda, half an ounce to six ounces of water. As constitutional treatment, he advises quinine with the tincture of bark and hydrochloric acid at short intervals. The best of nourishment and stimulants.

Dr. Semple (*Lancet*, October, 1858) says that the best treatment is the application of strong caustics, of which the concentrated hydrochloric was the best, at the very earliest possible period. Nourishing diet, &c.

M. Empis (*Arch. Gén. de Médecine*) advocates the use of local remedies, such as the hydrochloric acid and the nitrate of silver, and for constitutional treatment, tonics and nutritious food. In these views M. Isambert also concurs.

In our own country, during the epidemic which prevailed at Albany in 1858, gargles containing chlorates of potash and soda, or vinegar, the mineral acids and tonics internally, constituted the principal treatment. During the epidemics at San Francisco, very much the same treatment was pursued. So also in Connecticut.

Although there is some diversity in the treatment of diphtheria as laid down by different authorities, still, it will be seen that the affection is considered by all as one decidedly adynamic in its character, and that consequently a supporting treatment is necessary, and all depletory measures are to be strictly avoided. Authorities are also united upon the necessity of a more or less energetic local treatment, particularly in the early stages of the disease.

Before closing our remarks upon the treatment of diphtheria, we must say a few words upon *tubing of the glottis*.

The unquestionable efficacy of tracheotomy under certain circumstances suggested the idea of inserting into the larynx through the mouth an instrument which might replace the canula of tracheotomy and render unnecessary the use of the knife. Although others had tried the experiment, M. Bouchut is the first who put it to practical use.

The operation consists in inserting into the larynx a metallic tube, which is to be retained for a longer or shorter time according to circumstances.¹ The process is quite a recent one, and has not as yet been attended with any very great success. Bouchut reports seven cases, five of which terminated fatally, and the other two underwent tracheotomy. At a meeting of the French Academy of Medicine, a committee was appointed to examine M. Bouchut's communication on this subject; and of this committee M. Trousseau was appointed chairman. The report, which is of some length, concludes with the following resolutions:—

"1. Tubing the larynx in certain forms of acute laryngitis may, by delaying asphyxia, become a remedial agent.

"2. In certain chronic affections of the same organ, tubing may permit tracheotomy to be postponed, and may occasionally give time to treat and cure the disease.

"3. In the treatment of croup, tubing retards asphyxia, and affords a more easy mode of introduction into the air-passages of remedies calculated to modify diphtheritic inflammation.

"4. It cannot, however, supply the place of tracheotomy, which to this day remains the only expedient in croup when the resources of medicine seem to have been exhausted."

Résumé.—In the preceding pages, we first gave Bretonneau's description of diphtheria. We then remarked that it was only by a comparison of the various epidemics of "sore throat" which had prevailed at intervals in various parts of the world, that we could ascertain how far his description was to be taken as a model of the disease. Accordingly we took up the history of the epidemics from remote ages to the present day.

Having given an account of those which had prevailed in various parts of Europe, and having compared the descriptions of various writers upon these epidemics, with that of Bretonneau, we showed that he was incorrect in denying the presence of all constitutional disturbance, as also in insisting upon the absence of all relation between diphtheria and gangrene of the fauces—both of these conditions having been frequently observed, particularly during the epidemics of late years.

We next observed that Bretonneau's idea of croup, which he associates with diphtheria, does not conform to our ideas of that disease, founded as they are upon the description given by Dr. Home. The distinctions between diphtheria and croup were dwelt upon, as also the non-identity of diphtheria and scarlatina.

In order, we took up the history of the disease in England. A comparison of the descriptions of the disease by various writers, as it appeared in the several counties, gave no marked uniformity, and but little correspondence with Bretonneau's model.

Diphtheria in America was then considered, and we gave at some length a description of an epidemic of "sore throat," by Dr. Bard, also an account of the epidemics in California and other parts of the Union.

We next remarked that all these epidemics of "sore throat" were con-

¹ See Appendix E.

nected by a bond of union, to be found in the pathological anatomy of the disease, which consists in the peculiar exudation. That although Bretonneau fully recognized this fact, his description was deficient, hence we subjoined that of MM. Barthez and Rilliet, as being more comprehensive. We also considered the disease as existing under two forms, the mild and severe.

Certain points as respects the nature of the disease were taken up in order. First, the characteristics of the false membrane, its physical appearances, its seat, the experiments of Bretonneau in order to ascertain the specific nature of the diphtheritic membrane, its microscopic appearances, and its dependence upon certain parasites were discussed.

Next, in answer to the question, Is diphtheria infectious? Having given the arguments of various authors, we replied, that although we were ignorant of the exact laws which governed these epidemics, we *could* find certain hygienic or individual circumstances which undoubtedly had their effect, either as direct or as predisposing causes.

The presence of albumen in the urine and its signification were commented upon. We remarked that further observation was necessary before we could ascribe to it any settled prognostic value. We spoke of the singular after-effects of the disease, as shown especially upon the nervous system. We gave the observations of MM. Trousseau and Faure, upon this point.

In our account of the *treatment* of diphtheria, we said that it was only within the last few years that anything like unanimity had prevailed. That it was now universally regarded as an asthenic disease, and consequently would bear no depletory measures, but, on the contrary, required tonics, stimulants, and a nourishing diet, even in the early stages. Blisters, leeches, and local bleeding of any sort should be prohibited.

The tonics best suited, we enumerated. Of the auxiliary measures, we first spoke of the local applications to the fauces, their utility and propriety, the various agents which had been employed and the mode of use. The ablation of the tonsils recommended by Bouchut, we conceived to be inadmissible, excepting under rare circumstances.

Tracheotomy we discussed at considerable length. Remarking that the two diseases, inflammatory croup and diphtheria, were on an equal footing as regards the applicability of the operation, we answered the various objections which had been brought against it—the small amount of success; the difficulties of performing it; the tendency to the production of bronchitis, &c.

We considered that the proper time for performing the operation was an intermediate period. We gave some necessary rules as to the size of the canula, the state of the surrounding atmosphere, the importance of having some competent person on hand in case of emergency, the propriety of keeping up the medical treatment, and the time for removing the canula.

Having given a summary of the treatment recommended by some of the leading men in Europe, we concluded by a brief consideration of the operation for “tubing the larynx.”

APPENDIX A.—*Clinical Lecture on Diphtheria as caused by the Oidium Albicans in a case of Cancer of the Supra-renal Capsules.* By T. LAYCOCK, M. D.

James D——, aged 35, married—admitted into the infirmary March 19—stated that until two years ago, his health was good. About that time he had diarrhœa with frequent desire to go to stool; and much straining at stool without result. A few weeks afterwards had shiverings and sweatings, and a peculiar feeling of numbness, with loss of sensibility in upper and lower extremities. The arms would become stiff. At present, the attacks of stiffness come on only when his hands are placed behind the back. Continued at work until eight days ago.

On examination it was found that he slept well, swallowed easily, had no pain after eating, but was flatulent. Bowels regular, motions solid. Abdomen large and tumid. Urine of spec. grav. 100.5, no albumen, no sugar—amount seventy ounces per diem. Under the microscope, the blood was seen to contain colourless corpuscles in slightly increased quantity. Lungs healthy, no cough or expectoration. His skin under the clothing was pale; the inner surface of the lips pallid, the face unusually brown, but evidently from atmospheric pressure. In three weeks after admission, the bowels became relaxed, and by April 13 an obstinate diarrhœa had set in, which resisted all the usual remedies. On the 15th he complained of sore throat, and on examination the fauces were seen to be deeply congested, and covered with white spots. The tongue had also white patches upon it. He still complained of the hyperæsthetic sensations in his arms, and was hopeless as to his recovery. On April 23, pulse 120, deglutition difficult, with a constant burning pain in the throat. On the 24th the pharynx was seen to be covered with a thick yellowish pellicle, and the surface beneath, when it was detached, was raw and bleeding. The pellicle, when a fragment was placed under the microscope, was found to consist of the mycelium and sporules of the *oidium albicans*, with epithelium and pus cells. He was ordered the aqua chlorinata, and a solution to the fauces of nitrate of silver. . . . The patient gradually sank until the morning of the 11th inst. when he died.

Autopsy.—On removing the tongue, trachea and œsophagus, it was found that a soft yellowish-white pultaceous matter was adherent to the mucous membrane of the tongue, pharynx and œsophagus. This occurred in some places as a continuous layer, in other places as patches. It could be readily scraped off, when the mucous membrane was found to present a somewhat raw appearance. It was most abundant in the pharynx over the back of the larynx. The matter extended down the œsophagus to within two inches of the stomach. On examining microscopically the matter found on the mucous membrane, it was seen to consist of the branching filaments and sporules of the *oidium albicans*, mixed with large quantities of somewhat altered epithelial scales. The larynx and trachea were quite natural. . . . The mesentery was found to be converted into a large cancerous mass; the lumbar glands and supra-renal capsules were also implicated in the same disease.

Comment by Dr. Laycock.—The immediate cause of death was the exhausting diarrhœa. Now this supervened coincidentally with an attack of diphtheria. At the onset of the disease, and just at the period before death, we found in the pellicle formed on the tongue and fauces the sporules and mycelium of the *oidium albicans*, a parasitic fungus found also in muguet, the epidemic aphtha or diphtheria of infants in France. This is an interesting fact at the present moment, when diphtherite is prevailing, more especially as the pellicle was also found abundantly after death in the œsophagus. I have little doubt that this pellicle was due to the action of the parasite on the enfeebled mucous surface of the mouth, fauces, &c. It acts like all its tribe, as an irritant, inducing increased formation of epithelial scales, and effusion of mucous exudation, corpuscles or plasma; intermingled among these are the sporules, and the mycelium of the microscopic fungus; the whole constitutes a pellicle or membrane, varying in thickness. (Fig. 1.) The parasite seems to act upon the capillaries of the subjacent tissue, as when removed blood is not uncommonly effused, and the surface looks raw. Diphtheria is not, however, limited to one form of disease.

. . . . If the fungus multiply in a population at the same time that there is an epidemic of scarlatina or rubeola prevalent therein, that epidemic may be expected to take the diphtheritic form in those cases which are attacked by the *oidium*. I must add, however, that we have had reasons for thinking that the *oidium*, acting alone will fasten upon the mucous membrane of the mouth and throat, and excite inflammation and without the formation of a pellicle The diagnosis of diphtheritic *oidium* from ordinary aphtha is founded, first on the character of the morbid appearance, for, in ordinary aphtha, the disease is vesicular, and the white specks or patches are ulcers, white in diphtheria, they are pellicular, and not ulcerative, while the redness is much deeper than in aphtha. Besides the microscope may reveal the spores and mycelium of the fungus. The development of the mycelium is, however, by no means a necessary result of the action of the fungus. This seems to be peculiar to the more

advanced stages; at first there is not even a pellicle, only characteristic redness of the affected surface. . . . Further, it is probable that besides the stage of development the condition of the *habitat* may make a considerable difference as to the morbid products. . . . How great a share these microscopic parasitic organisms have in the causation of disease remains yet to be ascertained.

Fig. 1.



The sporules and the mycelium of the oïdium. After Robin.

APPENDIX B.—In answer to remarks made by Dr. Rogers, that he not only thinks diphtheria to be a blood disease, but that, *as such*, it cannot be a parasitic disease, Dr. Laycock says (*Lancet*, Jan. 22 and 29, 1859): “Comparative pathology teaches, however, that this conclusion is at least doubtful. The muscardine (an epizootic disease of the silkworm) is due to a species of fungus like that which infects the potato called, after its discoverer, the *Botrytis bassiana*, and the sporules are described as being reproduced in the blood of the insect when it becomes acid; while the filaments and mycelium appear on the respiratory surfaces, that is at the outlets of the tracheal tubes.

“Again, the fungus of the common house fly (*Mycophyton Cohnii*) is a mould or oïdium found in the blood, abdomen, and sometimes in the intestines of the insect at beginning of autumn. Its first symptom observed, is a milky appearance of the blood. It is found in the blood in all stages of development, from the simple minute spore or cell, to the full grown mycelium. It is found in like manner in the fluids of the intestines, and appears externally as a mould. Flies thus affected may be often seen sticking with outstretched wings to the window panes at the end of the summer and beginning of autumn. These are by no means solitary instances of parasitic blood disease. Indeed, hamatophyta, as Lebert terms these microscopic blood parasites, infest the blood of several classes of insects. The same facts also hold good as to the vegetable parasites. . . . These are facts which ought to make us hesitate, at least in coming to the conclusion, in the absence of all inquiry, that a parasitic disease cannot be a blood disease in man.

... "That these parasites are sometimes powerful irritants of the lining tissues, is, I think, fully established both from the history of muguet and other circumstances, and although French writers speak of *pseudo-diphtherite*, the accuracy of the term may be questioned, for the exudation appears externally on ulcerated or exposed surfaces, as well as internally, in both muguet and diphtheria alike. An interesting case of vaginal blennorrhœa, due probably to *oidium albicans* introduced from without, may be found in *Archiv für Physiologie*, vol. ix. p. 466. The labia were swollen, the vagina of a bright-red, studded with enlarged papillæ, and covered with star-like patches of membrane, like those of the mouth in muguet, which were found to contain the *O. albicans*. The patient in the next bed had subsequently active fever, abdominal tenderness, *O. albicans* of the mouth with muguet.

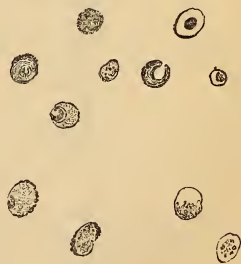
"It is usual to speak of the characteristic pellicle as if it were peculiar to diphtheria, but this is by no means the case. It is not unfrequently seen in cases of typhus and relapsing fever, sometimes in yellow fever, and I believe in all fevers. A series of carefully conducted experiments, made with a thorough knowledge of cryptogamic botany on lower animals so as to show the real pathological origin and the effects of these parasitic fungi, would be very valuable. . . . I am inclined to think that it would probably be shown that these parasites may act either through the blood or locally only.

"I may observe, in conclusion, that antiseptics and parasiticides appear to be the most efficient remedies in diphtheria. I can speak very favourably of the tinct. of the sesquichloride of iron (an antiseptic and hydrochlorate of potass)."

APPENDIX C.—"With the view of determining how far the act of fibrinous concretion may be considered a consequence of the anatomical or physiological peculiarities of the structure affected, independently of the constitutional state, or of the morbid condition of the blood arising from the disease, I made the following experiments: I injected into the air-passages of several dogs, small quantities of a solution of cantharides in olive oil, and examined the consequent alterations of the mucous membrane after various periods. Two hours after the introduction of the solution, it was found that the mucous surface of the larynx was scattered over with patches scarcely perceptibly redder than the surrounding membrane, and that that structure was covered co-extensively with these patches, with a gelatinous covering of tolerably firm concretion, differing from that of diphtheria only in its greater transparency—a character, probably, mainly attributable to the absence of lamination—arising from its simultaneous formation. This concretion possesses a structure, which is identical with that of the early condition of diphtheria, consisting of a fibrinous matrix or substratum, in some parts of which cells are embedded. The substratum appears on microscopic examination to be transparent and faintly granular, but sometimes exhibits indistinctly the characters of fibrillation. . . . The cell wall is of extreme delicacy, and incloses a spheroidal nucleus, distinguishable without the addition of acetic acid. (Fig. 2.) On the addition of that re-agent, the former becomes distended but does not disappear, while the latter either assumes the form of a spheroidal highly refractive body or is resolved into the double, triple, or horse-shoe shaped forms often described as characteristic of the pus-corpuscle. On examining the mucous membrane subjacent to the concretion, it was found to have lost its columnar or ciliated epithelium, but the cells of the subjacent epithelial layer existed in an unaltered condition. They differed so completely in appearance, size, and structure, from the exudation cells, that there was no difficulty whatever in distinguishing them. In two days the process of transformation of the substance of the concretion into fibrous tissue had commenced. The pellicle possessed great firmness and elasticity, and could be stripped off the affected patches with ease to any extent.

"Of these facts, I will not further comment than to observe that the only important difference between the cantharidic and the diphtheritic concretion, con-

Fig. 2.



After Sanderson.

sists in the absence of any tendency in the latter to transformation into permanent tissue, as contrasted with the early period, at which that process commences in the former. So far as concerns this mere fact of fibrinous concretion, we are perhaps entitled to infer that it indicates nothing more than the intensity of the process of exudation; and that in diphtheria, the subsequent changes are prevented or retarded either by a morbid modification of the fibrin itself, or by an abnormal condition of the adjacent living structures." (Sanderson on Diphtheritic Sore-throat. *Brit. and For. Med.-Chir. Review*, Jan. 1860, pp. 181-9.)

APPENDIX D.—The following are the results of tracheotomy for croup by Dr. Geo. H. Gay, of Boston, Mass., communicated to us:—

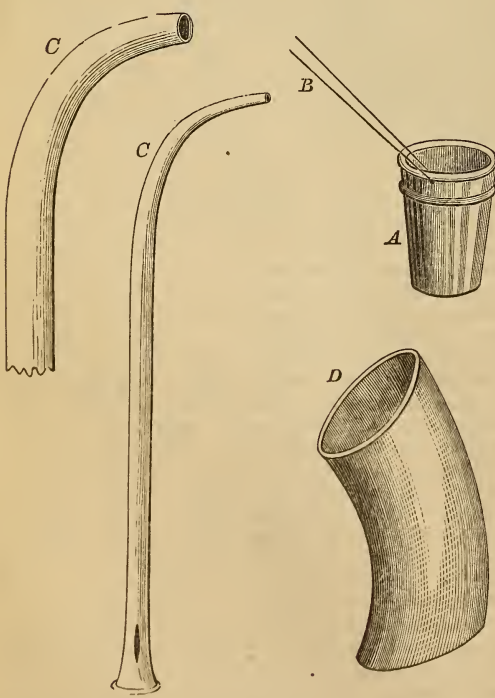
RECOVERIES.			
Jane,	æt. 20 years.		
L. F.,	" 8 "		
C. W.,	" 7 "		
M. L.,	" 11 "		
L. S.,	" 4 "	6 months.	
N. T.,	" 5 "	8 "	
L. B.,	" 3 "	5 "	
3 males, 4 females.			

DEATHS.			
T. W.,	æt. 9 years.		
W.,	" 3 "	11 months.	
W. H.,	" 5 "		
W. C.,	" 3 "	1 month.	
T. C.,	" 2 "	6 months.	
F. S.,	" 1 year 7 months.		

Total number, 13. Males, 8. Females, 5.
Result . . { Recoveries . . . Males 3 Females 4
 { Deaths . . . " 5 Female 1
Recoveries, 7. Deaths, 6.

APPENDIX E.—The instruments used by M. Bouchut in his operation

Fig. 3.



for "tubing the larynx" are: 1. Curved male catheters of different sizes, open at both ends, and intended to penetrate into the larynx (*C, C*). 2. Straight cylindrical silver rings (*A*) of from $\frac{1}{2}$ to $\frac{3}{4}$ of an inch long, provided at their extremities with two ridges at the distance of a quarter of an inch and pierced with a hole for the passage of a silk thread (*B*), the function of which is to preserve a hold upon the ring from without. 3. A ring (*D*) to protect the forefinger.

HARVARD UNIVERSITY.

SUMMER SESSION OF THE MEDICAL DEPARTMENT.

THE Annual Course of Summer Instruction in the Medical Department of Harvard University will commence at the Massachusetts Medical College, in North Grove Street, Boston, on Monday, March 11, 1861, and continue till November.

Clinical Medical and Surgical Instruction will be given at the Massachusetts General Hospital, adjoining the College.

Recitations from approved text-books will be held daily during the session at the College, upon all branches necessary to a medical education. Occasional lectures are also given, and demonstrations, illustrated by the Museums of the College.

During the Summer Session, instruction is given by lectures at Cambridge, on Botany, by Prof. Gray; on Comparative Anatomy, by Prof. Wyman; on Zoology, by Prof. Agassiz; on Acoustics and Optics, by Prof. Lovering. To these lectures, students of the Summer Session will be admitted without extra charge.

Good Board can be obtained at \$3 00 to \$4 00 per week.

Fees for the Summer Term (which must be paid in advance), \$100, without extra charge for Matriculation, Hospital, Library or Dissections; for six months, \$100; for three months, \$50.

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Tickets to the Session must be procured before students will be admitted to the Course.

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Jan. 1, 1861.—2t

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FISKE MEDICAL PRIZE QUESTIONS.

The Trustees of the Fiske Fund, at the Annual Meeting of the Rhode Island Medical Society, held in Newport, July 11, 1860, announced that the premium of One Hundred Dollars, offered by them, in 1859, for the best dissertation on the following subjects:—

“DIPHTHERIA; ITS NATURE AND TREATMENT, WITH AN ACCOUNT OF THE HISTORY OF ITS PREVALENCE IN DIFFERENT COUNTRIES,” had been awarded to the author of the dissertation bearing the motto—

“Felix qui potuit rerum cognoscere causas.”

And upon breaking the seal of the accompanying packet, they learned that the successful competitor was Daniel Denison Slade, M. D., of Boston, Massachusetts.

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“THE MORBID EFFECTS OF RETENTION IN THE BLOOD OF THE ELEMENTS OF THE URINARY SECRETION,” had been awarded to the author of the dissertation bearing the motto—

“Prius cognoscere dein sanare.”

And upon breaking the seal of the accompanying packet, they learned that the suc-

FISKE MEDICAL PRIZE QUESTIONS.—CONTINUED.

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Two dissertations, one on each subject, the first bearing the motto—

"*Pallida mors æquo pulsat pede pauperum tabernas regumque turres.*"

The second bearing the motto—

"*Il est toujours téméraire d'attaquer des expériences par des raisonnemens.*"

Were of a very high order of merit, and if published would be very interesting to the profession.

They propose the following subjects for 1861:—

1. ANEURISM; ITS VARIETIES AND THEIR APPROPRIATE TREATMENT.
2. OZONE; ITS RELATIONS TO HEALTH AND DISEASE.

For the best dissertation on either of these subjects the Trustees will pay the sum of One Hundred Dollars.

Every competitor for a premium is expected to conform to the following regulations, viz:—

To forward to the Secretary of the Fiske Fund Trustees, on or before the first day of May, 1861, free of all expense, a copy of his dissertation, with a motto written thereupon, and also accompanying a sealed packet, having the same motto inscribed upon the outside, and his name and place of residence within.

Previously to receiving the premium awarded, the author of the successful dissertation must transfer to the Trustees all his right, title and interest in and to the same, for the use, benefit and behoof of the Fiske Fund.

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THE Course preliminary to the Session of 1861 will begin on the 18th of February, and the *Regular Lectures* on the 18th of March, to continue till the middle of July.

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TO READERS AND CORRESPONDENTS.

We must express our obligations to many correspondents for valuable communications. Want of room has compelled us to postpone a number of these, but they shall receive early attention.

The following works have been received:—

Transactions of the Epidemiological Society of London. Vol. I., part I. London, 1860. (From the President and Council of the Society.)

Cours Théoriques et Cliniques de Pathologie interne et de Thérapie Médicale. Par E. GINTRAC, Professeur de Clinique interne et Directeur de l'Ecole de Médecine de Bordeaux, &c. &c. &c. Tomes IV. et V. Paris: Germer Baillière, 1859. (From the Author.)

Description of Cases communicated to the Pathological Society of London during the Session 1859-60. By Dr. JOHN OGLE, Ass. Phys. to St. George's Hospital.

Case of Recurrent Anæsthesia of almost the entire Surface of the Body. By JOHN W. OGLE, M. D., Ass. Phys. to St. George's Hospital. London, 1860.

A Treatise on Human Physiology; designed for the Use of Students and Practitioners of Medicine. By JOHN C. DALTON, JR., M. D. Second edition. Revised and enlarged. With two hundred and seventy-one illustrations. Philadelphia: Blanchard & Lea, 1861. (From the Publishers.)

A Treatise on Fever, or Selections from a Course of Lectures on Fever. Being part of a course of Theory and Practice of Medicine, delivered by ROBERT LYONS, K. C. C., Physician to Jervis St. Hospital, &c. &c. &c. Philadelphia: Blanchard & Lea, 1861. (From the Publishers.)

The Transactions of the American Medical Association. Instituted 1847. Vol. XIII. Philadelphia, 1860. (From the Association.)

Proceedings and Debates of the Fourth National Quarantine and Sanitary Convention, held in the city of Boston, June 14, 15 and 16, 1860. Reported for the City Council of Boston. Boston, 1860.

Proceedings of the Academy of Natural Sciences of Philadelphia, November and December, 1860. January and February, 1861.

Transactions of the Fifteenth Annual Meeting of the Ohio State Medical Society, held at Ohio White Sulphur Springs, June 12, 13 and 14, 1860. Columbus, 1860.

Proceedings of the Pathological Society of Philadelphia. Vol. I. Philadelphia: J. B. Lippincott & Co., 1860.

A Handbook of Hospital Practice, or Introduction to the Practical Study of Medicine at the Bedside. By ROBERT D. LYONS, K. C. C. &c., Physician to the Jervis Street Hospital, Dublin. New York: S. S. & W. Wood, 1861. (From the Publishers.)

On Diabetes and its Successful Treatment. By JOHN M. CAMPLIN, M. D. From the Second London Edition. New York: S. S. & W. Wood, 1861. (From the Publishers.)

On Diphtheria. By EDWARD HEADLAM GREENHOW, M. D., &c. New York: Baillière Brothers, 1861. (From the Publishers.)

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Theory and Practice of the Movement Cure; or the Treatment of Lateral Curvature of the Spine, &c., by the Swedish System of Localized Movements. By CHARLES FAYETTE TAYLOR, M. D. With illustrations. Philadelphia: Lindsay & Blakiston, 1861. (From the Publishers.)

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
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THE
AMERICAN JOURNAL
OF THE MEDICAL SCIENCES
FOR APRIL 1861.

ART. I.—*The Anterior Suspensory Apparatus for the Treatment of Fractures of the Lower Extremity.* By N. R. SMITH, M. D., Professor of Surgery in the University of Maryland.

IN the treatment of all fractures of the extremities, the general indication is to furnish mechanical support that shall supersede the office of the injured bone in maintaining the form and length of the limb, until union shall have been accomplished. The accomplishment of this object is attempted by the use of rigid materials, termed splints, formed of wood, iron, felt, gutta percha, or starched pasteboard and bandages.

There would be no difficulty in effecting the mechanical purpose in view, if we might treat the member as an insensible substance, capable of enduring pressure without the production of pain, sloughing, or ulceration; and if it were possible, at the same time, to maintain one unvarying posture of the trunk. But the living parts cannot long endure the pressure of any hard material, nor the continued constriction of a band; nor is it possible that the trunk can be kept in one unvarying posture.

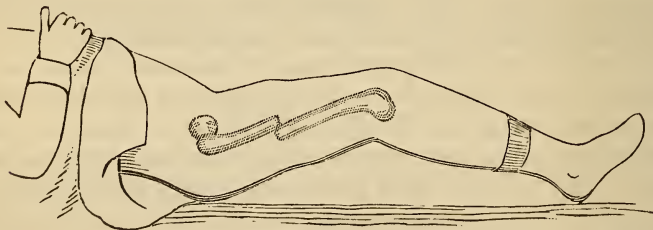
Another source of embarrassment arises from the difficulty of furnishing a well-adapted surface of support for the limb. This is not effected by supplying soft and yielding supports, such as pillows of down or feathers, or other soft bolsters, the name of which suggests comfort, but by no means furnishes it. As the member sinks into such supports, whatever part first touches the surface receives the greatest degree of pressure; and besides, such supports yield, and allow in the limb a change of posture in relation to the trunk. They are also otherwise objectionable, particularly in compound fractures, as causing too much heat, rendering it difficult to change dressings and to preserve cleanliness.

It is the duty of the surgeon to study carefully the physiological and
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mechanical causes of displacement, and to obviate them by posture and support, rather than by severe mechanical restraint.

There is a source of deformity and shortening, especially in fractures of the thigh, which has been by surgeons surprisingly overlooked. When the patient has been placed in the ordinary posture in bed, with a pillow and bolster under his head and shoulders, there is a constant tendency in the trunk to slide toward the foot of the bed. This tendency occurs in all cases in which the patient cannot exert his own muscular strength to correct his position. It occurs in typhoid fevers and other diseases inducing great debility, and manifestly must, and does, in fractures of the thigh and leg. The trunk also otherwise changes its position in relation to the limb. This is partly attributable to the yielding of the bed, and partly to the utter impossibility of maintaining the trunk in one immutable posture without the most intense suffering. The patient will instinctively change his posture in sleep, and will unconsciously do it when awake. If, then, the limb rests on the bed, and is embraced in a cumbrous apparatus, which also rests on the bed, it becomes incapable of obeying the motions of the body. The more firmly fixed the limb is in the apparatus, and the more immovable its posture, so much the worse in regard to the support of the fragments. The trunk *will* change its posture and disturb the upper fragment. The trunk controls the upper fragment, but does not carry with it the lower. When the trunk slides toward the foot of the bed the upper fragment is jammed upon the lower—made to overlap it, and to assume an angular

Fig. 1.



position in relation to it. This I am satisfied from the most careful observation is a much more fruitful source of shortening and deformity than is the contraction of the muscles, so constantly referred to by surgeons.

The objects to be attained, then, in the treatment of fractures of the lower extremity are: 1st. To furnish a surface of support which shall be accurately and permanently adapted to the surface and form of the limb which reposes in it. 2d. To so adapt the surface of support as that the limb, sinking into it, shall maintain its form by its own weight, without the necessity of lashings here and there, to secure it to the rigid portions of the apparatus. 3d. To make the limb obedient to all the unavoidable movements of the trunk, so that the upper fragment shall not be jammed

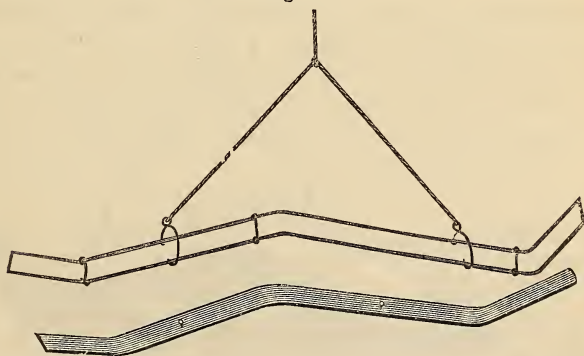
upon the lower, nor contorted in relation to it by the movements of the trunk. 4th. To obviate the contraction of the muscles by the employment of an extending force, uniform in its action, easily graduated, and not requiring the application of bands to the ankle. 5th. So to arrange the supports that in compound fractures we may have free access to the seat of injury without removing any of the essential supports of the limb. 6th. To effect these objects by an apparatus simple in construction, capable of being anywhere procured, easy of application, not requiring to be readjusted, and of but trifling expense.

That these objects are perfectly accomplished by the apparatus here presented I have demonstrated in the clearest manner in hospital and private practice during many years past.

The apparatus which I am about to describe is applicable to all fractures of the thigh and leg. I shall first describe it as employed in fractures of the thigh.

A single splint constitutes the whole of the rigid, or supporting, part of the apparatus. It may be made of wood or wire. The first employed by me was of wood. It was three inches broad, half an inch thick, and long enough to extend from above the spine of the ilium to the toes. It had an angle corresponding to that of the ankle, another at the knee more obtuse, allowing the leg to be very slightly flexed, and a third corresponding to the hip, still more obtuse, slightly flexing the thigh on the pelvis. Two staples by which to suspend it were attached to the upper surface, one a little above the knee and the other about the middle of the leg. It is represented in the plate. The splint may be sawed, in one piece, out of thick pine plank, or may be made of three pieces, united at the angles by nails or screws.

Fig. 2.

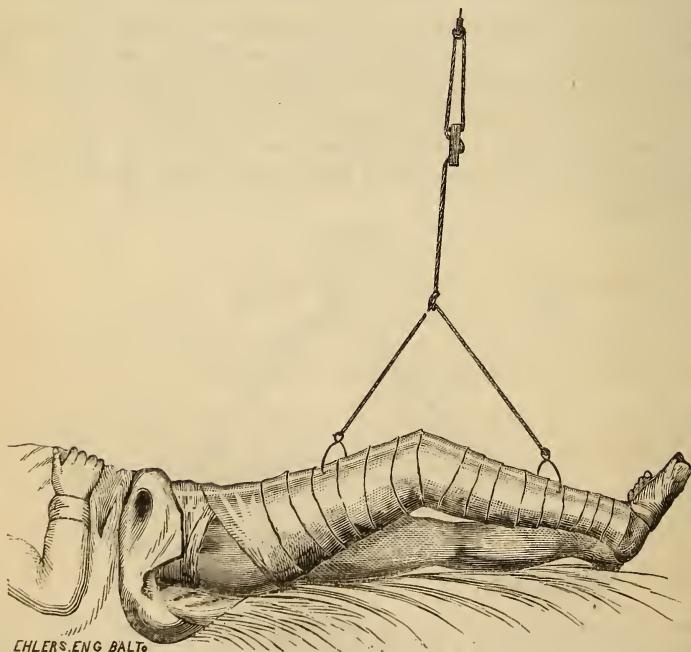


I now employ a splint of wire, as represented in the plate. The size of the wire should be that of a No. 10 bougie. If lighter than this it is apt to spring too much. It is constructed of one piece of wire bent twice at right angles at each extremity, in the form represented in the cut—that of a long parallelogram—being, however, three inches wide at its upper ex-

tremity and two and three-fourths at its lower. It must be long enough to extend from a point a little above the anterior spinous process of the ilium, to an inch beyond the toes when the leg, foot, and thigh are extended. Three feet eight inches will be sufficiently long for most adults. A little excess of length, above or below, is unimportant. The side-pieces are to be sustained by cross-pieces at distances of about eight inches, firmly clinched upon them. The wire frame is then to be bent by the surgeon to suit the case. The lower angle corresponds to the ankle, and is one of about 120° , to secure an easy posture of the foot. The angle at the knee is very obtuse—about 160° . The angle at the hip should be of about the same degree. The angle at the ankle should be about six inches from the extremity; that of the hip seven. The middle bend corresponds to the knee. The angles are easily made by bending the splint over the margin of a strong chair or table. It will often be necessary to vary these angles to suit particular fractures. The wire frame is now to be tightly wrapped with a muslin bandage, and it is ready for application.

The suspensory apparatus is simple and easy of application. A small iron pulley is to be screwed into the ceiling, over the bed of the patient,

Fig. 3.



perpendicularly over the middle of the shin, or nearly so. A cord, about as thick as the wire of the splint, passes over the pulley and is reeved through a small tent block, as seen in the cut, by which, slipping it upward

or downward, we elevate or depress the limb. The eccentric pressure prevents the weight of the limb from causing it to slip; if not, rub the cord with chalk. This single cord, which depends from the block, has a loop at its end, about two feet or more above the limb. Through this another cord, about five feet long, passes and hangs double from the loop, by its centre. Each end has a double hook attached to it, of the form represented in the cut. It is made of much smaller wire, has an expansion nearly equal to the breadth of the splint, and has the points sharpened and abruptly turned inward, the bends being quite angular, so as snugly to embrace the wire and not slip upon it. When everything is ready for suspension, these hooks are to be applied to the wire frame, piercing with them the muslin cover of the splint, at points to be presently indicated.

The application of the apparatus is extremely simple. The limb (we are now supposing a fracture of the thigh) is to be carefully adjusted and laid on pillows, still supported by the hands of assistants. The splint is now to be laid along the upper surface of the limb, the foot portion an inch remote from the instep, the middle angle nearly corresponding to the knee. I formerly placed a long narrow compress under the splint, but it is superfluous. One may, however, be placed on the instep, and one under the hip extremity of the splint. I now pass a piece of bandage, long enough to embrace the limb and overlap, under the foot and over the splint, so as to embrace both, not tightly. This band I besmear with starch. Formerly I used adhesive plasters. I pass another around the ankle, another beneath the knee, a fourth above the knee, and a fifth around the thigh near the hip. These bands are to be secured over the splint by pins. The starching of the bands obviates their slipping when traction is made on the splint, as presently to be described.

The limb being still kept extended by the hands of assistants, the hooks are now to be applied. It is very important to select properly their points of attachment, so as to adjust well the centre of gravity of the limb. In fractures of the middle of the thigh, I attach the upper double hook nearly over the seat of the fracture; the other a little above the middle of the leg. By pulling upon the tent-block the limb is now gently raised from the pillows and hangs suspended in the slings. Observe now whether the upper portion of the splint presses down too much on the thigh, or springs up too much from it. If the former occurs, then attach the upper hooks higher; if the latter, attach the lower ones lower. But this may be corrected, if slight, by slipping the cord, with the hooks attached, through the loop of the single cord, so as to correct the bearings, and securing it with a piece of twine just below the loop, to keep it thus.

The limb being now raised, so that one may pass his hand under every part of it, the application of the roller is made. The surgeon commences with the foot, the splint being held steady. Three or four turns are to be made loosely around the foot, care being taken not to press it upward

against the splint, which it must never touch; then figure of 8 turns are made around the ankle and foot, and the roller is carried upward along the leg and thigh, reversing, where necessary to make perfect adaptation, and great care being taken not to crowd the limb too firmly against the splint. On reaching the hip with the roller, a few spica turns are to be made obliquely through the groin and around the pelvis, concluding with two direct turns around the pelvis. I often besmear the whole under surface of the bandage, thus applied, with starch. This prevents the slipping of the bandage and preserves a uniform and accurately adapted surface of support.

The whole is now accomplished, except the application of the extending and counter-extending forces. This is in a moment effected by moving the bedstead of the patient head-ways (a word not found in Worcester), so far as to give a slight obliquity to the suspending cord, inclining toward the foot. It needs generally to deviate not more than one or two degrees from a perpendicular, but may be graduated to any degree of traction by simply moving the bed. Severe traction is not necessary, because the limb is supported in a manner to obviate the principal cause of retraction.

Counter-extension is made by the *vis inertia* of the body. To obviate the descent of the trunk toward the foot of the bed, caused by posture, and slightly promoted by the traction of the oblique cord, there may be a block, two inches thick, placed under each footpost of the bed. Under the upper portion of the thigh I usually place a small pillow, to obviate the lateral swinging motion which the unavoidable movements of the trunk may cause. The limb should be supported just high enough to lift the heel clear of the bed.

Let us now observe to what extent we have accomplished the indications which we proposed above.

1st. It will be seen that the member rests entirely on the turns of the bandage, and these are necessarily perfectly adapted to the form of the inferior surface of the limb. The limb cannot change its position in relation to this surface, even when the trunk moves and carries the member with it. So equally diffused is the pressure that the patient does not feel the contact of the support at any point.

2d. The apparatus being secured to the pelvis, the limb is obedient to all the movements of the trunk, and always preserves the same position in relation to it. Slung by the cord, it yields to the slightest impulse, and, therefore, the upper fragment cannot be disturbed in relation to the lower. If the body be inclined to the diagonal position, to right or left, this will not contort the upper fragment in relation to the lower, for the limb will instantly assume the corresponding position by the yielding of the cord, and the form of the limb will be preserved.

If the trunk descends towards the foot, the apparatus swings on the cord, and the upper fragment is not jammed upon the lower.

3d. To those who have not witnessed the action of the apparatus, it

would appear difficult, by its use alone, to obviate angular deformity; but in this I have never found the slightest difficulty. The loops of the bandage on which the limb rests, exert a lateral concentric pressure, corresponding in degree to the weight of the limb, and tending continually to straighten it, and resist distortion. The traction exerted by the oblique cord conduces to the same result. A force thus exerted upon any angular and yielding substance will of course render it straight. Lateral concave splints may, if deemed necessary, be interposed between the bandage and the limb; but, although I have used them, and found them very easy of adaptation, I am satisfied that they are unnecessary, and especially because there are now no forces which tend to displace the fragments.

4th. The mechanism of the extension is sufficiently obvious. It is exerted by the obliquity of the cord, and is easily graduated by varying it. It is perfectly uniform and gentle, and does not require the tightening or relaxation of bands from day to day. It needs not to be strong, because the tendency to shortening is otherwise in a great degree obviated.

The force is exerted on the apparatus, and by it transmitted to the limb through the embrace of the whole extent of the bandage, so that there is no galling pressure from distinct extending bands. The starching of the strings and the loops of the bandage, the more completely obviates the slipping of the bandage. The splint, however, inclines to be a little drawn toward the foot, and, therefore, as stated above, the foot portion ought to be applied an inch remote from the instep, to prevent pressure on that point where it cannot be endured without injury.

It will be observed that, in the method of treatment here described, the necessity for severe mechanical constraint is superseded by evading the causes of displacement, rather than by contending with them. The limb does not become distorted at the place of fracture by the movements of the trunk, because the member thus suspended always maintains the same relative position to it; nor are the muscles provoked to spasmodic and unequal action, because there is no unequal and galling pressure.

This apparatus, with slight modifications, is applicable to all fractures of the femur. To none is it more appropriate, and in none has it accomplished more satisfactory results than in fractures of the cervix, the events of which are so justly regarded as an opprobrium of surgery. So uniformly has non-union and deformity resulted, that eminent surgeons have denied that bony continuity is ever restored within the capsule. We hope to show that these results are rather the consequences of insufficient treatment than defect in the reparative power of nature. This power is embarrassed by greater difficulties than in other fractures, and requires more efficient aid. This being furnished, union will often be accomplished.

Many surgeons, in fractures of the cervix, reject all mechanical means in the shape of splints, because so uniformly inefficient in promoting union, and because producing so much mechanical irritation. But if we can

furnish a support which shall sustain the member more comfortably than the softest pillows, and allow the trunk a little liberty in regard to position, it would be expedient to employ it, though bony union were not expected to be effected.

In fractures of the cervix there is required no particular modification in the application, as described above. The pelvic portion of the splint is here very important, and it should extend well upon the trunk, and should be firmly secured to the pelvis by the oblique and direct turns of the bandage. The falling outward of the knee, and the same contortion of the foot, with shortening of the limb, are the characteristic deformities, and are of course to be obviated. The apparatus being slightly angular at the knee, the bandage effectually prevents the falling out of that portion of the member, and the bandaging of the foot to the foot-piece effectually secures it in a natural posture.

The extending apparatus is here extremely important, and the obliquity of the cord must be carefully maintained.

In the treatment of no fracture is it so important to preserve a uniform posture of the limb in relation to the trunk, and to identify the motions of the former with those of the latter. We have already explained the mechanism of the apparatus in these particulars.

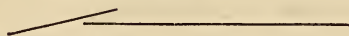
We shall present cases in which we have reason to believe bony union to have been accomplished. I would also direct the reader's attention to a case recorded by Prof. Geddings, of Charleston (see *The American Journal of the Medical Sciences* for Jan. 1847, p. 248), in which bony union was demonstrated as having been effected by the posterior apparatus introduced by me, and involving the same principles in regard to support and extension.

In the treatment of fractures through the base of the great trochanter (apt to be comminuted), the anterior splint is adequate to all the exigencies. Besides the general indications above described, the mechanism obviates a peculiar tendency to displacement incident to this fracture. The glutei muscles, acting as abductors, turn outward the upper fragment, and produce a salient angle externally. To obviate this tendency, the limb, as suspended, must be allowed to swing outward, by which the distorting muscles are relaxed, and their effect obviated. Indeed, the limb will spontaneously incline a little outward the moment it is raised, and the deformity will cease in consequence. It should be allowed thus to incline during the whole treatment, and any attempt to force it into a position parallel to the other member is mischievous, and the maintenance of this position is a serious objection to many forms of apparatus.

In fractures immediately below the lesser trochanter, the upper fragment, when the limb is allowed to fall into the horizontal position, is tilted forward by the action of the flexors of the femur—the psoas and iliacus muscles. The lower fragment, by the retraction of the muscles of the

femur, is drawn upward, and the limb shortened, as may be illustrated by these lines :—

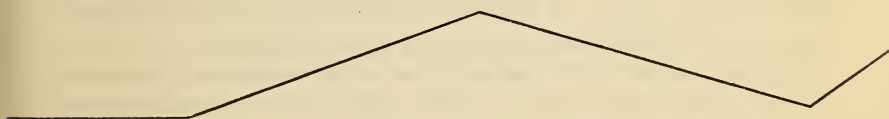
Fig. 4.



To force the upper short fragment backward, into continuity with the lower, is impossible, by any force which can be endured. Here is a case, then, in which we must humour the muscles by posture, and not endeavour to coerce, by which we should defeat our own object. We must therefore elevate the lower fragment till we bring it into line with the upper, elongating the limb at the same time by traction, and thus adjusting the fracture.

This position is easily rendered permanent, and the coaptation made secure, by applying the anterior splint with the angles at the knee and hip more acute than represented in the cut, thus allowing a greater degree of flexion at the hip and at the knee ; thus :—

Fig. 5.



The traction of the supporting cord will be exactly in the direction in which the upper fragment presents.

In fractures of the middle third of the femur I have observed, in almost all cases, the deformity to result from a salient angle of the fragments externally. I have examined many femurs of those who have been treated for this fracture, and almost always have found an external convexity of the thigh.

In these cases the abductor muscles, being inserted into the upper fragment, draw that portion of the bone outward, while the adductors, having their insertion continued to the lower extremity of the femur, draw the distal extremity of the lower fragment inward. When, in the use of any apparatus like Dessault's, the limb is approximated to, and made parallel with, the other member, the abductors are then rendered as tense as they can be, and the adductors placed in the opposite condition, and cause the drawing outward of the lower end of the upper fragment. Carry the whole limb outward, and the deformity disappears without the use of pressure.

To render the adjustment permanent, it is only necessary to suspend the limb in the apparatus—to allow it to swing outward, and thus to relax the abductor muscles. Indeed, the moment the limb is suspended it will instantly assume that position by the action of the muscles, they becoming relaxed by this movement, and ceasing to distort the fragment. Here is

another instance in which we succeed by humoring the muscles rather than by forcing the fragments into place by direct pressure. Coercion is certainly an impracticable principle in this department of surgery, provoking secession and the non-union of the fragments.

In fractures near the condyles, the upper extremity of the lower fragment is drawn backward into the ham by the heads of the gastrocnemius, especially if the leg be completely extended on the thigh. If we flex the leg, and, applying the hand to the upper part of the calf, make traction in the direction in which the femur points, the deformity disappears. We make this position permanent, and the adjustment secure, by suspending it in the anterior splint, making the angle at the knee more acute than that seen in the cut. The gentle support of the turns of the bandage in the ham contributes essentially to the desired result.

In fractures of the patella I have employed the same apparatus with signal success and comfort to the patient. The apparatus for this purpose must be made straight at the knee—the heel kept well elevated, so as to flex the thigh in some degree on the pelvis—the cord made perpendicular, or rather inclining toward the trunk of the patient, inasmuch as no necessity for traction exists. A compress must be placed above and below the patella, and the limb bandaged snugly up against the splint.

The application of the anterior splint to the treatment of fractures of the leg, especially compound and comminuted, has been not less satisfactory than in those of the femur. Indeed, the first case in which its usefulness was demonstrated was one of compound and comminuted fracture of the bones of the leg. The subject was Edward English, who came under my care in the Baltimore Infirmary, Sept. 28, 1854. He had been for months under treatment in good hands, in a neighbouring hospital. Amputation had been proposed, and would have been justified by the rules of conservative surgery. The patient, however, demurred, and prevailed on his friends to take him to the Baltimore Infirmary, with the hope that I would make further effort to save the member. I had every motive therefore to accomplish the object, if possible.

The leg was a shapeless mass of disease. The comminution, and lesion of the soft parts, had been very extensive. Many necrosed fragments were still included in the tumid parts; many fistulous openings existed along the whole length of the tibia, discharging matter from numerous sinuses. The limb was enormously tumid and of a livid hue from impeded circulation.

I employed the apparatus of wood, but extending no higher than six inches above the knee. It had the usual angle at the knee and at the ankle. Having placed it along the anterior surface of the leg, I first attached it loosely by adhesive strips, one passing under the foot, one under the ankle, one below the seat of fracture, one above it, and one above the knee. I placed a compress under the upper extremity of the splint where it rested on the front of the thigh. Staples were attached to the splint,

one near the knee and the other near the ankle, and by these the limb was suspended, the cord and pulley being employed as illustrated in the cut. I employed three bandages, two designed to be permanent and one movable. One was applied to the foot and carried up, with occasional reverses to effect accurate adaptation beneath, to the vicinity of the fracture. A second began above the fracture and was carried to the upper extremity of the splint, so as to grasp firmly the thigh. The region of the fracture was now exposed, but the support was complete. I now, after placing a basin beneath the limb and thoroughly washing it, applied the requisite dressings, and finally the movable bandage, embracing the region of fracture and many of the fistulous openings, arranging the turns so as not to obstruct the free issue of matter.

I was exceedingly gratified at the immediate results. The moment the member was slung the patient exulted in the relief which he experienced from the rigid confinement and constrained posture from which he had suffered while the limb lay an immovable mass, encumbered by splints, on the bed. He found himself capable of changing his posture at will, without the slightest hurt. The gentle concentric pressure of the bandage, determined by the weight of the limb, began at once to express the matter from the sinuses, and it was seen dripping copiously from the limb. The sinuses were thus emptied, and their walls compressed. The same mechanism promoted the absorption of lymph and serum from the tumid parts, and, together with the slightly elevated posture, aided the return of blood from the limb.

Improvement was immediate and rapid. The dressings were accomplished without the slightest disturbance of the limb, by removing the movable bandage and exposing completely the seat of injury. I removed, from time to time, considerable portions of necrosed bone, and laid open sinuses. The result was complete recovery with a useful limb.

The wire splint for the leg is applied as described above. In all fractures of this member above, or at, the middle, it is essential that the apparatus should extend above the knee, and grasp the lower third of the thigh. All the unavoidable movements of the thigh will then be communicated to the splint and will not disturb the upper fragment.

In fractures above the middle, the splint must be straight at the knee, so as to extend perfectly the leg on the thigh, and thus relax the muscles which would elevate the superior fragment, especially in oblique fractures. In compound fractures, the splint should not be wrapped with muslin opposite to the seat of injury. Adhesive strips should then be employed to attach the limb to the splint, before elevating it, because more cleanly. A permanent bandage should be applied below, and another above. A movable bandage encircles the wounded region. It may sometimes be expedient to interpose compresses at certain points between the shin and splint, to render the coaptation more perfect, but I have seldom found them necessary.

When the fracture is of the lower third, it is exceedingly important that the weight of the foot should be well supported, otherwise there will be a salient angle of deformity in front. This must be done without injurious pressure on the heel, and can be effected by supporting the sole of the foot well with the adhesive strap, which is drawn under the sole, and carrying the bandage with several well-adapted turns under the heel. The heel will then not suffer, because it rests on a surface perfectly adapted to its form. The turns may be starched after application, to render them permanent.

The weight of the limb, sinking into the apparatus, mainly exerts the force that restores and preserves the normal form of the member. The gentle traction of the cord, which should be but slight, contributes to the same result.

In compound dislocations of the ankle, complicated as they usually are with fracture, I have found the anterior splint far more satisfactory than any other means of support which I have employed. In those cases the foot must, as much as possible, repose upon its sole, where sustained by the adhesive strap. Another adhesive strap should be carried under the heel and over the splint. In all cases of injury at the ankle or near it, the lower hooks should be applied to the apparatus near the bend at the ankle.

When, in recent fractures of the leg, the member is first placed in the apparatus, the muscles will be, for a time, affected with a spasmodic rigidity and quivering which will for a brief time distort, in a degree, the fragments, and shorten the limb; but when it has reposed for a short time in the well-adapted concavity of the apparatus, the irritation ceases, and the fragments fall into their true relations. It is worse than useless to struggle with this tendency to distortion by direct mechanical pressure. If deformity continues, it is because the member is not well sustained by the proper adjustment of the supports.

If, in the application of the splint to fractures of the leg, the upper end of the apparatus is found to press too much upon the anterior surface of the member, the supporting cord must be slipped in the loop of the single cord, so as to shorten the portion to which the upper hook is attached. A compress should also be placed under the upper extremity of the splint. A pillow should be placed under the thigh, and the heel should be sufficiently elevated to clear the bed.

Care must be taken that the foot-piece does not press too much on the instep. It ought not to touch it when the apparatus is first applied. Often I interpose a thick, soft compress, and remove it in case the pressure becomes annoying.

It is generally not necessary, in fractures of the leg, to make extension. The cord, therefore, may be arranged perpendicularly. When it is necessary it will not usually be requisite to continue it in any considerable degree for more than four or five days.

It is worthy of remark that in the employment of this, or any other

apparatus which furnishes nearly perfect support to the fractured bone, and allows no appreciable motion, there will be no provisional tumour, or callus. The apparatus is the provisional expedient, and nature, instinctively aware of its existence, produces no exterior callus, but proceeds to restore the direct continuity. The time, therefore, occupied in restoring the rigidity of the limb will be greater, in some cases, than where less perfect support is given; but the ultimate direct union is sooner effected and far more perfect; nor will the muscles, after union, be embarrassed in their action by callus or by projecting fragments. The attentions necessary for the evacuations of the patient are greatly facilitated by this apparatus. There is nothing in the way obstructing the use of the bed-pan. When raised for this purpose the limb may be correspondingly elevated by slipping the block on the single cord.

NOTE.—It was my original design to have added to this article a series of cases showing the varieties of extensive compound and comminuted fractures to which “The Anterior Suspensory Apparatus” has been successfully applied by myself and others in this city and the surrounding counties; but by so doing its length would be extended beyond proper limits. I have employed it constantly since September, 1854, and some of the cases were briefly alluded to in a clinical lecture reported for the *Maryland and Virginia Medical Journal*, and printed in its 1st vol. for 1860. Since it was known that the above paper was being prepared many medical gentlemen have offered me illustrative cases, for which they will please accept my thanks.

ART. II.—*Sulphuric Ether and Chloroform as Anæsthetics, considered with reference to their relative Safety and Efficiency.* By FREDERICK D. LENTE, M. D., of Cold Spring, New York.

NOTWITHSTANDING the many plans that have been devised within the past few years for avoiding the dangers of chloroform inhalation, or for recovering the patient when threatened with its occasional alarming consequences, and the many ingenious contrivances, in the way of inhalers, invented for the same purpose; deaths, which, upon autopsy, can be attributed to no other cause than the anæsthetic, do still occur; and seemingly in as large a ratio as ever; for we are, every now and then, as we open a number of some foreign or domestic journal, greeted with the unwelcome caption, “Death from chloroform;” not occurring as they formerly did, in a considerable proportion of the cases, during the administration of the agent by dentists and non-medical persons, but in the practice of men well known in the profession. One effect of this, on the writer at least, is to produce a very unpleasant anxiety during the performance of any operation with the aid of anæsthesia, in addition to that which naturally attaches itself to

every operation of importance, and which, more or less, distracts the attention of the operator, according to the more or less reliance we place on the judgment of the person intrusted with the administration of the anæsthetic; and unless in a city, a reliable assistant is not always to be obtained. The important question is then still before the profession—*how are the dangers of anæsthesia to be avoided?*

Greater or less danger is inseparable from the administration of every powerful agent in the *Materia Medica*, however cautious and skilful the practitioner by whom it is employed; nor can we reasonably expect that an agent, so powerful as in a few minutes to render the body insensible to the pain of a torturing operation, shall be entirely exempt from risk. By what means, then, can we reduce this risk to its *minimum*? One tells us that it is to be effected by the use of his inhaler; another by his; another by some peculiar arrangement of the sponge or towel so as to insure a due admixture of atmospheric air, or by regulating the successive doses of the agent. But still the fact stares us in the face that the number of deaths is not diminished.

Various anæsthetics have been proposed, but only two have stood the test of time—sulphuric ether and chloroform. Both have been used to a great extent all over the world; but, for some years past, the latter has almost entirely superseded the former in Europe,¹ and still retains the confidence of many of the profession in this country. Five years ago, when the number of deaths from anæsthetics had fully aroused the profession to the necessity of seeking some means of diminishing its danger, the writer published an article in the *New York Journal of Medicine*, calling attention to the fact that it was from chloroform that all these accidents had occurred, and that the most obvious means of avoiding their repetition was its entire disuse, and the substitution of its less dangerous, but no less efficient competitor—sulphuric ether. Some months after this, an article, from the pen of M. Barrier, of the Hôtel Dieu de Lyons, appeared in a French journal, taking precisely the same ground, which was extensively noticed in foreign journals. But whether either article had any effect in directing the notice of surgeons more forcibly to the comparative safety of ether is not apparent. Since that date we have met with additional facts bearing on the relative value of these anæsthetics, and have thought that some good might perhaps result from again endeavouring to direct the consideration of surgeons to this matter.

In the first place, it is important to correct a very erroneous impression which has got abroad in consequence of the publication of certain European statistics of death from sulphuric ether and chloroform. These statistics have been recently referred to as authority in an editorial in the *American*

¹ Sulphuric ether is not, I believe, employed anywhere in Europe except in Lyons and Naples.

Medical Times (August 18th), which gave rise to a correspondence in the September number; from which it appears that they were taken from a work by Dr. Kidd, or from an extract from this work published by the author in the *London Med. Times and Gazette*. From these statistics it would appear that, out of one hundred and twenty-five deaths from anæsthetics in Europe, twenty-five are from ether, which, if accepted, would indicate that it is fully as dangerous as chloroform, the latter having doubtless been employed in at least five times as many instances. Upon examination, however, we find that the authority for the ether cases is, in most instances, a bare statement of M. Trousseau, without any reliable details whatever. Some of these alleged cases of death from ether are noticed by Dr. Snow in his work on *Anæsthetics*; and, except in two instances, the fatal result is clearly not chargeable to the ether, but to the shock of the operation; as for instance in the two cases on p. 365 (Lond. edit.). In one of these cases, the patient did not succumb until the third day; and in the other, forty hours after the operation, which was a very protracted one, and during which the patient was not even thoroughly etherized, thus failing, as it should have done, to diminish the shock. The coronor's verdict was, nevertheless, "Death from ether." I will only refer to two other cases (pp. 268-269). In one, the operation was an amputation of the thigh for compound fracture of the thigh with extensive laceration, and a simple fracture of the other thigh. The etherization was not perfect during the most painful steps of the operation, and its shock thus undiminished; as might have been anticipated from the nature of the injury, the patient died, but not until *three hours* after the amputation. In the second case, a cancerous tumour, weighing three and a quarter pounds, was removed from the breast of a feeble old man; and here also the fatal result occurred only after the lapse of *seven hours*. Dr. Kidd's language is too vague to admit of any confidence in his reports regarding so important a matter as the one under discussion. He speaks of deaths from ether, and from a mixture of ether and chloroform, and from amylene, in such a confused manner that one is completely in the dark as to how many are to be attributed to each of these agents individually, even if it be admitted that they were due to the anæsthetic, and not to the accident or operation. But if any one wishes to get a clear idea of how utterly unreliable Dr. Kidd's writings are, he would do well to peruse the review of his "little work on chloroform" in the *London Med. Times and Gazette*, or the *Edinburgh*, or the *Dublin Medical Journal* for 1858. Whether the death in any given case is to be attributed to the anæsthetic, or the shock, or hemorrhage, or other accident connected with the operation itself, is often a matter of opinion or judgment, and sometimes, perhaps, even of prejudice; as in some of the cases above alluded to. Therefore, each case, to be accepted as authentic, should be carefully given in detail, and, if possible, accompanied with the autopsy. For instance, in two cases of alleged death from sul-

phuric ether recorded in Dr. Snow's work (pp. 362-64), the author repudiates the idea of the death being due to the anæsthetic; whereas, both cases certainly admit of doubt, and most readers would find the ether guilty in one instance.¹

Let us now inquire how many authentic cases of death from sulphuric ether are to be found on record. Happily they are few indeed. The only European case worthy of credit, and this repudiated, as we have seen by the highest authority, is that which happened at the Hôtel Dieu d'Auxerre. At the autopsy, the spleen was found disorganized; but the general condition of the patient was good previous to the operation, which was for cancer. The case which happened under M. Barrier's hands at Lyons, I think, we may fairly attribute, with Dr. Snow, to the hemorrhage and shock of the operation. With regard to American cases, I will first notice that which happened at Bellevue Hospital in the city of New York; which will be found well reported in the *New York Journal of Medicine* for July, 1859. It is hardly fair to attribute the fatal result in this instance to the anæsthetic, when it is known that the autopsy revealed a *large malignant tumour of the cerebellum*; also that the patient had been suffering for weeks with unmistakable symptoms of serious disease of the brain; unsteadiness of gait, loss of vision in one eye, partial loss of mental power, constant tendency to roll out of bed, always to one side; and especially when it is known that sudden death is very apt to occur, if indeed, it does not always occur, in these cases of tumour of the cerebellum. Dr. A. Clark referred to two cases at the meeting of the Pathological Society of New York, at which this case was reported, of sudden death, where the autopsy revealed tumour of the cerebellum. Dr. Eve relates two cases of what he considers death from inhalation of sulph. ether, in the *Southern Med. and Surg. Journ.* for 1849, republished in the *Am. Journ. of Med. Sci.* for the same year. The first was that of a medical student, who, for amusement, inhaled about an ounce of the liquid, and became furiously excited, so that it required several persons to restrain him. He fell into a deep sleep, was awakened, and became again excited and uncontrollable; then was allowed to be quiet, and slept well all night, complaining next morning only of a slight headache. This soon increased to severity, and he died of symptoms of *phrenitis*. But no autopsy was held, nor is there any evidence of inquiry into his previous history, as to symptoms of cerebral disease. It is at least doubtful, admitting phrenitis to have been the immediate cause of death, whether this was due to the ether directly, or to the violent excitement caused by the etherization having been arrested previous to complete insensibility. Dr. Eve's second case occurred in the

¹ Dr. G. Hayward, of Boston, who is excellent authority in this matter, agrees with Dr. Snow, that no well attested case of death from sulphuric ether is recorded. (*British and For. Med.-Chirurg. Rev.* for 1859.)

practice of his friend Dr. Bassett, at Huntsville, Ala. The patient was labouring under a violent attack of tetanus; and, we may infer from the history, that he was well nigh exhausted by the disease before the consultation was held at which it was determined to give ether. The report states that, *in one minute*, he was under its full influence, and in a quarter more, he was dead. This case occurred shortly after the discovery of the anæsthetic influence of ether; and probably none of those present were acquainted with its phenomena. If they had had the experience of the present day, they never could have arrived unanimously at the conclusion that the patient died of the effects of the ether. It is almost, if not absolutely impossible to induce full anæsthesia with ether in the time alleged; the supposed anæsthesia was doubtless the death-stroke of the disease.¹

But it scarcely falls within the scope of this paper to discuss the use of anesthetics or their dangers in *medical* practice; and we might, with propriety, have omitted all reference to these cases, were it not that we might be suspected of concealing unfavourable facts, in order to place sulph. ether on a more favourable basis. If anæsthetics be made use of in medical practice, especially in those violent, dangerous, and oftentimes almost incurable cases to which they are generally applied, as in tetanus, epilepsy, puerperal convulsions, delirium tremens, cerebral diseases, and the like, we must expect a sudden death now and then—either from the disease, and coinciding with the administration of the anæsthetic, or from the combined effect of the disease and the remedy—just as we occasionally witness the death of a patient from croup, during the attempt to save life by tracheotomy. I have myself desisted from administering chloroform to a man labouring under tetanus, who had been relieved again and again by it of his terrible suffering, because it was evident that he was liable to die during the application of the remedy. I have abandoned anæsthetics in the paroxysms of delirium tremens, because I found there was danger of immediate death from the combined effect of the vapour and the violent resistance to its reception by the raving patient. I deem it my duty to allude here to two other *surgical* cases which I know to have happened recently, where the ether might be considered as chargeable with the fatal result. In one, at least, from the history of the case, I think it was so. It is to be regretted that the history of these cases cannot be fully given. In one the patient was exceedingly reduced, and in an almost hopeless condition, previous to the operation; but the death, which occurred suddenly, and during the insensibility from the anæsthetic, was probably not entirely due to the shock of the operative procedure. In the second case, the operation, which was not a “cutting” one, necessitated a very unfavourable position for giving ether, the head being flexed forcibly on the chest; and it was while

¹ Dr. Hayward relates one case, which he saw at Naples. where anæsthesia was effected with sulph. ether in one minute and a third. But this case is unique.

in this position, that the patient was noticed to be in a dying condition—too little attention having probably been given to the state of the respiration and pulse. Both these operations were performed by a skilful and experienced surgeon.

We thus admit *three deaths* from sulph. ether throughout the world. It is safe to say that there have been at least a hundred from chloroform in Europe alone. It is impossible to give the *ratio* of deaths to the *number* of inhalations of these two agents respectively, which is very desirable. But, we know that, for one year before the introduction of chloroform into practice, sulph. ether was the sole anæsthetic; that it was very extensively employed in Europe and America; that ever since, ether has been almost the only anæsthetic in some of our large cities, and for some years past almost exclusively used in most of our large hospitals in the United States. Dr. Geo. Hayward, of Boston, who was the first surgeon to use an anæsthetic successfully, in a capital operation, and to whom is due the credit of having made the most persevering and successful efforts to obviate the dangers of anæsthesia, by recommending the disuse of chloroform, had personal experience of near a thousand cases of ether inhalation, near two years ago, which of course only comprise a certain proportion of the cases that have occurred in his city under other surgeons. Dr. Hayward informs us, in a letter just received, “that ether is administered in this city (Boston) daily to a great extent, without producing death in a single instance, or any alarming or troublesome symptoms.” In the New York Hospital, ether has been almost, if not entirely, the only anæsthetic for some twelve or fourteen years. It has been stated, by French authority (*L'Union Médicale*), that from 18,000 or 19,000 chloroform inhalations in the Crimea, only two deaths took place—one in the English, and one in the French army. The assertion of M. Baudens, that among 30,000 cases in the Crimea, no death occurred from anæsthesia, is denied by other French army surgeons, who themselves saw deaths from it. As we have before asserted, it is a matter of individual judgment, in many cases, as to whether the death is to be attributed to the anæsthetic; and we may well suppose that, on the field of battle, it would be extremely difficult, if not impossible, in most cases, to judge whether a death, occurring during a terrible operation or soon after it, while the patient is still suffering from the shock, is due to the one cause or the other. This must have been especially the case on the sanguinary fields of the Crimea, where the surgeons were so overtasked that they could have had but little time or inclination to note the relative effects of the chloroform and the knife, or the injury. In the hospital operations the case might be different; but even there, the same cool judgment cannot be bestowed on the cases as in civil hospitals, where medical assistants and spectators are numerous, and not personally absorbed in the operative procedure. If these reflections are unjust, it is unaccountable that such a disproportion of deaths to the number of cases of inhala-

tion should have occurred in military surgery, and under comparatively unfavourable circumstances. Every medical man who reads the periodicals of the present day, especially the foreign, must be struck with the frequency with which "death from chloroform" meets the eye, and yet it would require many months to add up 30,000 cases of chloroform inhalation in civil hospitals; and it is from hospitals almost exclusively that we get our reports of fatal cases. The ratio of fatal to successful cases of chloroform inhalation must therefore be greater than these military statistics would indicate.¹

But, there are other evidences in favour of the superior safety of sulph. ether besides those deduced from statistics, which latter may be considered, and are to some extent, imperfect, as regards both these agents. First, it is admitted, universally, that ether is less powerful than chloroform, and therefore there is less danger of that sudden and unexpected action, which has been the cause of most of the deaths from anæsthesia; or, of the ill consequences of carelessness or inattention on the part of the person intrusted with its administration, which has no doubt been the true cause of not a few of the fatalities.

The evidence of physiologists, who have largely used both these agents in quieting animals subjected to vivisection, points strongly to the greater safety of ether inhalation, the occurrence of accidental death being very common under the use of chloroform, and very rare with ether—the insensibility being complete in both instances. My friend, Dr. H. B. Sands, of the University of New York, found it so in his numerous experiments, and he informs me that Prof. Dalton, the distinguished physiologist of the same institution, has met with like results. Dr. Snow, whose authority has been so often quoted, and whose opinions on these subjects are regarded with the greatest respect by the profession in Great Britain, says, p. 362, "I have not been able to kill an animal in that manner (meaning the sudden manner in which he found chloroform act) with ether, even when I have made it boil, and administered the vapour almost pure." He adds, "I hold it therefore almost impossible that a death from this agent can occur in the hands of a medical man, who is applying it with ordinary intelligence and attention." The following incident, which recently fell under my own observation, is sufficiently interesting in this connection to merit an introduction here, for it seems to settle the question, as far, at least, as the inferior orders are concerned, of the relative safety of the two anæsthetics under discussion. My old friend, Mr. John Lyell, a teacher in this place, has a large number of bees, with whose operations he has long

¹ It is a significant fact, alluded to by Dr. Snow, that although ether was used over a year in Europe and America before the discovery of chloroform, only one death occurred from anæsthesia. Whereas, chloroform was only introduced to the profession in November, and in January a death from it occurred, and soon after, others in various parts of the world.

been in the habit of amusing himself, during his leisure hours, and wishing to take his honey without destroying his pets, asked my advice about resorting to anæsthetics. I advised chloroform, as being the stronger and more convenient agent, and the one usually employed for such a purpose. He accordingly tried it; but, on finding that he lost a great many bees by suffocation, he substituted sulph. ether. This he found equally efficacious, and, what is very remarkable, that he did not by it lose a single bee, while he showed me more than a quart that had been destroyed by the chloroform, the insensibility having been just as perfect and prolonged in both instances, and both agents employed in the same manner, and under the same circumstances. It may not be out of place to relate a case which occurred a year ago, in my own practice, as showing the greater facility with which recovery ensues from too thorough anæsthesia from ether than from chloroform. The operation consisted in the removal of a fibrous tumour from the submaxillary region, together with the submaxillary gland. It was requisite that the insensibility should be complete; and, in addition to this, the necessity for keeping the handle of a knife in the mouth in order to force out the tumour, and render it salient externally, tended to impede the respiration still more. The ether was superintended by a surgeon attached to a large hospital, and well acquainted with the action of anæsthetics, who noticed, after the inhalation had proceeded satisfactorily for some time, and after the operation was nearly finished, that the respiration and pulse were both failing. Further proceedings were immediately suspended, and for a time the danger seemed imminent. The window was opened, a little water dashed in the face, and the patient allowed to remain perfectly quiet. In a few moments he revived, the respiration and pulse resumed their natural rhythm; he was again brought under the influence of the ether, and the operation completed. It was not only my opinion, but that of at least two of the hospital surgeons present, that had it been chloroform instead of ether, the case would not have terminated so favourably.

Another case, illustrating still more forcibly this matter, occurred a short time since at the New York Eye Infirmary, to which I am permitted to allude, by Dr. Noyes, of that institution. The patient had been thoroughly *etherized*, and had been breathing stertorously, when his countenance was observed to be livid, his respiration to cease, and his pulse very feeble. He was turned on his side, his tongue drawn forward; and, very soon, without any further treatment, his pulse and respiration resumed their wonted fullness and regularity. Some cases have been reported of apparent death from *chloroform*, in which the patients were resuscitated; but it was only by the most energetic means, including skilful and protracted artificial respiration. But, it is unfortunately true that artificial respiration, which, by Marshall Hall's method, is so successful in cases of asphyxia from quite protracted submersion, and in suspended foetal animation, and even after

poisoning by narcotics, has very generally failed in suspended animation from chloroform inhalation. A few cases, however, of resuscitation of patients asphyxiated by chloroform vapour, which have fallen under my notice, seem to indicate that artificial respiration, if resorted to at once, and before loss of time, and possibly additional injury to the patient by other less efficient measures, might prove less unsuccessful. Prof. Metcalfe, in a paper on this subject read before the New York Academy of Medicine, some years ago, mentions four cases of recovery after pulse and respiration had ceased. One occurred in his own practice. He promptly applied his lips to the mouth of the patient, and inflated the lungs, while another physician compressed the thorax and produced expiration. Prof. Valentine Mott informs me that he had two cases in his own practice, where the patients were rescued from impending death by prompt artificial respiration. One was the case of the wife of a medical gentleman in New York; the other was a child, on whom he operated for stone. I find a case in a late number of the London *Lancet*, of which the following is a brief abstract: Chloroform was administered to a girl 23 years of age, for the purpose of amputating the thigh. Everything seemed to be going on as usual, and the operation was about to be commenced, when “she *suddenly ceased to breathe, and the pulse could not be felt*; cold water was instantly thrown on her face, air freely admitted into the room, and artificial respiration kept up by alternately compressing and relaxing the chest.” No success. “The head fell on the chest, the chin dropped—in fact, she appeared quite dead. As a last resource, in order to use artificial respiration more effectually, it was decided to open the trachea, and inflate the lungs through the wound. This was at first done with the mouth applied to the wound, with some success. A female catheter was now introduced into the trachea, and artificial respiration kept up through it.” This was fully successful. In most, I believe in all the above cases, the respiration was kept up by the old method—*insufflation*; in the last case, we see that it was successful after the mechanical method had failed. It may be that chloroform asphyxia is not as amenable to the “ready method,” and other similar means of inducing respiration, as the asphyxia referable to other causes. It is certain that to be effective it must be far more prompt.

It is proper to make a brief allusion to a mixture of chloroform and sulphuric ether, which has been recommended as an anæsthetic, with a view to combine the advantages of both, and to lessen the danger of the former. It was at one time used to a considerable extent. A death from this anæsthetic occurred in the service of M. Valette, of Lyons; and another is reported from Virginia; but, from the history of this case, there is doubt whether the fatal result might not have been, with greater propriety, attributed to the operation. My own opinion of this mixture, without any actual experience, is that it is more dangerous than either the one or the other anæsthetic alone. For, the person intrusted with its

administration, supposing it to be less dangerous than chloroform, gives it more rapidly, and perhaps with less care; whereas, he may be giving the chloroform almost as pure as if it had not been mixed; for the ether being far more volatile than the chloroform, six times, according to Dr. Snow, is soon dissipated, and the latter left on the sponge to be inhaled alone. Unless they could be uniformly evaporated together, I see only danger in combining them.

But, even if admitting the greater safety of sulph. ether, its opponents argue that, after all, the proportion of deaths from chloroform is extremely small compared with the aggregate number of cases of inhalation; that no powerful drug in the *materia medica* can show as clean a record, and that the complexity of the apparatus necessary for giving it, the inconvenience, the delay, the quantity required, and other alleged disadvantages of ether, more than counterbalance its greater safety. Let us now examine these objections; but, first, I am ready to admit the wonderful safety of chloroform inhalation, considering its astonishing power in annulling pain and spasm, and that the proportion of fatal cases to the actual number in which it has been employed under almost every conceivable circumstance, is extremely small. But, if we have a simple means of reducing this proportion still more, as I contend we have, it is our duty to do so. The objections above enumerated are urged by those who have not given ether a fair trial, and would soon vanish if they were deprived of chloroform. I know this from actual experience. Connected with the New York Hospital, at the period of the discovery of the anæsthetic power of sulph. ether, as assistant to the house surgeon, it was one of my duties to administer it to patients about to undergo operations, and no difficulty was experienced in producing its full effect. It was administered by all the surgeons until the introduction of chloroform. It was now soon discovered that ether was troublesome, that large quantities were required, increasing the expense, and that it often failed to produce full insensibility even after considerable delay; it would often be cast aside after a few minutes' trial, and chloroform substituted; with a few drachms, or perhaps an ounce or so of this, and the delay of only a few minutes, the patient would be in a stertorous condition, and all trouble over. The inference was plain, ether was uncertain in its operation, productive of unnecessary delay, and ought to give place to chloroform; and so it did. But, soon one or two deaths from it were reported; and, one day, while we were chloroforming a patient, to whom I had myself assisted in giving it but a short time before without a bad symptom, he suddenly died, and nobody was to blame on the coroner's inquest, and to nothing else could the unfortunate result be attributed but the anæsthetic.¹ So to avoid a similar occurrence, as no one had then heard of a death from ether, it was resolved to return to it; and, after

¹ Case reported in Dr. Snow's work, p. 136.

this, we had no more difficulty in etherizing patients. But let us take up the objections in detail, and consider them fairly. First, as to *quantity*, being a less powerful agent than chloroform, it must of course be used in larger quantity to produce the same effect, but by no means in the enormous quantities alleged by those who prefer chloroform; that is, if properly administered; and the proper mode of giving it will best be considered here. If one accustomed to the careful and gradual manner in which chloroform must needs be given to insure any degree of safety, attempts to give ether in a similar manner, and without bearing in mind that it is not liable to that sudden action of the former, and especially that it is six times more volatile, he will certainly fail, or be subjected to great delay and inconvenience, and use enough of the drug to put half a dozen patients asleep. I have known skilful physicians to do this very thing, and have put patients sound asleep for the dentist with two or three ounces, who had been previously subjected to the fumes of ether for half an hour, in one instance, the patient said, an hour, involving also the waste of near a pound of the anæsthetic, the effort finally ending in failure. For an adult, two or three ounces, sometimes less, are abundant for the production of full anæsthesia for a painful operation. For children, from three drachms to an ounce and a half, according to age; children are affected much more readily and pleasantly by anæsthetics than adults, with the exception of the preliminary struggles from fright, during which the inhaler should be kept closely applied. By using a properly constructed inhaler, even less ether would probably be consumed. And this I conceive to be the only advantage of an "inhaler." As regards delay, I think it is increased; as regards safety, I cannot see that it is enhanced by any of the various apparatus invented. In the very first fatal case reported as having occurred from ether, an "inhaler" was used.¹ The principal alleged advantage of a regular inhaler is the regulation and due admixture of the air admitted. But this can be just as well regulated by a proper arrangement of the sponge, or even the towel, as will presently be explained. My friend, Dr. Squibb, of Brooklyn, has recently, through some mistake, represented me as advocating the inhaler which he highly approves of, made by rolling up a thick, folded towel in a cylindrical form, open at top and bottom; the ether is poured around the inside of this, and the open top insures a safe amount of air—unfortunately, for its success, too large an amount. On this recommendation, Dr. Weir, of the New York Hospital, tried it, but found that it caused much waste, both of time and ether. I will quote from an article on this subject, furnished by me to the *New York Journal of Medicine*,² to explain my mode of employing it, which is the plan

¹ From Scotland we have had fewer reports of death from chloroform than from any other part of Europe, although this anæsthetic is exclusively used there, and still on a folded handkerchief or towel, as at first.

² September, 1855.

originally adopted in Boston, and now generally used throughout the United States, with some trifling modifications. "To be effective within a reasonably short time, it must be given rapidly, and the *access of air cut off as much as possible*, the reverse of what is safe with chloroform. The only effective inhaler we have used or seen used, is a large cup-shaped sponge, sufficient to cover completely the nose and mouth, and covered with a thick folded napkin, to prevent undue evaporation as far as possible." I now pour into the sponge about half an ounce of ether, and, covering it fully with two thick towels placed together and folded into a square large enough to cover both it and the face to some extent, approach it gradually, but without much delay, close to the face of the patient, after directing him to take full inspirations through nose and mouth. If he is disposed to hold his breath during the inhalation, or to cough, I withdraw it a trifle to allow a larger admixture of air, and then immediately approach it again to the face; and as soon as it can be inhaled fully, without coughing or strangling, the sponge is kept lightly *in contact* with the face, and the towels held completely over the sponge and face, gathered in around the edges so as to cut off *evaporation of ether*, and *ingress of air*. The half ounce is soon followed by about an ounce, which is rapidly poured *into*, and not *on* the sponge, though the latter is occasionally preferable, as recommended by Dr. Hayward. The necessity for replenishing being judged of by the time, or by the nose applied to the edge of the sponge a little raised. When insensibility is *complete*, judged of by the relaxation of the muscles of the arms, or by touching the *conjunctiva oculi*, the sponge is either entirely or partially removed from the face, according, sometimes, to the nature of the operation, sometimes to the character of the respiration and pulse—stertorous breathing contraindicating even the smallest quantity of the vapour.¹

As regards the *quantity* of ether required in protracted operations, it is not so great as is generally supposed. I have no statistics to guide me; but, more than a pint, if judiciously used, even in the most tedious cases, is seldom necessary—the patient gets so saturated with the ether, after six or eight ounces have been inhaled, that very little, applied from time to time, will keep up full insensibility. The last protracted operation of mine, a few nights ago, required the patient, a female in delicate health, to be constantly kept in a state of full anæsthesia for at least an hour and a half, and not more than eleven ounces of ether were consumed, and it was administered most of the time by an apothecary. Dr. Hayward says from four to eight ounces, but he means for the whole operation, not merely for the preliminary anæsthesia, of which we have been speaking, when alluding

¹ I have never heard that this precaution, of cutting off as far as possible, all admixture of air, has been attended to anywhere, either here or in Europe, except in Naples, where, Dr. Hayward informs us, it is very successful.

to the minimum quantity; and he adds, that less would be required if care be taken to exclude atmospheric air, which he either thinks not quite safe, or unnecessary. I am well aware that much larger quantities of ether are used than are spoken of as sufficient by Dr. Hayward and myself, even where this agent is extensively employed, as for instance, even now, at the New York Hospital. But, still, if they proceeded upon the principle of saving ether, and of using only enough to produce the desired effect, even without entirely excluding the air, as here recommended, much less would be consumed. But, it is obviously idle to discuss quantity, except with reference to time; for, what difference does it make, in a painful and important operation, whether the surgeon uses four ounces or eight ounces? If quantity is referred to by those who object to ether, with reference to expense, a pound of Dr. Squibb's article, the best for inhalation in the market, in our judgment, can be purchased for half a dollar, and two fluid-ounces of his chloroform costs as much or more.

As to *time*: to induce full anesthesia in an adult with sulph. ether should not require more than five minutes, sometimes six. With children much less time is required. Dr. Snow gives two or three minutes as the average for children, and four or five for adults. And how much sooner can a patient be *safely* chloroformed? Dr. Hayward states that "*in no case* were more than eight minutes required to produce complete anæsthesia." This is the maximum; he does not state the average.

I do not desire to be understood as ignoring all danger and all necessity for caution in the administration of sulph. ether. On the contrary, I hold that every surgeon, who performs an operation with the aid of any anæsthetic, is bound, if possible, to secure the services of some competent person to attend to this department, or to look to the condition of the patient himself from time to time; and this he may generally do in the case of ether, an agent so uniform and regular in its action, without distracting his mind too much from the operative procedure itself. It is of great importance also to attend to the state of the pulse for a short time subsequent to the completion of the operation; for, occasionally, alarming prostration has followed anæsthesia, after trivial operations, even where the inhalation has proceeded with perfect regularity and success. I do not of course allude to those alleged *deaths* from ether, previously referred to in this paper, where the patients died hours, and even days after protracted and dangerous operations, but to such cases as one which I reported in the *New York Journal of Medicine*, for 1856, and which is referred to in the last edition of the *U. S. Dispensatory*. I have recently met with one almost precisely similar, of which, the fear of occupying too much space in the *Journal*, forbids any notice at present. Should vomiting succeed the administration of the anæsthetic, the patient being partially under its influence, the air-passages may become suddenly clogged by the lumps of food which some people bolt, and thus endanger life. We have not always

the opportunity of enjoining an empty stomach, which is advisable. Another danger, that we occasionally meet with, is the falling backward of the tongue, as in the case at the Eye Infirmary. There is no danger, however, that may not be readily avoided by a little care.

An important preliminary with patients who are nervous, or alarmed at the idea of taking the ether, is to get their confidence, to assure them positively of the thorough safety of the drug, and to explain the sensations which they are likely to experience in passing through the several stages to complete anæsthesia; especially the choking feeling which immediately precedes total insensibility, and which often otherwise causes a struggle, and delays the operation. It is sometimes necessary to administer the agent in an adjoining room, when there are a number of spectators or assistants assembled. With sulph. ether, it is also sometimes advisable to close the doors and windows, if open, should there be a current of air, as I have found this to delay the anæsthesia. The objection urged against ether, that its odour is disagreeable, and saturates the clothes, and penetrates the whole house, is too puerile to merit notice. Its inflammability is a more valid objection during operations by candlelight; but, a little care will remove all danger from this source.

I am aware that all this minute account of a simple matter, which everybody is supposed already to understand fully, will be considered by many only tedious and useless; but, that it is not entirely unnecessary, is apparent from the following paragraph, which I quote from an editorial in a leading London Journal.¹ "The complexity of the apparatus necessary for its administration, the large quantity required, and the time taken up, are the great hindrances to its more general use in this country." It has been my humble endeavour and sole object in writing this paper, as it was in coming before the profession previously on the same subject, to aid in correcting these very erroneous impressions regarding sulph. ether, under which many of the profession in this country, and almost the entire profession in Europe, are labouring.

NOTE.—Soon after forwarding the above paper to the Editor, I observed, in a medical periodical, a brief notice of a case of "death from sulphuric ether," said to have been read before the Cincinnati Academy of Medicine, by Dr. W. H. Mussey. Upon application to the reporter, I was very promptly furnished with the details of the very interesting case, which will be found in the *Cincinnati Lancet and Observer*, January, 1861. From a careful consideration of these details, I cannot attribute the fatal result to the anæsthetic. Only *four* ounces of ether were used; and, as it was poured on a folded towel, and from twenty to twenty-five minutes, apparently, consumed in its administration, there having been two intermissions on account of vomiting, but a very small proportion of these four ounces must have been breathed. And when the operation was *commenced*, the administration had been discontinued on account of some peculiarity of the breathing, "the pa-

¹ London Medical Times and Gazette, July, 1858.

tient screaming out and writhing with pain, and apparently perfectly conscious." Prompt artificial respiration, and other energetic means, failed to restore life.

The autopsy revealed extensive injuries (from the accident), sufficient, in the opinion of Dr. Mussey, and several other physicians present, to have caused death in a short time. The brain was not examined; and the left ventricle of the heart was empty, which is not the case after death from suffocation, or from anæsthetics.

Dr. M. remarks, that—"A lengthy discussion arose in the academy, occupying two evenings of its session, in which, two members contended that the case was clearly one of death from ether. Of the remaining disputants, two thought ether possibly auxiliary, while a majority thought ether not at all responsible."

ART. III.—*On Perineal Section.* By J. MASON WARREN, M. D., one of the Surgeons to the Massachusetts General Hospital.

THE distinguished surgeon, Mr. Syme, of Edinboro', has of late years brought more particularly to the notice of the profession the incision of the urethra in the perineum for the purpose of relieving strictures of a bad character. Some confusion seems to exist as to the cases for which this operation is more particularly applicable. So far as we can understand Mr. Syme, his operation appears to be directed to those cases which are permeable to the instrument, yet so irritable and intractable as to be a permanent source of irritation to the patient. Mr. Syme considers that every stricture, with patience, is capable of being penetrated. Other gentlemen, and particularly Mr. Bryant, in an excellent paper in Guy's Hospital Reports, think, there are certain cases of stricture admitted to exist by most surgeons which no patience will overcome, and which the use of the knife alone will remedy. In these he advises, that an instrument should be introduced into the urethra as far as allowed by the obstruction; the finger of the left hand is then introduced into the rectum as a guide to the prostate or neck of the bladder. The integuments now being divided, the knife is introduced with its back towards the rectum so as to strike the urethra in front of the prostate, and a cut is made forwards till it meets the instrument in the urethra.

Mr. Bryant, after mentioning the different forms of stricture requiring this operation, rejects that in which with retention of urine the urethra is impermeable to an instrument; for which case he advises as the only proper operation the puncture of the bladder through the rectum. Without entering upon a discussion as to the merits of the operation, we would simply state, that in our experience this is the very case which most frequently calls for the active interference of the surgeon; and there seems to be no good reason, if the history indicate a callous and incurable stricture as the cause of the retention, why the operation should not be so extended as to

divide the stricture at the same time, and give the patient a chance of permanent instead of temporary relief.

In order to illustrate this subject and bring it more fully before the profession I propose, by the exhibition of two or three cases, to show the advantage of the operation in these extreme cases which we are so often obliged to deal with; as it is well known that in permeable strictures, to which Mr. Syme's operation would be applicable, the patient, so long as he could obtain relief otherwise, would be unlikely to submit to a cutting operation. One of these is a case in point with those mentioned by Mr. Bryant, viz., a case of organic stricture of long standing gradually diminishing the calibre of the urethra, and finally terminating in complete retention. In this case, it will be perceived, that instead of puncturing the rectum, as would have been the practice of Mr. B., the urethra was cut down upon in the perineum, opened near the neck of the bladder, and then the whole portion of the strictured urethra divided; and a large instrument being introduced the urethra was moulded upon it until a complete cure was produced. The other two cases are those in which the operation of Mr. Syme must have been rejected; as, after much patient treatment in one of them no instrument could be made to enter the bladder, and in the other, although for a time an instrument could pass through the stricture, ultimately this became impracticable, and the patient's life was gradually being exhausted by pain, confinement, and the irritation and discharge from the numerous fistulous openings in the scrotum and perineum. To Mr. Syme undoubtedly belongs the credit of having fully brought this subject before the profession, and shown the great relief that might be extended to a class of cases allowed to linger on in torment through many years.

In his very valuable paper Mr. Bryant has indicated yet other cases, a class to which the attention of hospital surgeons is much more likely to be called, and of which the preceding are an illustration. We take the liberty of copying Mr. Bryant's conclusions, to which we fully agree, with the exception of the first, upon which we have commented above.

Conclusions.—1. In uncomplicated retention of urine from organic stricture, the operation of opening the urethra in the perineum is not required, the more simple and safe one of puncturing the bladder through the rectum being preferable.

"2. When complicated with extravasation of urine from any cause it should be performed at once, and the stricture, when present, divided, if possible.

"3. In laceration of the urethra from injury, when a catheter cannot be passed, the urethra should be opened.

"4. And also when the above injury is associated with pelvic mischief.

"5. Strictures are occasionally met with which are impermeable, and urethras which are obliterated.

"6. That in cases of organic stricture, when the passage of a catheter is possible and not difficult; where it does not produce either any injurious, or painful constitutional or local disturbance, and where, after dilatation of the stricture, an occasional passage only of the instrument is required to maintain an open channel, no other surgical means can be called for.

"7. That cases of stricture do occur occasionally, which are so exquisitely

sensitive, and in which the passage of a catheter, however skilfully performed, is followed by such severe constitutional and local disturbance as to produce more harm than good; and others, which are relieved by means of a catheter, and are even fully dilated, but which have a tendency to contract again immediately upon the omission of the treatment; in such cases the operation of 'external division' is most valuable.

"8. That the majority of cases of what are called impermeable strictures may be rendered permeable by constitutional treatment, but that some are undoubtedly impermeable; in such cases the operation of 'perineal section' is of value.

"9. When the urethra is obliterated the operation of 'perineal section' may occasionally be demanded, particularly when associated with perineal fistulae.

"10. That the worst and most intractable forms of stricture are the result of injury, and in those cases the operation either of 'external division' or 'perineal section' is of great value.

"11. That in boys the operation is not so successful as in adults, although no better can be suggested."

CASE I. *Organic Stricture of the Urethra of Ten Years' Standing; the Urine finally passed in Drops; Retention; Perineal Section; Division of the Stricture; Complete Recovery.*—A. L. Foydt, about thirty years of age, entered the hospital on April 30, 1860, with a stricture of the urethra of ten years' duration. The water, when he entered, passed in a very small stream, and urine was constantly dribbling away from him during the night, accompanied with a purulent discharge. He complained of pain in the renal region, though not of a severe character. After very careful attempts to pass a bougie of the smallest description, it was finally decided to attempt to cut the stricture from within, which was done from a canula with a concealed blade, on May 15, and which, although it cut but partially through the stricture, seemed to afford him temporary relief.

On May 26 the urine became less free; there was considerable dulness in the pubic region, and the patient complained of pain about the bladder, with general uneasiness. I had already made up my mind to perform the perineal section in this case, and ordered the preparations for the operation to be made for the following day.

On visiting him the next day, I found he had a complete retention of urine, and therefore proceeded at once to the operation. The patient was placed on a table, on his back, and after being thoroughly etherized, so that his joints were fully relaxed, he was tied as in the operation for lithotomy. I have found it much better to confine the limbs in this way than to intrust them to the care of assistants, who are apt to be worn out during an operation so likely to be long and tedious. A small staff was introduced, and it seemed to penetrate the first stricture, which, as stated above, had been incised about a couple of weeks before, and brought up against a second stricture, apparently just behind the root of the scrotum. The forefinger of the left hand was now introduced into the rectum, and the situation of the prostate ascertained. The perineum was then divided, and a careful dissection made, to ascertain the site of the urethra. This was rendered very tedious from the constant discharge of blood at the bottom of the deep wound from the bulb of the urethra, which in this case was more than ordinarily troublesome. The urethra, however, was finally opened directly in front of the prostate, and a gum-elastic catheter passed into the bladder, giving exit to a very large collection of urine. The canal was now opened forwards, and the callosities freely divided, until the staff was reached. A second gum-elastic catheter was now passed downwards through the penis until it appeared in the wound; and the ivory end of the first having been

cut off, the point of the second was insinuated into it, and firmly fixed. In this manner it was dragged up through the whole extent of the urethra. It might be here mentioned, as a hint for finding the urethra, which I have before practised upon, without the staff for a guide, and where other means have failed, to do as was done in the present case—viz., allow the patient partially to recover from the ether, stimulate him to make an effort to urinate, and when the urethra behind the stricture became dilated, a minute stream of urine issued, indicating the spot for the introduction of a probe, and the urethra was found. No unpleasant symptoms followed the operation. On the next day the patient expressed himself in a state of great comfort, more so than he had experienced for many years. The sense of fulness, arising from probable distension of the bladder, ureters, and pelves of the kidneys, having entirely passed away. At the end of a week the first catheter was removed, and at once replaced by another, which had been prepared the day before, of the same size and curve. The catheter was found to have been partially acted upon by the urine, and its calibre somewhat obstructed.

At the end of a month the patient was able to introduce the instrument himself; and at the end of two months, the wound in the perineum having nearly healed, he kept it in at night, leaving it out during the day.

He shortly after left the hospital, well, with the exception of a small aperture not larger than a pinhole, and some weeks subsequently presented himself there on the eve of a voyage to sea.

CASE II. Stricture of the Urethra of Twenty-five Years' Duration; numerous Urinary Fistulæ in the Scrotum and Perineum; Perineal Section; Cure.—E. O. B., shipmaster, entered the Massachusetts General Hospital on April 24, 1860, and gave the following history of himself. He had suffered from gonorrhœa twenty-five years previously, and was treated by his captain at sea by strong injections. The gonorrhœa terminated in gleet, which was treated by bougies for two years, when it was renewed on account of increased obstruction. He then remained pretty well until eight years since, when the strictures became so tight as to give him a great deal of inconvenience, for which he entered the hospital, under my care, and the stricture was divided by internal incision. This relieved the urinary trouble, and for a time he enjoyed good health. Two years since a fistulous opening appeared in the perineum; four months later another in the scrotum; five months since a third; through these openings purulent matter with urine was freely discharged. The whole of the scrotum was tense, indurated, and barred by sinuses. Two months previous to his admission a No. 1 bougie was passed into the bladder, after much effort; his symptoms have been aggravated by it.

I passed a No. 1 bougie down to the stricture, and kept it applied for two or three hours daily, exerting a gentle pressure against the stricture. After treatment for about a week in this way, the stricture gave way, and the instrument passed into the bladder.

On May 1, I made an incision into one of the scrotal openings, and gave vent to a quantity of purulent matter mixed with urine. Notwithstanding the passage of the bougie, the symptoms of trouble about the scrotum increased, and towards the 1st of June, the stricture having again closed, so that it was quite impossible to pass any instrument, the operation was done, at his request, on June 4.

The patient being etherized, and confined in the position for lithotomy, Syme's sound was passed through one stricture, and encountered a second.

An incision was now made in the median line of the perineum, and dissection performed until the point of the staff was reached. The tissues were much hardened, and infiltrated by a deposit, which greatly opposed the progress of the knife. The flow of blood from these diseased parts interfered much with the operation; the perineum also was uncommonly deep. In order the better to bring the continuation of the urethra beyond the stricture into view, the end of the staff was turned out through the incision, and served to hook up and thus bring the deeper parts more fully in sight. The wound being freely sponged with iced water, a puncture was made into the supposed urethra, in the neighbourhood of the neck of the bladder, which allowed the passage of a probe into that organ; and the urine having escaped, showing it had gone in the right direction, a large gum-elastic catheter was substituted for the probe. A sharp-pointed knife was now passed up by the side of a small Syme's sound, and the first stricture, which lay behind the middle of the scrotum, and which would not allow anything larger than the sound to pass, freely divided.

A second gum-elastic catheter was now introduced through the glans and urethra, insinuated into the mouth of the first, which remained in the bladder, and was thus drawn upwards as in the former case.

No great constitutional irritation followed the operation, and the patient on the following day described the comfort of freeing the bladder by a large stream, after having suffered for so many years in his efforts to do so, as beyond belief. The first bougie was left in place a week, then becoming obstructed, it was replaced by another, and this was done weekly through the course of the treatment. The wound in the perineum, on account of the diseased state of the tissues, was very slow in healing, and the patient remained in the hospital for two months afterwards.

A communication was received from him in Nov. 1860, in which he states that he was well.

CASE III. *Traumatic Stricture of the Urethra of Five Years' Duration.*

—G. U., 36 years of age, a teamster, came into the hospital under my care in the early part of the spring of 1860. He said that five years before he had been jammed against a wall by the buffer of a freight car with such force as to produce a rupture of the urethra. He remained in a critical situation for a time, and had never since been able to pass his water, except in a very small stream. Two years previous to his admission, a fistula appeared near the tuber ischii of the left side, between that and the rectum. His water has passed by drops through this fistula, and another in the rectum, ever since. Various attempts have been made to perforate the stricture, but all of them without avail.

On examination by the rectum I found that the bladder, intestine, and surrounding parts were glued together and involved in an indurated mass, and the calibre of the intestine was very much diminished. A probe being introduced into the fistula by the side of the rectum, passed up by the side of the gut through these indurated tissues, and apparently entered the interior of the bladder. Having made very careful attempts for a time to get through the stricture by gentle means, I finally advised him, unless willing to have the perineal section performed, to desist from any further efforts, for fear of producing irritation and complete retention, especially as he did not suffer much from his disease.

In June, having heard of the success of two of the above operations, he came back to the hospital for the purpose of having one performed upon himself.

Operation.—A sound was passed down as far as the stricture, an incision made upon it, and its point reached deep in the perineum. No traces of the urethra beyond could be discovered by the most careful manipulation. He was, therefore, allowed to recover partially from the effects of the ether, and to make an effort to void his urine. An incision was now made in the direction of the neck of the bladder, and a female catheter introduced as a guide, which was replaced by a No. 8 elastic catheter. The parts were very vascular, and the steps of the operation, which lasted over an hour, much obscured by blood.

Not the slightest unfavourable symptom followed; but at the end of the week, when it was necessary to replace the catheter, it was found to be a matter of much difficulty to do so, the end of the instrument escaping into the rectum. This was, however, finally effected both at that time, and afterwards, by hooking the beak of the instrument against the pubes, and then, instead of trying to force it forwards, the handle was suddenly depressed, and it slipped into the bladder. Carried forwards in the ordinary way, it always went into the rectum.

On the 27th, it was recorded that all the urine was discharged by the catheter, although there was purulent discharge through the fistulous opening near the rectum, and occasionally from the rectum itself. The patient remained in the hospital rather more than two months, when he left, and has since been seen well.

ART. IV.—*On Fever and Inflammation.* By J. L. TEED, M. D., of Mendota, Ill.

NEXT to the various processes of healthy nutrition, fever and inflammation present the greatest interest both to the physiologist and pathologist—they enter singly or conjoined, into nearly every condition of disease; primarily or by their results, they lay by far the larger portion of civilized humanity in its grave; they meet us at every turn, and present themselves under nearly every phase: it is no wonder, therefore, that from the earliest days they have engrossed, and still continue to engross so much of the attention of physicians.

Yet these processes are even still mysteries. After all that has been said and written about them, and the innumerable disputations, how little has been reduced to the certainty of science, and what a vast portion of our fancied knowledge still remains hypothesis or conjecture!

Whence the cause? Partly the very difficulty of the subject, and its intimate connection with the processes of life, themselves so little comprehended; and partly because physicians, while yet ignorant of the natural workings of the healthy body, having observed a few more superficial facts of its diseased conditions, have immediately framed hypotheses by which to explain them; when each observer meeting some variances in the facts, and finding some discrepancies in the hypothetical explanations of the others,

has thought it necessary to rebuild the whole fabric from the foundation ; thus hypothesis has been piled on hypothesis, until the student in sheer bewilderment sides with one or with another, without knowing exactly why, and then declares his party the true faith, and brands all others as heterodox or heretic.

These conditions have many things common to both, as well as peculiar to each. Their very names exhibit one of their connecting circumstances—increased heat; in inflammation this is local, in fever general; the part *inflamat*, the whole *fervet*; they arise also from a common cause, therefore they are seldom met with separately, for inflammation is scarcely ever present without fever, and fever, unless periodic or febricula, is seldom observed free from all inflammation; and indeed several pathologists have declared fever to be an inflammation, some of one organ, some of another. Either one, however, may be the primary or the secondary disease. They differ principally in that inflammation is essentially a local disturbance, while fever is a general derangement—inflammation, while consisting of a nervous as well as a vascular element, is yet more particularly seated in the organic tissue itself, starting from which as a centre, it may implicate the whole frame—so as to have been declared a perverted nutrition; while fever has no local habitation, no fixed point of departure, but from the very commencement seems to be a general disorder of the whole organic functions. In inflammation the peculiar product is an excess of fibrin, in addition to the varied results arising from the affection of different organs; in fever the tendency is to a diminution of the fibrin and of the red corpuscles, the results of a general derangement of the secreting functions being added.

Pathologists have endeavoured to condense these processes into that *multum in parvo* a definition; but from their complex nature, this attempt has been generally a failure. Galen described fever as *calor præter naturam*, and Virchow writes, “fever consists essentially in elevation of temperature, which must arise from increased consumption of tissue, and appears to have its immediate cause in alterations of the nervous system.” Copland had previously written, “that the morbid impression (producing fever) is first made upon the organic or ganglial nervous system, and owing to the circumstances of this system actuating the circulating, secreting, and excreting viscera, is manifested in an especial manner by the changes observed in the state of vascular action, in the animal temperature, in the functions of secretion and excretion, in the circulating fluids, and in the other functions which are more or less intimately dependent upon the ganglial system.” Virchow adds, “Every disease may become febrile, every disturbance may form itself into a fever if it extend itself to the centres which regulate the waste of the tissues, and the proper moderating power of the tissue metamorphosis is suspended.”

Dr. Southwood Smith observes, "that the order in which the morbid actions, which constitute fever, occur, is first, derangement in the nervous and sensorial functions, this is the invariable antecedent; secondly, derangement in the circulating function; this is the invariable sequent; and thirdly, derangement in the secreting and excreting functions; this is the last result in the succession of morbid changes. Derangement in the functions of secretion and excretion never comes first in the series; derangement in the nervous and sensorial functions never comes last in the series; derangement in the function of circulation never comes either first or last in the series, but always the second in succession."

Inflammation has been defined as increased sensibility and vascularity of a part, with a tendency to morbid secretion, and consequent disorganization. Dr. Bennett defines it as an exudation of the normal liquor sanguinis, and Dr. Alison as a *tendency* to exudation.

In the number of this journal for July, 1860, I traced the histological arrangement of two of the tissues, the muscular and the osseous, as typical of the whole, to be that of cells in various approximations having one surface in connection with a nutrient vessel of the plasmatic vascular system; both vessel and cell being filled with distinct fluids, which pass from one to the other according to their osmotic conditions; and this interchange of fluids constitutes nutrition. If, therefore, liquor sanguinis osmose from the vessel it must be into the cell, for the wall of a cell forms a part of the limiting membrane of the vessel. One of two things may happen, the fluid may osmose too largely *into* the cells, causing over-distension of them; or it may remain in the vessels and distend *them*; and in addition to either of these, it may rupture the walls of some vessels and cells, and blend the whole into one confused mass: either of these states, except the first, can be caused only by some sudden impediment to the circulation in vessels nearer the heart, as may arise from sudden contraction of the vessels at the seat of the disorder. Should the engorgement come on gradually, a larger amount of plasma may circulate through the part, and exude largely, or osmose into the cells, but becoming organized, hypertrophy will be the result, or some other structural change, and yet be unattended by inflammation. So also an impediment may gradually arise to the circulation in a part, causing obstruction in, and osmosis from the plasmatic vessels, distending the cells, or perhaps even rupturing some vessel and cell-walls without it becoming organized, and yet be unattended by inflammation, as in some dropsical conditions. It seems to be only that local condition, which is attended by an increased formation of fibrin in the fluids, a loss of the contractile power of its smallest vessels with dilation and engorgement, and an irritated state of its contained nerves which should be considered as inflammation.

The nerve, the vessel, the ultimate tissue element, and the fluids in the part are all unitedly concerned in the production of inflammation, nor can

it exist without the implication of them all; no two even of those factors can produce it.

This same histological arrangement, being applied to the term waste of tissue, naturally raises the question, what is the tissue which suffers this increased consumption in fever? If we examine closely the phenomena of nutrition, it can be only the albuminous pabulum of the blood, the similar portions of the cell fluids, the blood-corpuscles, and the adipose deposits in the body, for we have no evidence that the cells, whether muscular, nervous, osseous, or other, are broken up and carried away, but the contrary, unless the forming of fresh Haversian systems in bone be considered such a removal of cells, although it may admit of other explanations. If this increased consumption were a result of a natural or healthy cause, it might be replaced by an increase in the food, as happens in increased exertion, &c. &c.; in fever the increased consumption is accompanied by a diminution of the appetite, and with impairment of the assimilating functions. But while there is an increased consumption, there is no increase in the amount of fibrin contained in the blood, unless some local inflammation be conjoined with it.

We may thus see the important differences between these two processes arising from their elementary composition, and extending through all their relations, treatment, &c. &c. Increased heat is a symptom of both diseases, but its production, as much as that of fibrin, remains an unsettled question; it is chiefly the result of chemical decompositions and oxidations; while the friction of the blood against the vessels may assist in its formation, as also may nervous currents, as is the case with ordinary electricity—however any increase in the quantity of blood circulating through a part will elevate its temperature, and an acceleration of the general circulation is more frequently attended with the same result, if not invariably so—the local increase in the circulation attends always in inflammation, the general as constantly in fever—and to moderate this activity of the circulating system has been a constant desideratum in the treatment of these diseases, whether by venesection, arterial compression, digitalis, the revived veratrum viride, or indirectly by promoting the evacuations. Inflammation as well as fever may arise from causes originating with the body, as well as from those affecting it from without—while particular causes and particular differences in the individual will occasion corresponding differences in the fever or inflammation produced, and thus may arise sporadic cases of those fevers generally contracted by contagion or infection; for in such diseases the *first* case must have originated *suâ sponte*; but in all accelerated circulation and increased heat are constant factors.

Starting from the acknowledged basis, that fever is the result of an effect produced on the nervous system, two methods of treatment have been promulgated; the first is to act directly on the nervous system, and restore its equilibrium; the second is to remove the effete productions until the

cause has ceased to operate, or the incited action has expended itself—the first, attempts to cut the fever short; the second, denies this possibility—in the first secernents, in the second, stimulants are used as adjuvants; of course the main stay of the treatment being the reverse. And notwithstanding the intrinsic differences between them, the same theory and plan of treatment have been adopted by either party in regard to inflammation.

There are many objections to the general use of the means with which we are at present acquainted, that exert a powerful influence on the nervous system; they do not act on all alike, and they are not always controllable in their action—instance the different effects of morphia, strychnia, digitalis, prussic acid, &c., on different individuals.

The second plan limits the formation of effete matter, counteracts its influence while it remains in the system and constantly promotes its removal.

The first method is exemplified in the cure of periodic fever, where a powerful stimulant taken shortly before the accession of a paroxysm, puts a complete stop to the disease. The second plan, indeed, from its very character of expectancy, cannot present such a striking example, but by comparing the general results of the two modes of practice in continued fevers and inflammations, they are greatly in favour of the second.

This free use of stimulants is based on the theory that disease arises always from deficiency of nervous force, either direct or indirect; it ignores the fact so strongly, and so continually dwelt on by Copland, that vital manifestations, or the results of the vital forces, may be vitiated in kind as well as in degree; it looks only at the force acting, instead of also considering the matter acted upon; and forgets that with increased tissue change, is an increased amount of effete products, which by their very presence vitiating the blood stream, produce an unhealthy condition of parts previously unaffected, while the very stimulants poured in increasing the products of decomposition, the nervous centres are reacted upon from their peripheries, organs already overworked, or unable to work from congestion receive fresh excitation, and additional local and general disorder are awakened, under which the disease and the patient are terminated together.

One argument raised in favour of alcoholic stimulants in continued fevers and inflammations, is their alleged power of reducing the action of the heart. But the action of the heart is subject to great variations in these diseases spontaneously, it will vary on different days, and even on different parts of the same day: it is, however, difficult to conceive how a stimulant, which as its ordinary results causes acceleration of the heart's action, and increased fulness in the pulse, will reduce the action of the heart while the pulse remains full, and it is not until the pulse has become *soft* as well as *frequent*, as in the latter stages of continued fever and inflammation, that the use of alcoholic stimulants has been found most serviceable.

The blood plasma circulating around every cell in the body, as well as being the vehicle of nutrient matter to them, is also the channel by which

effete matter is removed from them. In a state of health this nutrient matter is derived from a due manufacture of alimentary substances into the albuminous pabulum of the liquor sanguinis; while the effete matter is removed by the secreting and excreting organs. Paralysis of the organic nerves supplying these glandular structures, is followed by a cessation, or at least by a diminution of their secreting function. The blood stream may pass through them in increased quantity, but the secretion is hindered, and its peculiar constituents retained in the blood. Should this be the case with only one organ, then by the principle of compensation, other organs act vicariously; when, however, the disturbance becomes general, and the secretions of all are hindered, the trouble is increased in such a degree, that life is soon terminated unless they are restored—under the second plan of treatment mentioned above, the object is, from the earliest stages, to promote the action of those organs by which effete matter is expelled, and by abstinence or low diet reduce their work as much as possible. The excreta being capable of division into two classes, the nitrogenous, and the non-nitrogenous, and the chief of the former being urea and uric acid, those remedies which have the most effect in promoting their secretion are of especial value in the treatment of both diseases, especially in their earlier stages, and liquor ammoniæ acetatis, and bicarbonate of potash, or its citrate, have descended to us as heir-looms of great value, although from the researches of Dr. Garrod, lithia and its carbonate appear to possess superior powers to them.

The non-nitrogenous excreta are derived largely from the intestinal canal; but owing to the great tendency to diarrhœa in many of these disorders, and the consequent debility thereby induced, and owing to the fact that the small quantity of food allowable may require longer time than ordinary for its digestion and assimilation, great care must be always observed in the use of purgatives, of which enemata may be in a great majority of instances the better kind; while it is essential that they act, it is also essential that they do not act too much. In both fever and inflammation the action of the skin may be advantageously promoted. Tepid sponging with alkaline solutions, or with diluted alcohol, will be found very effectual, and contribute much to the ease and comfort of the patient. The pulvis antimonialis comp. of the Pharmacopœia, or James' powder as usually called, is by far the best diaphoretic for general use, and when combined with small doses, one-sixth or one-eighth of a grain of acetate of morphia, a little camphor, and nitrate potash when allowable, will often lull a febrile paroxysm after a few repetitions, and induce a copious diaphoresis; a calm and quiet sleep; a moist tongue, and a general improvement with a gentle increase of all the secretions.

Another remedy much used in both these diseases, and in nearly all their stages and kinds, is mercury in one or other of its forms, and no remedy has been so abused by a reckless use, so much so as to have become a by-

word and a reproach to the profession. It is usually given in what are termed alterative doses. This term alterative is exceedingly vague; it means everything or nothing, and justifies the saying of Chateaubriand "that words were invented to conceal our thoughts," and our ignorance, it may well be added. An alterative is a remedy which changes the character of the secretions without increasing or diminishing them. But we neither see this effect produced, nor do we expect to see it in the use of mercury in either fever or inflammation. If quicksilver or any of its preparations is digested with solution of chloride of sodium, decomposition ensues, chloride of mercury, *i. e.*, corrosive sublimate is formed, and soda from oxidation of the sodium by atmospheric oxygen, and this process is facilitated by the addition of a little blood; chloride of mercury is soluble in excess of albumen; and the double compounds, chloride of mercury and albumen, with chloride of sodium or with chloride of potassium, present considerable resistance against decomposition by oxygen. The mercury, therefore, passing into the various cells from the blood stream in these double compounds, as it were, withdraws a large amount of the blood on the one side, and of the tissues on the other, from the action of oxygen as effectually as venesection, while the volume of the blood not being altered, the action of the heart is lessened, having to circulate that amount of unstimulating matter; and the blood itself is at the same time just so much diluted, absorbs so much less oxygen, and is consequently so much less stimulating. There is no necessity in the majority of cases to cause as much mercury to be absorbed as the system will tolerate without disturbance; for it is not until this point is reached that the particular local effects of salivation are produced, and its remedial effects are obtained previously; its approach may be easily recognized, except where the system is very impatient of this mineral; and when we wish to rid the system of it, a rapidly oxidizing agent, as the chlorate of potash, causes its decomposition and discharge.

During the formative stage, or the stage of incubation, any zymosis spreads rapidly through the whole system; while all the excretions being checked, the opportunities for its discharge are more and more lessened; in the inflammatory stage the nitrogenous excreta are increased, and remain above their natural standard as long as this stage lasts, while the chlorides are as constantly diminished, the patient discharging a large excess of nitrogen, over and above that consumed in his food; the carbon and hydrogen excreted not being so easily estimated, but they manifest greatly disordered conditions as evinced by the fetor of the breath, of the stools, and of the sweat. In this stage the circulation becomes accelerated, and the pulse full and bounding, in proportion to the sthenic character of the disease. The connection between this condition of the pulse and the intensity of the disease is so obvious, and the remedy so instantaneous, that bloodletting has been the practice from time immemorial; and in incipient inflammations of a sthenic type under the diminished blood stream, the

pulse will often become soft and compressible, all the secretions will begin to flow, and the patient pass directly into the stage of convalescence. It is only when the case has been of long standing, or the cause of a specific character, or the disease too closely approximating the asthenic or typhoid condition from the first, or too apt to run into it, that bloodletting is inadmissible; but in all inflammations where the vascular element is more involved than the nervous bloodletting—while in the opposite, that is, where the nervous is more involved than the vascular—counter-irritation should be resorted to, and often with considerable freedom. In inflammation of a distant joint, arterial compression in the limb above has been often followed by good effects, the diminution of the blood stream being in this case confined to the part affected.

These diseases form an excellent confirmation of the correctness of Liebig's division of food into plastic and respiratory. We observe, and more particularly so in fever, how largely the nitrogenous portions of the body are removed, as evinced by the large excess of nitrogen discharged over and above that contained in the food; and also by the great amount of caloric set free in the system from the secondary decompositions thereby occasioned; while the large quantity of cold water, and often of ice, consumed is but barely sufficient to render the internal heat bearable. The food in these cases is always extremely scanty, for nature has wisely abolished the desire for it, and weak, thin broths, or whey, are generally preferred, and drinks acidulated by some vegetable acid. The carbo-hydrates and fats, more especially the latter, are extremely ill-suited to this condition of things, and if taken are very apt to produce intestinal disturbances. It has been lately objected to Liebig's view, that fat, salts, and water are as essentially components of the tissues as albumen. This objection is very superficial, for all nitrogenous food contains both fat and salt, and the animal substance void of them, gelatin and its allied substances are not, although rich in nitrogen, capable of being substituted for them. It is not nitrogen that makes *plastic food*, that is not Liebig's point; but that food to be plastic must contain nitrogen. This by no means denies that a body may contain nitrogen, and yet be unfit for food; it only affirms, what no one has yet proved to be false, that animal life cannot be supported on non-nitrogenous food; but may be supported solely on single articles of food, as milk, flesh, bread, and that these articles, besides other things, are very rich in nitrogen; and not only are fever and inflammation in their relations to food, good examples of the truth of Liebig's views, but the more fully these views are understood and carried out by the physician in the treatment of these diseases, and of the periods of convalescence from them, the better will it be for his patients, and the less likely will they be to suffer relapses.

In examining fever or inflammation under general principles, great care should be taken not to select peculiar forms of fever or inflammations of particular organs as types of these diseases in general, by which to guide

our treatment of them as a class. No two forms of fever, and inflammations of no two organs, are to be treated on any stereotyped plan; and it may almost be added, no two cases of either disease. Peculiarities of the individual, and of the circumstances are almost infinite, and will cause variations of treatment correspondingly numerous—and the laying down rules for the use of stimulants, or purgatives, or any other remedy, or the disuse of bloodletting as a constant practice in inflammation generally from a special practice, in inflammation of a particular organ, or in fever, from a particular epidemic, will tend rather to reduce the practice of medicine to a shallow empiricism, than to extend its rational and scientific foundations, and plant the minds of its students and practitioners on those vito-chemical principles which deal with the body as a material reality, and its changes as real material changes; and when these are unknown will lead them, not to invent hypotheses, but to institute investigations to obtain the facts. The present state of physiology has been attained chiefly on the foundation of animal chemistry; for although the microscope has revealed the minute structure, we have not been able to gain thereby a definite idea of the function of the part; but by learning more and more fully the chemical characters of each secretion, and by carrying the investigation into the laboratory, we have obtained information which has yielded valuable results. The same plan in pathological physiology will add greatly to them, and when observers shall take the vito-chemical stand-point, medicine will progress as a science and as an art.

ART. V.—*On the Action of Potash, Soda, Lithia, Lead, Opium, and Colchicum on the Urine.* By WILLIAM MOSS, M. D., of Philadelphia.

THE following experiments were made with the design of determining the influence of the above-mentioned substances over the quantity of the urine, and the amount of its solid, organic, and inorganic constituents, with especial reference to the amount of urea and uric acid. They were all performed upon myself during the winter months of 1860–61.

My age is 27 years; height 5 feet 5½ inches; weight 125 pounds; health good. I regret that it was impossible for me to regulate accurately the quantity and proportions of my food, and the amount of exercise, further than to make them, in a general way, as uniform as possible.¹ This disadvantage necessarily attends experiments extending over several months, and is common to most of the previous experimenters on this subject. The quantity of urine was determined in cubic centimetres, and the weight of

¹ I take two meals daily: breakfasting, *à la fourchette*, at 9 A. M., drinking at this meal strong black tea; and dining at 5 P. M.

the solids in grammes. The method employed for the determination of the amount of urea, was the volumetric analysis of Liebig. The amount of uric acid was determined by quantitative analysis, as the accuracy of the volumetric analysis of Dr. Scholtz is not as yet sufficiently authenticated. To determine the whole amount of solid matter, I subjected ten cubic centimetres of urine to a gentle heat, in a large air-bath, during forty-eight to seventy-two hours, and completed the evaporation, in vacuo, over sulphuric acid. The residue was accurately weighed, and the whole amount found by simple proportion. By heating the residue, mixed with nitric acid, until the carbon was consumed, and subtracting its weight from the whole weight of the solids, I ascertained the respective weights of the inorganic and organic solids.¹ The specific gravity was determined with a specific-gravity bottle weighed in a delicate balance.

The medicines were all ingested, with the exception to be mentioned hereafter, at one hour before breakfast, one hour before dinner, and before retiring for the night; *i. e.*, at 8 A. M., 4 P. M., and 12 P. M. The urine was collected from 8 A. M. to 8 A. M.

Normal Urine.

DATE.	Quantity of urine.	Total solids.	Organic solids.	Inorganic solids.	Urea.	Uric acid.	Specific gravity.
Nov. 13, 1860	1100	50.72	38.17	12.55	28.93	.284	1019.60
" 14, "	752	44.10	34.83	9.20	29.98	.257	1019.00
" 15, "	948	61.83	48.66	13.16	36.97	.306	1026.00
" 16, "	838	58.24	46.57	11.67	33.97	.382	1036.00
" 19, "	757	53.01	44.10	8.90	27.98	.319	1027.00
" 20, "	689	54.58	46.47	8.11	33.09	.430	1028.00
" 21, "	703	48.32	39.08	9.24	25.87	.371	1027.00
" 22, "	611	41.33	33.61	7.71	22.47	.310	1028.00
" 23, "	812	50.15	38.65	11.50	30.48	.455	1026.90
Average	801	51.36	41.12	10.22	29.97	.346	1028.61

Acetate of Potassa.—Of this salt, I took an ounce per diem, dissolved in an ounce of water, and divided into three equal doses. During its administration, I carefully abstained from increasing the daily amount of food and water ingested. The urine passed was of a pale straw colour and strong alkaline reaction.

DATE.	Quantity of urine.	Total solids.	Organic solids.	Inorganic solids.	Urea.	Uric acid.	Specific gravity.
Nov. 24, 1860	1400	73.18	45.69	27.47	32.609	.317	1023.8
" 26, "	1305	74.59	50.87	23.65	34.256	.564	1025.6
" 28, "	897	55.13	35.67	19.46	26.469	.231	1026.4
Dec. 1, "	1198	65.44	45.15	20.28	35.997	.404	1025.3
Average	1200	67.085	44.345	22.715	32.332	.379	1025.27

¹ There is probably some slight excess in the weights of the solids and the organic constituents, as it was impossible to expel the whole of the water by this process.

Acetate of Soda.—An ounce of this salt was taken daily, in the same manner as the preceding one. The urine passed was of a pale straw colour and alkaline reaction.

DATE.	Quantity of urine.	Total solids.	Organic solids.	Inorganic solids.	Urea.	Uric acid.	Specific gravity.
Dec. 3, 1860	981	58.90	40.68	18.21	32.856	.082	1028.9
" 4, "	1000	55.44	42.49	12.95	28.821	.103	1025.5
" 5, "	1010	54.27	34.58	19.68	26.351	.297	1025.8
" 6, "	1090	60.05	36.61	23.43	29.633	.199	1027.1
Average	1020	57.165	38.59	18.56	29.415	.170	1026.82

Acetate of Lead.—The ingestion of this salt lasted four days, during which I took nine, eight, nine, and nine grains, respectively, per diem. It induced thirst, constipation, and finally slight griping. The urine passed was acid and high coloured. It was tested daily, with sulphuretted hydrogen, for lead—which first appeared in the urine of the third day. On the fourth day, also, it was present. On the fifth day, on which I took no lead, repeated examinations failed to show its presence. On the sixth and seventh days, I took five grains of iodide of potassium per diem, and obtained faint traces of the lead; after which, it did not reappear.

DATE.	Quantity of urine.	Total solids.	Organic solids.	Inorganic solids.	Urea.	Uric acid.	Specific gravity.
Dec. 8, 1860	800	52.43	41.50	10.93	28.704	.398	1024.4
" 10, "	797	49.43	39.22	10.20	25.410	.398	1024.0
" 11, "	721	48.16	38.37	9.79	27.104	.349	1025.9
" 12, "	773	51.86	37.44	14.41	30.332	.039	1028.0
Average	772	50.47	39.13	11.33	27.887	.296	1025.57

Carbonate of Lithia.—Fifteen grains of this salt were taken daily; grs. v, water f3j. The urine was of a pale colour and faint acid reaction.

DATE.	Quantity of urine.	Total solids.	Organic solids.	Inorganic solids.	Urea.	Uric acid.	Specific gravity.
Dec. 20, 1860	1146	60.03	44.16	15.86	30.526	.431	1021.0
" 21, "	1500	73.83	50.90	22.92	36.908	.457	1019.6
" 22, "	1151	55.41	44.09	11.32	31.223	.409	1018.0
" 24, "	1253	56.02	43.31	12.71	29.999	.186	1016.4
Average	1262	61.32	45.61	15.70	32.164	.370	1018.75

Opium.—During this series, I took for two days one and a half grains of opium per diem, in pill form, in three doses; and, for the two following days, two grains daily, in four doses. Urine high coloured, reaction strongly acid. The dryness and thirst from which I suffered while under the influence of this drug, were so great that I was unable to refrain from largely increasing my ingestion of water.

DATE.	Quantity of urine.	Total solids.	Organic solids.	Inorganic solids.	Urea.	Uric acid.	Specific gravity.
Dec. 27, 1860	1139	58.09	46.41	11.61	27.98	.104	1020.0
" 28, "	1300	57.00	48.24	8.75	29.62	.277	1015.3
" 29, "	1209	52.22	39.48	12.74	25.16	.046	1016.8
" 31, "	1052	51.41	37.67	13.73	27.23	.340	1026.3
Average	1175	54.68	42.95	11.70	27.50	.191	1018.35

Colchicum.—Sixty drops of the wine of the fresh root were taken daily. Urine of strong acid reaction.

DATE.	Quantity of urine.	Total solids.	Organic solids.	Inorganic solids.	Urea.	Uric acid.	Specific gravity.
Jan. 6, 1861	899	57.94	44.82	13.11	33.817	.384	1027.0
" 7, "	806	54.50	43.67	10.82	31.027	.222	1027.3
" 9, "	1029	54.67	41.61	13.01	27.872	.204	1020.8
" 10, "	900	54.50	40.51	13.98	28.908	.507	1024.2
" 11, "	816	53.31	40.56	12.74	27.151	.331	1026.9
Average	890	54.98	42.23	12.73	29.755	.329	1025.24

Table of Averages.

	Quantity of urine.	Total solids.	Organic solids.	Inorganic solids.	Urea.	Uric acid.	Specific gravity.
Average of Normal urine	801	51.36	41.12	10.22	29.97	.346	1028.61
Acetate of potassa	1200	67.08	44.34	22.71	32.33	.379	1025.27
Acetate of soda	1020	57.16	38.59	18.56	29.41	.170	1026.82
Acetate of lead	772	50.47	39.13	11.33	27.88	.296	1025.57
Carbonate of lithia	1262	61.32	45.61	15.70	32.16	.370	1018.75
Opium	1175	54.68	42.95	11.70	27.50	.191	1018.35
Colchicum	890	54.98	42.23	12.73	29.75	.329	1025.24

It will be seen, on reference to the above table, that the amount of normal urine secreted by me was far below the usual average, which is from 1400 to 1600 cubic centimetres. During this first series of investigations, the weather was cold, and, with the exception of one rainy and one cloudy day, remarkably clear and dry, with a thermometrical range of from 42° to 55° F.; so that the small amount of water cannot be due to increased action of the transpiratory function. I subsequently repeated my measurements with the same results, the average for three days being 824 cubic centimetres. The normal averages for the solids, urea, and uric acid, are below the medium found by previous observers, but not disproportionately to my weight and size. The high rate of the specific gravity was caused by its unaccountable, and, I may add, vexatious, rise, on the fourth day of the series, to 1036; a number so clearly abnormal that I consider myself justified in eliminating it from the series—the average of which without it would be 1025.18.

The experiments with soda, potassa, and lithia, were conducted with especial reference to the relative influence of these alkalies over the elimination of uric acid from the system in the gouty diathesis. Under the use of potassa, the excretion of urine was increased by one-half, with a notable augmentation in the amount of all its different solid constituents. The soda increased the amount of water, while the amount of organic solids was decreased, and that of uric acid was diminished one-half. The augmentation in the amount of inorganic matter is attributable to the egestion of the alkali itself.

The results of the experiments with lithia are confirmatory of the high praise awarded by Dr. Garrod¹ to the action of this agent. Although taken in infinitely smaller quantity than the potassa, the effects produced by it, with the exception that it failed to render the urine alkaline,² were almost precisely similar. The amount of organic matter excreted during its ingestion was even greater than during that of the potassa, while the difference in their average weights of uric acid was less than a centigramme. Dr. Garrod, I believe, first called attention to the fact that, during the ingestion of lead into the system, the excretion of uric acid is diminished. I also found this to be the case, though to no very considerable extent; but, as it will be seen on reference to the table that the amounts of water, organic matter, and urea were likewise diminished—the inorganic matter alone undergoing no diminution—it is a fair inference that lead exercises no specific influence over the uric acid, and that the decrease is but incidental to the general diminution of organic activity caused by this metal.

The value of the results of the experiments with opium was doubtless affected by the large quantity of water ingested during its administration, and to which we may attribute the increase in the amount of water and solids excreted. Of these last, however, the amounts of urea and uric acid fell—the latter by nearly one-half.

The result of the experiment with colchicum agrees with those obtained by Drs. Krahmer and Garrod—viz., that colchicum is not a diuretic—and differs from that of Dr. Hammond; for, although there was an increase in the quantity of urine and of the organic and inorganic matters excreted, it was mainly in the amount of inorganic constituents, and was too inconsiderable to be considered an effect of the medicine, while the amount of urea was unchanged, and that of uric acid even diminished.

I had intended to have closed these investigations with comparative experiments on several of the other salts of the alkalies, but they have already occupied so much more time than I had anticipated, that I am reluctantly forced to defer their completion until some future period.

¹ Garrod on Gout and Rheumatic Gout.

² I have since taken thirty grains of the carbonate of lithia at one dose, with the effect of rendering the urine passed three hours afterwards strongly alkaline.

ART. VI.—*New Method of treating long standing Dislocations of the Scapulo-clavicular Articulation.* By E. S. COOPER, A. M., M. D., Prof. of Anatomy and Surgery in the Medical Department of the University of the Pacific.

THE difficulty of effecting a cure even in recent cases of dislocations of the scapulo-clavicular articulation without deformity is well known, and in those of long standing treatment has heretofore seldom, if ever, been attended with complete success.

Prof. S. D. Gross says: "Dislocations of the scapular extremity of the clavicle are always attended with difficulty, and it very rarely happens that they get well without some degree of distortion. Prof. Boyer, one of the most able and copious writers upon surgery of the present day, relates a case in which the patient, after five months' regular treatment, could not move his arm without causing a relapse of the complaint."—*Anat. Physiol. and Diseases of the Bones and Joints*, p. 255.

This joint admits of the smallest possible degree of motion appertaining to any joint in the natural condition, and the plan of treatment now to be explained was based upon the supposition that a bony union between the end of the clavicle and acromion process of the scapula could confer little or no inconvenience, but would at the same time remove the deformity; and having long since discarded the favourite doctrine that air admitted to bones produces irritation and a tendency to necrosis, I decided upon the practice shown in the following cases.

CASE 1. Mrs. M. A., a native of Wales, æt. 36, mother of several children, consulted me in 1856 for a dislocation of the acromial extremity of the left clavicle of several years' standing, which had impaired the usefulness of the arm of that side to a very great extent. She had consulted several surgeons in Wales, one of whom used compression upon the elevated extremity of the clavicle for several months without avail. Two years afterwards she consulted some of the most eminent surgeons of London, but without receiving any permanent benefit, though she wore an apparatus recommended by one of them for several months. Subsequently coming to the United States, she consulted different surgeons, none of whom, however, advised any effort to be made to remove the deformity.

She had a nervous desire to get rid of the deformity amounting almost to insanity, and entreated me to do something for her. I proposed cutting down upon the bones, drilling them, and applying ligatures of metal to hold them together. To my surprise she accepted the proposition, not only with promptitude but apparent enthusiasm. All the *supposed* danger of admitting air to bones was fully portrayed to her, as founded upon the authority of the greatest practitioners for hundreds of years, but all had no effect. Her husband and herself would take all responsibility, she said, bearing in mind an operation which had been performed upon her nephew, where metallic ligatures were applied to the tibia, and to which the husband consented.

Although from my experience with ligatures applied to bones I confidently calculated upon success in the case, it was desirable to have the parties fully informed as to what others would say should any untoward result follow, against which we cannot safely guarantee in any operation, however simple, and which is liable to fall particularly heavy upon the surgeon who performs an experimental operation upon principles opposed to the opinions of the profession.

Operation.—The operation was commenced by making an incision three inches long, commencing a little external to the border of the acromion process of the scapula, and following the direction of the clavicle. A transverse incision of two inches was next made, passing over the elevated extremity of the clavicle. The flaps were then dissected away, the bones exposed, their articulating faces trimmed with the bone scissors, and then bored with a drill one line and a half in diameter, about half an inch from the articulating surfaces. The drill was passed through obliquely from above in either bone, the end of the drill being directed towards the opposite one. A wire one line in thickness, made of virgin silver, was then introduced, and a firm knot formed by twisting the ends together immediately over the articulation. Two healthy bony surfaces being brought together and held firmly there, I calculated most confidently upon a speedy osseous union, which turned out to be the case.

Considerable difficulty was encountered in passing the wire from one bone to the other. By having the end well sharpened, however, and bent, after several trials it was passed through the holes in the bones.

After-treatment.—The after-treatment consisted of applying a piece of lint wet with an evaporating lotion (composed of one part of alcohol and ten of water) in the wound around the ends of the wire, which were then bent down over the margin of the wound, over which was placed a piece of wet lint. A roller was then applied over the arm and shoulder of that side as tightly as the patient could conveniently bear. The dressings about the wound were kept constantly wet with the evaporating lotion for four days, when they were removed and a poultice applied, after which the wound granulated kindly, and without an untoward symptom the patient recovered perfectly, without deformity.

At the end of six weeks the wires were untwisted, one end cut off by passing the bone scissors down to the side of it nearly by the bone, when the other end was readily withdrawn.

As soon as the wound began to suppurate freely a movement of the wire was commenced by carrying the ends back and forth. This was done for the double purpose of keeping the wound slightly open for the discharge of any spiculæ of bone which might be thrown off, and which not unfrequently occurs in all operations upon the bones and for facilitating the final removal of the wires.

The motion of the arm is as good as ever, and at the end of nearly three years there is no indication of a return of the deformity.

Remarks.—There were no sutures used to bring the soft parts together in this case, which is in accordance with my common practice in all operations upon the bones unless the wound be necessarily over four inches long, and in all cases I fill some part of it with lint, so that a communication may be kept up between the surface and the bone operated upon, until it is apparent that there is no longer danger of any portions of bone exfoliating,

which might otherwise be confined deep under the surface and cause great irritation.

The application of lint in the wound, when pressed tightly against its surfaces by means of a roller, is used for these purposes, viz: 1st, to prevent secondary hemorrhage; and 2d, to condense the tissues and produce firm adhesions between all the parts constituting the surface of the wound, so that purulent matter cannot escape into the adjacent tissues; and to accomplish this object a roller applied as tightly as the patient can conveniently bear, is, I think, absolutely indispensable. The lint is permitted to remain in the wound from five days to three weeks; it is generally, however, removed at the end of the first week.

What are the sources of danger and death in all the various operations upon the bones? A few die of a shock to the nervous system, or of immediate supervening local inflammation; but these cases are very rare. Nearly all the suffering and fatality occurring after operations upon the bones arises from the burrowing of purulent matter, either with or without secondary disease of bone. The lingering but severe prostration and fatal constitutional irritation so frequently attending, is nearly always dependent upon this cause, which will in most cases be removed by the plan above recommended.

I do most earnestly commend this matter to the serious consideration of the medical world, because in this city, where such an unusual number of cases of diseases of bone requiring operations are constantly occurring (owing to the habits and avocations of our people), death scarcely ever takes place even in operations upon the femur; while in some parts of Europe one case in five or seven terminates fatally of all the different operations upon the bones of the upper and lower extremities where any considerable amount of bone is removed.

My plan in all these operations is to keep the ends of the bones operated upon in sight until they are covered with healthy granulations, indicating that exfoliation or further disease of bone is no longer to be feared. The confinement of a small piece of dead bone in the centre of a limb would be amply sufficient to cause a very high grade of constitutional and local irritation, and not unfrequently hectic fever and death. Many limbs, and lives too, have been sacrificed from this cause, which would generally be obviated by the plan recommended.

CASES 2 and 3. The first of these was operated upon one year after the one just detailed. The deformity was one of three years' standing. Treatment and results the same as before.

CASE 3 was operated upon in January of the present year (1860). The disarticulation was of six months' standing. Operation, treatment, and result the same as in the first two cases. I am well aware of the great opposition to the above practice which is likely to be raised by members of the profession, in which it is universally admitted that atmosphere admitted

to bones is apt to be a source of great injury to a patient; but I recommend it with confidence, and am satisfied to rest the justification of its claims to value upon the statistic of results in future operations. But the after-treatment must be the same as practised in these cases.

ART. VII.—*Some Account of Diphtheria as it occurred at Oakland College (Miss.), and Vicinity.* By R. H. GOLDSMITH, M. D.

THE latitude of this locality is about 32° N. The country is hilly, and its altitude from fifty to seventy-five feet above the Mississippi River. The soil is clayey, and except in the bottoms, poor. The woods are gradually disappearing, but in the bottoms the undergrowth is rank and luxuriant, with many willows, dogwood, cane thickets, &c. The soil being clayey, retains much moisture: and, although not marshy, is constantly in a saturated condition. In summer, when the thermometer attains an average range of 90° , the most noisome malaria is generated in these bottoms, and is carried over the hills by every wind. This heat continues for three full months, with a fall of the thermometer of 10° to 15° every night, during August and September. As a consequence, during these months the dews are heavy, and the putrefaction of decaying vegetable matter rapid and abundant.

The summer of 1859 was a season of malarial fevers, severe in form, with a typhoid tendency, and extremely difficult to treat. We had a singularly dry summer, although the river had overflowed the flat lands along its whole extent. The winter following presented a series of atmospheric changes, such as the South has never before known; thermometrical records exhibited the highest and lowest temperatures ever recorded in the State. An abundant snow, with the thermometer below zero, were the precursors of the most virulent and malignant diphtherial epidemic ever seen in *any locality*.

Diphtheria has been endemic in this vicinity for three years, and epidemic since January, 1860. It has varied in severity, from the slightest deposition of exudatory membrane, to the complete plugging of the larynx and trachea; yea, I may add, with the deposition of diphtherial membrane from *fauces to anus*. I propose to give the history of a few cases, from a record of three hundred which have occurred in my practice alone. On a single plantation, in January, I count one hundred and twenty cases, and, from this place, I select, at random, cases demonstrative of different shades of the disease.

CASE I. Called on Jan. 2, 1860, to visit Lizzie, a black woman, aged 25, in the following condition: Voice thick and husky, complains of inability

to swallow; eyes red, and capillaries filled almost to bursting; pulse 120; skin dry and harsh, with a most pungent heat. High fever; bowels constipated. On examining throat, I found it covered with an ash-coloured, gangrenous-looking deposit, so thick as to impede respiration, the air passing through, causing a sound like that of a child's whistle. This, I take it, is the ordinary form of diphtheria, allowed to run on to this stage without treatment.

Treatment.—I immediately swabbed her throat with a solution of nitrate of silver (gr. xx to the ounce of water). For this purpose, I use a large and also a very small size probang. The large probang has a sponge, ball-shaped, one inch in diameter, and will take up a tablespoonful of the solution. The small probang I use for washing the œsophagus and larynx, when the deposit reaches these localities. Here, let me remark that, in all my cases, I have never had occasion to regret immediate application of the caustic; on the contrary, the only case that I lost on this place, died because I had no proper instrument with which to make a thorough application of the caustic. My plan has been (and experience confirms my method), to apply the caustic solution as soon as possible, regardless of the amount of deposit. The first application enables the patient to breathe by removing the deposition, and the application is to be reapplied, according to circumstances, the object being to prevent the re-forming of the false membrane. To resume, I also gave the following solution: R.—Saturated solution of chlorate of potash ʒxij; Tinct. sesquichloride of iron ʒss; Syrup ʒij. Mix. Take a tablespoonful every two or three hours. She is also to take a teaspoonful of brandy every hour, mixed with a little sweetened water. I have never yet seen a case in which the prompt administration of stimulants was not only beneficial, but *absolutely abortive* in their action on the disease. The fever and complications are of a typhoid character—the poison like that of typhus—and, consequently, the treatment should be tonic and stimulant. This woman was also allowed a liberal quantity of beef-tea during the day, with an opiate at night. This constituted the whole treatment, and convalescence was prompt.

CASE II. A boy named Branch, aged about 16, a stout, healthy negro. He presented that most alarming symptom, a picking at the bedclothes, constant desire to throw off bedclothes; a low, muttering delirium; he passes his feces involuntarily. Pulse 150, compressible, gaseous. Heart labouring violently to propel its blood. He has had ten or twelve dejections during the last twelve hours. They resemble the deposit on the throat, with the addition of blood. Tongue of a fiery red, with a black, dry crust in centre. Respiration reduced to a whistle, varying from 20 to 30 in the minute. Sordes about teeth; fauces covered with the diphtheritic deposit, as far down as I can see. I had no hope of this patient, but I went briskly to work. Washed out the throat *well* with small probang, touched the *rima glottidis*, and far down the œsophagus, and brought up a lump of the deposit as large as a hickory nut, which resembled cotton, painted gray. The boy breathed freer immediately, and, with some difficulty, swallowed. I gave the following: R.—Carb. ammonia ʒj; Æther sulphur. ʒiv; Tinct. zingib. ʒij; Spts. vin. gall. ʒiv. Mix. Dose, a dessertspoonful in a little water, every half hour.

To check diarrhœa: R.—Plumb. acet. gr. x; Tinct. opii gtt. xx; Starch water ʒij. Use at once, and repeat every two or three hours, *pro re nata*.

At 5 o'clock of this day (Jan. 6), the boy is more quiet; operations not so frequent. I reapplied solution of caustic, and gave the chlorate of potash mixture, as in Case I., and the following injection *per anum*: R.—Nitrate of silver gr. j; Laudanum gtt. xx; Starch water ℥ij. Mix. Use at once, and repeat in three hours.

I continued a similar course for seven days, adding various tonics as the indications demanded, and giving brandy as freely as the boy could take it. He made a perfect recovery.

CASE III. A little negro boy aged 7 years. I found him with eyes red as fire, spitting mouthfuls of blood; prostrate. I washed his mouth with water, to examine throat, &c., and, to my astonishment, beheld blood oozing from gums, tongue, and whole interior of throat. During his attack he lost, at least, a pint. He had profuse diarrhœa. I may here remark that the worst cases are those accompanied with diarrhœa, as it soon exhausts the patient, and is difficult to check. I pursued my usual plan, using nitrate of silver, brandy, beef-tea, &c.; subcarb. bismuth and morphia to check bowels. This child recovered.

CASE IV. I mention because of a most untoward complication, namely, paralysis of muscles of neck, &c. All symptoms of the disease were checked, deposit had disappeared, and I regarded the little patient (a child aged 8 months), as convalescent. Suddenly the paralysis occurred, and all known remedies were exhausted, without avail. It died.

CASE V. The last case I will mention is that of a mulatto boy, aged about 14. He had all the worst symptoms from the time he was taken sick; was delirious; the secretions from throat at each expiration, were forced through the nose. I had no probang with me, but made a rude one, and applied caustic solution as thoroughly as the instrument permitted. In about eight hours used the proper instrument, with much relief to patient, but he was suffocated by covering his head with the bed-clothes, a practice which our negroes have in sickness and health. I sustained him for days before he died, with stimulants by mouth and bowels, and think he too might have been saved, but for the accident.

Remarks.—I believe that this disease is produced by some specific poison, being similar to a disease in cattle, the black tongue. This cattle disease—black tongue—prevailed among the stock of our neighbourhood, whilst diphtheria was raging, black tongue preceding diphtheria. It is certainly a remarkable coincidence that the country adjacent to the plantation on which the black tongue prevailed, was most affected with diphtheria.

On the same plantation, every possible shade of the disease occurred, together with diseases which I regard as cousin-germain, namely, typhoid pneumonia, typhoid fever, and two cases of pure typhus. This plantation is well managed, the houses new, the negroes well clothed, well fed, and yet the whole number of servants—one hundred and fifty—were attacked. They were taken sick just after the Christmas holidays, and after the unusual luxuries consequent on that festival. Among other things, *beef* formed one of the principal staples of their feasts, and this, again, is a striking coincidence, as I took the trouble to ascertain who were the beef eaters, and found they were my first patients.

I regard the disease as *in the highest degree* contagious. Many experiments were made with the view of ascertaining this fact. When an isolated case would occur, I shut the patient in a room with the other sick, and removed all others from the sick quarter. All communication, as far as possible, was prevented between the sick and well, and yet not a single person escaped. Servants from other plantations came to this one, and carried the disease to their homes. I shut them up, in their turn, cut them off from all communication with their fellows, and succeeded in keeping the disease to a single house, when far away from the "Black Tongue" district.

If recognized early, this disease is perfectly amenable to treatment. Since the epidemic, the author has treated two hundred and seventy-five cases, and, of these, but five died, three of which might have been saved if they had been seen early. There are two diagnostic symptoms by which this disease can be surely known, namely, a fiery eye, and a blue, congested appearance of upper lip; the latter symptom is especially apparent among children. A view of throat is, of course, positive.

Treatment must be prompt, immediate, and untiring. Thorough application of a solution of nitrate of silver, of proper strength, at all stages of the disease, and as much brandy as the patient can digest. The chlorate of potash and muriated tincture of iron are also highly useful; the latter, especially, stands high in my estimation, forming as it does almost a specific in an analogous disease, erysipelas. Depressing remedies are worse than useless, nay, accelerate death. Blisters, and all outward applications, are highly injurious; the only one used in this epidemic, a strong solution of iodine, I am not entirely satisfied with, as it sometimes produced frightful ulcerations. If the patient cannot swallow, the following enema may prove beneficial: Port wine ʒj; quinine gr. v to x; beef-tea ʒij—to be used milk-warm every two or three hours. We are never permitted to despair in this disease; the simple passage of the probang has saved patients who seemed in articulo mortis, and free stimulation perfected the cure. There are many other remedies recommended, and may be tried; the simple course laid down above, has answered for the epidemic seen by the author..

OAKLAND COLLEGE, Miss.

ART. VIII.—*Case of Transverse Fracture of the Patella, successfully treated by means of Malgaigne's Hooks.* By JOHN H. PACKARD, M.D., of Philadelphia.

ON the 28th of November, 1860, I was requested by a friend engaged in an exclusively medical practice to take charge of a lady who had that morning sustained a fracture of the left patella. She had, in going down

stairs with her child, two years old, in her arms, thought herself at the bottom of the flight when she had still a step to descend; she fell, and found that, without to her knowledge the receipt of any blow, the patella had given way. A physician living in the neighbourhood was called in at once, and applied a straight board at the back of the limb, with compresses and a bandage to maintain the fragments in apposition. When her own physician saw her, in the afternoon, the swelling and pain were so great that he directed the application of forty leeches around the joint; the bites bled freely, but there was still great tenderness when I visited the patient with him in the evening.

Upon careful examination, we now made out that the fracture was transverse, and the lower fragment small; it was impossible to define the exact extent of separation of the two portions, but they did not seem to be more than three-quarters of an inch or an inch apart. The compresses were replaced by strips of adhesive plaster applied transversely so as to fix the lower fragment and draw down the upper; a bladder partly filled with pounded ice was laid over the knee, and morphia freely given. This plan of treatment, with very slight modifications, was adhered to for several days.

Mrs. J., the patient, was a very fleshy person, about thirty years of age, and accustomed to somewhat high living. She soon began to feel the effects of pressure upon the bony points as she lay upon her back, and to suffer from constipation. This latter difficulty seemed partly owing to her supine posture. Hence it became very desirable to employ some means by which the coaptation of the fragments should be insured even without absolute quiet.

On the 6th of December, at 11 A. M., eight days after the occurrence of the accident, I inserted the hooks invented by M. Malgaigne for the treatment of these fractures. The inflammation had almost entirely subsided, and I had the full consent of the patient, as well as of my colleague, to the experiment. A good deal of pain seemed to be caused by the application of the instrument, but by the external use of lead-water, and the exhibition of morphia internally, it was assuaged in great measure. The adhesive strips were reapplied, for the sake of safety, and the posterior splint and the bandage were retained. By measurement, the length of the patella was found to be the same as that of the sound bone, the hooks being firmly opposed to one another. At 8 P. M. the patient was free from fever, and there was no evidence of anything unfavourable locally.

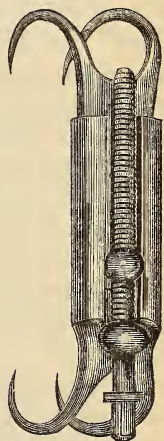
On the 11th, five days after the insertion of the hooks, I endeavoured to tighten them by a few turns of the screw; but they were as closely approximated as they well could be. A few days after this, the splint was found to be unnecessary, except as affording a convenient resting-place for the limb; and the bandage was only very loosely applied. On the 21st a trifling degree of soreness and swelling was present, especially about the lower pair of hooks; a rag wet with lead-water was laid over the part, and next day an ointment composed of equal portions of Goulard's cerate and extract of belladonna was applied, after which there was no further trouble.

On the 6th of January, 1861, the thirty-first day from the insertion of the hooks, and the thirty-ninth from the occurrence of the fracture, the instrument was removed. Some swelling of the integuments was apparent, but the bone seemed perfectly solid. The patient was directed to remain in bed, but to use as much motion of the knee-joint as she could, with a view to the restoration of its flexibility.

By the 1st of February Mrs. J. was walking out in the street, having, of course, gained confidence by previous attempts in her room and about her house. She had no sense of insecurity in the limb, and the joint was daily becoming more supple, although, as might have been expected, by slow degrees.

So far as I am aware, the case now detailed is the first one in this country in which trial has been successfully made of M. Malgaigne's plan.¹ The remarks which I have to offer in connection with the foregoing account will have reference, in the first place, to the plan itself; in the second place, to its supposed dangers; and lastly, to its real advantages.

A description of the instrument may not be out of place, since to some of the readers of the *Journal* it may be unfamiliar. The one I used is larger than the one described by Malgaigne in 1847. It is composed of two plates of steel, three-quarters of an inch in width and a quarter of an inch in thickness. Each of these plates is at one extremity recurved downwards into two very sharp hooks an inch and a half long, the points of which are parallel with the plates themselves. The upper plate is two inches and a half in length before the curve of the hooks becomes decided; the lower one is shorter than this by an inch. Projecting up from the lower plate, about three-eighths of an inch from the notch between the hooks, is an upright, which runs in a slit rather more than an inch and a half long in the upper plate; at the extremity of this slit is another upright, fixed in the upper plate, and these two uprights are pierced by a screw which runs parallel to the plates, forcing them, and therefore the points of the hooks, together. The screw is squared at one end, so as to be turned by means of a key.



The lower pair of hooks are to be inserted first, the skin being drawn slightly downwards; they should catch the edge of the tip of the lower fragment, on either side of the ligamentum patellæ. Next the upper plate is slipped on to the lower (which is turned up for the purpose), and its hooks forced in so as to pierce the skin at right angles, and engage the

¹ Dr. Ellis, of Boston, in answer to an inquiry from me in regard to Malgaigne's hooks, says, "They are not liked here, and are never used." Dr. H. D. Noyes, of New York, writes me that in 1854 he saw a case treated with them by Dr. Buck. They produced so much inflammation at the end of two weeks as to require removal. Abscesses subsequently formed around the joint, and the treatment was prolonged to twice the usual period. Bony union took place, but the joint remained quite rigid until the patient was lost sight of. "This case was pretty generally known among New York surgeons, and its results proved condemnatory of the hooks."

points in the upper edge of the upper fragment of the bone. The screw is now passed through the uprights, and turned until the fragments are brought as firmly together as may seem safe. When screwed home, the opposing points are rather less than an inch and a half apart.

All that is necessary to secure the mechanical effect of this instrument is to engage the points properly. The slipping of the upper hooks, spoken of by M. Malgaigne as apt to occur at about the seventeenth to the twenty-second day, did not take place in my own patient, nor do I see how it is possible if the points are rightly applied. One precaution should be taken: to guard the screw by some arrangement of hoops, lest it be subjected to any sudden wrench. Supporting the limb by a posterior splint, although not essential, is, of course, judicious.

In the second place, as to the supposed dangers of this plan. The worst of these would, of course, be that of perforation of the knee-joint by the hooks. But this, as my friend Dr. Brinton and myself have proved by experiment upon the dead subject, is an anatomical impossibility. We not only applied the instrument as for a fracture, but we attempted to drive the points in after the joint had been opened along the lateral edge of the patella. Hence the dread of such an accident in practice may be abandoned.

Another danger that might, *a priori*, be anticipated, is that of ulceration. But my patient had nothing of the kind; there were not three drops of pus discharged from all four of the perforations made in the skin, although around each hook a small circle of induration existed. Nor is there any greater risk of necrosis, since the points do not really enter the bone.

As to the pain caused by such an instrument, it is limited to the few hours succeeding the first application. My patient declared throughout that she felt quite as comfortable as she had when the adhesive strips only were employed. And if necessary, in a nervous or timid subject, anæsthesia might be resorted to when the hooks were put in place.

Lastly, as to the advantages afforded by the plan. It must be obvious that in no other way can we act so directly upon the fragments, or secure them so firmly in apposition; in fact, every other plan is more or less open to the objection that the pressure exerted falls mainly upon the tip of each fragment, so as to tilt the opposed surfaces upwards, and create a V-shaped gap between them. Nor can we by any other means enable the patient to sit up in bed or in an easy-chair, without risk of affecting the closeness of the subsequent union. Moreover, it is easy at any time to test the accuracy of the coaptation, by trying whether the bone and the instrument move as one mass. And finally, we may dispense in a great measure—in fact, almost entirely—with the heating and galling constraint of a firmly-applied bandage, so necessary in most cases of fracture of the patella; or, at least, this may be left off at a much earlier stage of the treatment than when the usual method is employed.

I have dwelt more fully upon the plan proposed by M. Malgaigne, because in the two latest and most authoritative American works bearing upon fractures—the elaborate treatise of Dr. Hamilton, and the *System of Surgery* of Prof. Gross—it is passed over without the slightest mention; and I trust that my success with it may encourage others to careful experiments in its use, so that, if proved worthy upon further trial, it may be added to the therapeutical resources of American surgery.

ART. IX.—*Case of Rupture of the Common Duct of the Liver. Formation of a Cyst containing Bile. Death occurring on the fifty-third day. Autopsy.* By T. M. DRYSDALE, M. D., of Philadelphia. (With two wood-cuts.)

ON the 19th of September, 1859, I was requested by Dr. G. Spackman to visit George Pepper, aged 13 years, who had been injured, several weeks before, by the tongue of a fire-engine striking him in the right hypochondriac region, and forcibly pressing him against a wall, close to which he was standing, and injuring him so that he had to be carried home.

Dr. Spackman saw him about eight hours after the accident. He was then pale, with an anxious expression of countenance, and extremely prostrated. He had vomited a considerable quantity of blood, and was complaining of intense pain in the abdomen, which had seized him immediately after the injury. Dr. S. prescribed morph. sulph. gr. $\frac{1}{4}$, to be repeated every two hours until relief was obtained. After taking three doses of the medicine he slept for several hours. He vomited blood occasionally for three days.

The second day after the accident, his bowels not having been moved, a dose of castor oil was given, which caused an evacuation without any unusual pain, and brought away a large quantity of dark blood. He continued to pass blood with every alvine discharge for several days; and when the feces ceased to be mixed with it, they were nearly white, and ever since have been of a light clay colour.

The first time his bladder was emptied after the injury he passed bloody water, accompanied with a burning pain in the urethra; and for a week following his urine was stained with blood, after which it was very dark, with a green tinge, and extremely muddy.

Dr. S. remarked that the patient was slightly jaundiced on the third day. This increased until he became of a dark yellow colour. He vomited more or less every day, and food produced great agony until it was rejected; it also caused his abdomen to swell very much. He had but little abdominal tenderness, and could bear firm pressure without complaint. Very little medicine was given; in addition to the oleum ricini, already mentioned, he twice took the citrate of magnesia, as his bowels were constipated.

For the jaundice, Dr. S. prescribed the following pills, one to be given every three hours: R.—Mass. hydrar. gr. viij; pulv. ipecac, pulv. opii, āā gr. j.—M. et div. in pil. viij. These were followed by a powder composed of hyd. chlor. mit. gr. j; pulv. opii gr. $\frac{1}{2}$; to be repeated every four hours until he had taken twelve.

The abdomen was painted, morning and evening, with the tincture of iodine; but all proved useless; he continued to emaciate rapidly, and the abdomen became greatly distended.

Present Condition.—He is unable to lie down, but sits constantly, propped in a chair. He is extremely restless, and his countenance is expressive of great distress. He cannot stoop without suffering. A deep inspiration occasions intense pain in the right side, below the ribs; “it feels as if something tears.” He is very much emaciated, and deeply jaundiced. His skin is cool and dry; pulse 130, feeble and frequent; respiration laborious and hurried. The urine is very dark; it has a green tinge, and deposits a pale yellow sediment. The feces are nearly white.

Examination of the Abdomen.—The distension is very great. The superficial veins are full and prominent. Percussion, above a line running obliquely from the lower border of the eighth rib of the left side to the umbilicus, and curving downward, extending three inches beyond this point, gives a clear tympanitic sound; below this, except along the course of the ascending colon, instead of resonance, there are dulness and fluctuation.

To complete the examination of the abdomen, the catheter was used, and about three ounces of dark urine removed.

Examination of the Chest.—On the left side the lung gives everywhere a loud resonant sound on percussion; the respiration is puerile. The right side of the chest is dull on percussion, anteriorly, as high as the nipple; posteriorly, the dulness extends an inch higher, and over this dull space no respiratory sounds can be detected; above this a feeble vesicular murmur is heard. There is no change in the location of the dulness on altering the position of the patient.

Treatment.—The bowels having remained unmoved for two days, a purgative injection was directed to be given; the application of the tincture of iodine to the abdomen to be continued, and a pill of calomel, half a grain, sulphate of morphia, one-eighth of a grain, to be taken every three hours. For the tympanitic distension ten drops of turpentine to be taken three times a day.

Sept. 20th. The turpentine produced strangury, which was relieved by warm fomentations and opium. There has been no vomiting since yesterday; otherwise the same. As the injection did not operate, we directed it to be repeated; to omit the turpentine and continue the pills.

The urine obtained yesterday was examined, and found to contain bile in abundance. The deposit consisted of the triple phosphate, and of urate of ammonia.

24th. There has been very little change in the symptoms. The vomiting has increased so that his stomach rejects everything but laudanum, of which he is taking teaspoonful doses every three hours. He has vomited some bright blood. The accumulation of fluid in the abdomen is steadily increasing. He suffers intensely.

26th. Condition unaltered. Dr. Washington L. Atlee visited him in consultation this morning, and proposed tapping the abdomen.

4 o'clock P. M. Dr. Atlee performed the operation of paracentesis abdominis. Two gallons of a dark green, viscid fluid, which stained the patient's linen yellow when it fell on it, were removed. As the fluid escaped, the breathing improved; the large veins showing through the skin of the abdomen disappeared; and the tympanitic portion of the enlargement gradually sank, this having evidently been formed by the stomach and the intestines being pushed forward by the fluid behind them. The pulse, which before

the tapping was 130, and scarcely perceptible, had fallen to 108, and become fuller. The patient expressed his satisfaction at the relief afforded.

27th. Pulse 100, soft and full; respiration easy. He did not sleep last night; has some tenderness on pressure over the umbilical region; is comparatively comfortable. To have nourishment frequently, and to continue the laudanum. His urine is much lighter in colour.

I examined the fluid removed yesterday by tapping. It was viscid. Its colour in mass was dark green or olive, but when in a thin stratum was yellow. Specific gravity 1018; alkaline reaction. Boiling, and also the addition of nitric acid, produced a coagulum. Sulphuric acid changed its colour to red, followed in a few minutes by green. Nitric acid produced a blue and green colour, which soon changed to gray. Pettenkofer's test produced the characteristic reaction.

Microscopic Examination.—A few shrunken epithelial cells and molecular matter were the only objects seen.

The fluid corresponded in most respects with the description of bile given by Budd, Carpenter, and Simon, but differed from it in containing albumen and being of a less specific gravity. This difference was probably owing to its admixture with the serous effusion from the peritoneum.

28th. He is weaker, and continues to emaciate; still vomits his food. His urine is again dark; sleeps very little; complains constantly of pain in his stomach, which even rouses him when asleep; the tenderness of the abdomen continues; no enlargement. To take eight grains of inspissated ox gall three times a day, and to have food in very small quantities, together with injections of beef-tea.

30th. The pills have been taken more freely than directed, but the bowels have not been moved. In the evening an injection was given, which brought away ten pills entirely unacted upon. His mind wanders.

Oct. 8th. He has continued to sink; the abdomen has been slowly increasing in size. The capillary circulation has been nearly arrested; his chest and arms being quite blue, and in some places having an ecchymosed appearance. He is frightfully emaciated. His stomach rejects the opium; is extremely restless; constant delirium. Directed morphia and beef-essence to be given by injection.

16th. Without any change in his symptoms, he continued sinking, and died at 12 M.

Autopsy.—Oct. 17th, 4 P. M. The examination was made, twenty-eight hours after death, in the presence of Drs. Atlee, Spackman, Burpee, Fish, Foster, and Dr. Meeteer, of Maryland. The body was extremely emaciated; the skin mottled, and of a purplish colour. Over the chest, abdomen, and various parts of the body, were large ecchymosed patches, which existed during life. The abdomen was enormously distended. On exposing the abdominal cavity, it appeared to be almost entirely occupied by a large cyst, which was in some parts of a greenish colour, in others of a light brown. It was feebly adherent, anteriorly and laterally, to the abdominal walls, as high as an inch above the level of the umbilicus. In handling the cyst, its walls proved to be softened and easily torn, permitting the escape of a large quantity of the same kind of fluid removed by tapping.

On tracing its relations, it was found to dip into the pelvis between the bladder and rectum, being attached to the peritoneal surface of both of these organs by weak adhesions. It filled the abdominal cavity as high as the umbilicus; and on the right side pushed the diaphragm before it, ascended until it reached the nipple, displaced the small intestines, the

cæcum, and the transverse colon, which was folded upon itself, and pressed them all to the left side of the vertebral column. The liver was pushed toward the left side, the right lobe downward, bending it upon itself.

The upward progress of the cyst in the median line was arrested by the root of the mesentery; winding around which, it rose up on the left side, beneath the intestines, as high as the diaphragm, thus forming a tumour of a horseshoe form.

The accompanying drawing represents the parts as they appeared when the abdomen was opened, and will render the above description more easily understood.

Fig. 1.

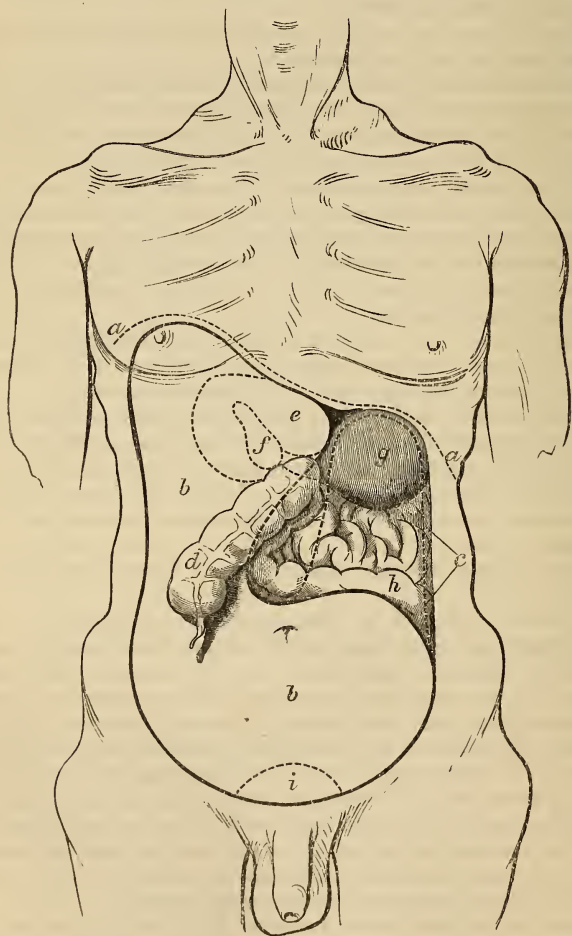


Fig. 1.—*a*. Diaphragm. *b*, *b*. Cyst. *c*. Cyst on the left side lying beneath the intestines and stomach, as indicated by the dotted lines. *d* Cæcum. *e*. The dotted lines mark the location of the liver beneath the cyst, and of the gall-bladder, *f*. *g*. Stomach. *h*. Transverse colon. *i*. Bladder, beneath the cyst.

The transverse mesocolon was spread over the upper part of the cyst, to which it adhered. On the right side, the tumour projected into the epigastric region, anteriorly to the liver, which it concealed, except a small portion of the left lobe, which was firmly adherent by its entire upper surface to the diaphragm. The stomach adhered to the liver, and the pyloric orifice was compressed between this organ and the cyst.

The peritoneum was discoloured wherever it touched the cystic walls, but it presented no signs of inflammatory action, except where the cyst adhered to the surrounding parts. The cyst and liver were so intimately adherent that they were removed together from the body for examination.

The adhesions between the cyst and the liver were broken down until the transverse fissure of the liver was reached, where the cyst became continuous, in part, with the peritoneal surface of this organ. The gall-bladder was found beneath the cyst, filled with bile, and of the normal size.

A careful dissection showed the tumour to be a continuation of the peritoneal covering, both of the liver and of the ruptured ductus communis choledochus; the rupture occurring three-quarters of an inch below the cystic duct. The ragged end of the duct projected a quarter of an inch into the cyst. (Fig. 2, *b*.) The tumour was lined over the greater part of its internal surface with a layer of lymph.

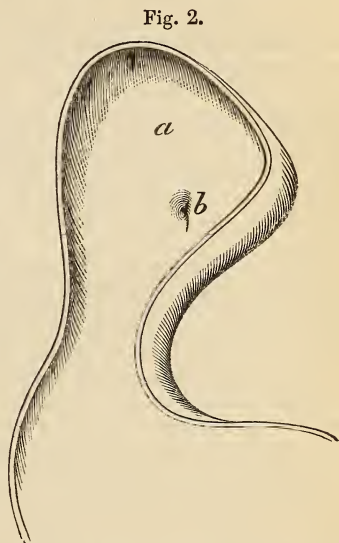


Fig. 2 represents a section of the upper part of the cyst on the right side. *a*. Interior of cyst. *b*. Remains of the common duct.

Remarks.—This enormous cyst was probably developed in the following manner: The blow from the tongue of the engine ruptured the ductus communis choledochus, thus permitting the bile to enter the gastro-hepatic omentum, which incloses this duct in its right border, causing inflammation of this portion of the peritoneum and sealing it to the neighbouring parts. The bile, which was subsequently secreted, was poured into the new receptacle, gradually enlarging it until it displaced the viscera and attained the size found after death.

A remarkable feature in this case is the extraordinarily rapid growth of a small portion of serous membrane; for we cannot regard it as a mere stretching of this membrane, as the cyst-walls were actually thicker than healthy peritoneum.

The case also demonstrates the power of the muscular coat of the gall-bladder to resist distension, the force of which must have been very great.

In searching authorities for a similar case, I have met with but one having any resemblance to it. This is found in Cooper's *Surgical Dictionary* (Amer. edition, p. 498), and is extracted from the report of Mr. Fryer, of

Stampford, published in the *Med.-Chir. Transactions*, vol. v. p. 330. In the symptoms during life, it so closely resembles the above case that I have transferred it to these pages, both for the sake of comparison and to encourage to persistence in treatment in any like accident, despite the apparently fatal character of the injury.

"A boy, about thirteen years old, received a violent blow from one of the shafts of a cart on the region of the liver. The injury was succeeded by pain, frequent vomiting of bilious matter, great sinking, coldness of the extremities, and a weak, small, fluttering pulse. The belly was fomented, and purging clysters thrown up. On the third day, symptoms of inflammation began, attended with considerable pain about the liver, great tension and soreness of the abdomen, and frequent vomiting. The pulse was quick, small, and weak; the skin hot and dry; the tongue much furred; the urine high coloured; and there was some difficulty of breathing, and great thirst. Eight ounces of blood were taken away, the fomentations continued, and a few grains of calomel were directed to be given every four hours, until the bowels were properly opened. Afterward, the effervescing mixture, with ten drops of laudanum, was exhibited every four hours.

"On the following day, the patient had some motions, and was much better; but, as his sickness continued, he was ordered a grain of opium every four hours. About a week afterward, he complained of a great increase of pain, which was somewhat relieved by a blister. He was now completely jaundiced, and his stools were white, but the tension, pain, and sickness were abated.

"Two days afterward, a fluctuation was perceived in the abdomen, which, in another week, became considerably distended with fluid. The patient now did not complain of much pain, but appeared to be sinking fast. A puncture was made in the swelling, and thirteen pints of what appeared to be pure bile were evacuated. The bowels then soon became regular, and the appetite good. In twelve days, the operation was repeated, and fifteen pints of the same bilious fluid were drawn off. Nine days afterward, another puncture was made, and thirteen pints more let out; and six were discharged in another fortnight. From this period the boy went on well, and perfectly recovered under the use of light tonic medicines."

This case is reported as rupture of the gall-bladder; but, as the patient recovered, the lesion from which he suffered is doubtful, and might, as far as the symptoms indicate, have been one of partial rupture of the common duct.

TRANSACTIONS OF SOCIETIES.

ART. X.—*Summary of the Transactions of the College of Physicians of Philadelphia.*

1861. Jan. 2. *Large Interstitial Cancerous Tumour of the Impregnated Uterus, rendering Delivery Impossible.*—Dr. WM. V. KEATING read the following account of a very remarkable case of this kind:—

I was requested to visit Mrs. J. in the month of July, 1857. She had been married on the 19th of May previous, and had perceived a slight menstrual discharge on the 22d of the same month, since which time she had no signs of menstruation. Being requested to inform her at what period she might expect her confinement, I calculated that her labour might supervene at any moment after the 24th of February.

Mrs. J. was a native of South America, and when I saw her was in her 30th year. Her complexion was of an olive tint; temperament nervobilious. She had previously enjoyed good health, having in fact suffered from none of those ailments incident to her sex at the period of puberty. In the year 1853, without any appreciable cause, in the interval of a menstrual period, she was suddenly seized with uterine hemorrhage, which, though short in duration, was quite active in character. Her attendant, the late Dr. Jos. Nancrede, was called in, and by means of rest and the usual remedies was soon able to arrest it. She never had a return. Mrs. J. subsequently suffered from impaired vision, and had been under Dr. Sichel's care in Paris; and although his directions were strictly carried out, she could seldom indulge in reading, writing, or sewing.

The peculiarity of Mrs. J.'s complexion had forcibly impressed me upon my first visit, and elicited from me many inquiries as to her previous health and the history of her family. I could not but feel some apprehensions in reference to the existence of some cachexia, and my solicitude was only quieted by the assurances of her friends, to whom I communicated my fears, that the peculiar tint resembling cirrhosis, which attracted my attention, was natural to her. Upon questioning her maid, after her death, I discovered that her menstrual discharge had generally been scanty and emitted a very fetid odour, such as she had never noticed before, although in other respects it had the usual characteristics. Mrs. J. enjoyed remarkable health during the period of gestation, and was unusually active, walking often six miles a day, for inasmuch as her weakness of vision prevented her from attending to the usual occupations at home, she was forced to resort to out-door amusement. My sole attendance during her pregnancy was for a violent pain in the anterior portion of the left thigh, an inch above the patella. The pain was very severe, and seemed to cause her great annoyance; it was paroxysmal in its character, and was finally relieved by repeated vesications and endermic applications of the sulphate of morphia.

On Sunday, the 28th of February, at 9 A. M., I was summoned to visit Mrs. J. She had been seized with acute pains in the uterine region, be-

tween 1 and 2 o'clock A. M. About 4 A. M., during one of her pains, she felt something give way, and immediately there followed a gush of a greenish fluid. These pains continued accompanied by a discharge of some bloody mucus until I reached the house, not increasing in intensity or rapidity, but always attended with a dribbling of the greenish-coloured fluid. I immediately took advantage of the access of a pain to make a per vaginam examination. Upon reaching the os uteri, I found it, to my utter astonishment, entirely closed with a cervix uteri as undeveloped as in the sixth month of pregnancy. The os felt hard and not well defined, the cervix almost cartilaginous to the touch, and totally unlike anything which I had ever observed in the organ at this period of pregnancy; repeated and accurate examinations soon revealed the fact to me that the pains had no effect whatever upon the os uteri, whose dilatations had not as yet commenced. I made the usual examination of the straits of the pelvis, and determined that that organ was in every way normal, and hence feared no difficulty in that quarter. My patient was of course in an exceedingly nervous condition, and most anxious to ascertain my diagnosis and prognosis. The pains having had no effect upon the os uteri, and seeming to diminish in intensity and frequency, I deemed it most prudent to state that she was not as yet in labour, but that probably true labour-pains would soon supervene.

The advent of the estimated period of labour, the supervention and continuation of regular uterine contractions with the rupture of the membranes, viewed in connection with the existence of an undeveloped cervix and a perfectly closed os uteri, aroused serious apprehensions in my mind as to the probable existence of some obstruction in the labour or malposition of the foetus. I requested a careful external examination of the abdomen. I found the abdominal parietes presenting all the tension of the full period of gestation; the uterus had also subsided, and inspection revealed an abdominal tumour of singular shape, the longest diameter of which corresponded to a line drawn from the right hypochondriac region to the anterior-superior spinous process of the left ileum. Careful palpation and even inspection revealed a sulcus or line of separation in the tumour, commencing on the left side about four inches below the last false rib, and extending obliquely down to within an inch of the symphysis pubis, and dividing the uterus into two portions. That portion of the tumour towards the right side was quite prominent; towards the left iliac region a less prominent orbicular mass could be felt, resembling the foetal head. During the contractions of the fundus and body of the uterus, which I could distinctly feel with my hand laid on the abdomen, the right and more prominent portion of the tumour seemed to undergo forcible contractions, but the orbicular mass to the left seemed in no ways affected, and always maintained its primitive position and condition.

Auscultation at this period revealed very feeble pulsations of the foetal heart in the hypogastric region, a little to the right side.

I had serious misgivings as to the nature of the case. The above examination convinced me that there existed probably a twin pregnancy with a malpresentation, complicated with premature rupture of the membranes and a rigid os uteri. As the bowels had not been moved for forty-eight hours, I prescribed a dose of oil, and left. Upon my return at 2 P. M. I found that the pains had entirely ceased, but the discharge of liquor amnii still continued whenever she moved. I made another examination, and found everything in the same condition. The os uteri being closed, the pains

having subsided, there was nothing to do but to revive the drooping spirits of my anxious patient, whose extreme confidence in the correctness of the calculation of her period of labour, caused her to suspect that there was some serious cause retarding its progress. The existence of a cervix uteri, the entire closure of the os uteri, the subsidence of all pains, were sufficient considerations to lead me to doubt the accuracy of the computation of her pregnancy; hence I deemed myself justified in asserting that she could not have reached the full period of gestation, and having thus succeeded in calming her fears, I left her.

I visited Mrs. J. on Monday the 1st of March; found her sitting up, and quite refreshed in body and mind, having passed an excellent night, without any return of uterine contractions. The liquor amnii was still oozing from her, and she was annoyed with a constant desire to micturate; she was slightly feverish, but not more so than might be expected in one of such a nervous temperament, annoyed by the sudden subsidence of her pains, and most anxious for labour to supervene. She continued very much in the same condition until the 14th of March. Auscultation, however, never revealed any sound of the foetal heart after the 28th of February. The dribbling of the liquor amnii continued till the 4th of March; the colour of the fluid then changed to a dark mahogany, with a fetid odour. Occasionally slight paroxysms of fever occurred, but generally as the result of her extreme restlessness and anxiety. During this period there would also occur now and then slight paroxysms of pain, but very feeble and of very short duration, and upon palpation during these pains I could invariably discern that the contractions were confined to the right and more prominent portion of the tumour. Repeated vaginal examinations afforded always the same results, the same conditions of the os and cervix uteri. About the 8th of March the uterine tumour assumed such a position as to cause great inconvenience and at times much suffering. Locomotion was almost impossible, and when the patient was seated the uterine tumour seemed to project over the symphysis pubis, and, as the nurse stated, seemed to rest on the upper portion of her thighs; the legs and feet became quite œdematous. The desire to micturate was incessant, but the urine was scanty, high-coloured, and slightly albuminous. On the 10th of March slight labour-pains occurred, with no effect upon the os or cervix uteri, and after a few hours subsided; the discharge became much more fetid, paroxysms of fever supervened, and so much restlessness and despondency ensued as to force me to resort to anodynes to calm her and to produce sleep. Finding that no change took place, and feeling serious apprehensions in reference to her state of mind, I requested a consultation on the 14th of March. Prof. Meigs visited my patient at 12 o'clock that day, and after a careful examination both internally and externally, fully coincided in my opinion, viz., that the condition of the os and cervix uteri and the absence of all true uterine contractions producing dilatations of the os, with the impossibility to feel any motion of the fœtus, authorized the conclusion that our patient could not have reached the full period of gestation. The extraordinary form of the uterine tumour induced Prof. Meigs also to suspect that there might either be a transverse presentation of the fœtus, uterine pregnancy, a bicorned uterus, or a Ritachristina. As the result of our consultation, we endeavoured to calm the fears of the patient and her friends, and resolved to wait and abide the indications of nature.

Mrs. J. continued in the same condition until the 16th (Wednesday), save a change in the colour and consistence of the discharge, which con-

sisted now of a pink-coloured grumous matter with a peculiarly fetid odour. Upon palpation of the abdomen, I could often discover contractions of the right portion of the abdominal tumour, without, however, any responsive action in the cervix or os, and without occasioning much pain; her state of mind was truly alarming—there were frequent febrile exacerbations, attended with a highly furred tongue, harsh dry skin, and copious discharges of mahogany-coloured urine, which yielded a considerable amount of albumen to the proper tests. She was kept on saline draughts, and under the influence of Dover's powder.

On Wednesday morning I found her suffering from well-defined uterine pains, recurring about every fifteen minutes; upon palpation I discovered that coincident with these pains there were powerful contractions of the right portion of the tumour, but the orbicular mass situated in the left iliac region seemed completely inert and in no way affected by the uterine contractions. These labour-pains had supervened about two o'clock in the morning, and having increased regularly in intensity and frequency ever since, I was led to hope that a per vaginam examination would reveal a decided progress in her labour. At 10 A. M. I made a per vaginam examination. I found the canal very hot and dry, the os uteri seemed distant, and it was difficult to reach it; it was slightly opened, about one-eighth of an inch; cervix uteri still present, but the anterior lip of the os and left portion of the cervix uteri seemed exceedingly indurated, almost cartilaginous. No portion of the fœtus could be felt, and consequently it was absolutely impossible to diagnosticate presentation or position. Bowels being confined, and there being considerable febrile excitement, I prescribed a dose of castor oil and effervescing draught. Returned at 1 P. M.: pains continue increasing in intensity; os uteri opened a little more, and in pushing my index-finger well up into what seemed to be the cervix uteri, I thought I discovered an intestinal-like protrusion of the membranes; repeated examinations confirmed my opinion; I requested Prof. Meigs to visit her with me at 2 P. M. He coincided in my diagnosis, and suspecting the existence of a transverse presentation, recommended me to keep a strict watch so that I might seize the proper moment, rupture the membranes, and attempt version by the head. I remained near the patient all the afternoon, hoping to be able to carry out our design; the pains continued, not very intense, but of a most harassing character; no more effect was produced upon the os uteri or the presenting portion of the child. At 8 P. M. we held another consultation. Ordered anodyne enema and saline draught.

Thursday, 17th, 9½ A. M. Met Prof. Meigs. Our patient had a calmer night, and slept about fifteen minutes at a time. The pains continued during the night, but were now much more irregular and less intense; febrile action continues; discharge from vagina diminished; tongue much furred; breath extremely fetid. She was very fretful and irritable; form of tumour remains the same; no change in condition of os uteri; urine still very dark and albuminous. Ordered small doses of hydrg. cum creta, to be followed by oil.

I returned at 2 P. M. She had two bilious evacuations and seemed much relieved, and was much more cheerful; skin moist; pains almost disappeared; no change in uterus; slight discharge of a grumous character and very offensive. Relished her dinner, and seemed inclined to sleep. Consultation held again at 6 P. M. She seemed more cheerful; had a refreshing sleep of four hours; pains slight and not frequent; the contractions still continued in right portion of the tumour. Anodyne enema repeated.

Friday 18th. Held a consultation at 9½. Passed a restless night; pains returned at regular intervals, and, although not very intense, were of a harassing character, causing her to be fretful and despondent; no change in internal parts; tongue furred; febrile action continues; breath very fetid. Ordered oil. Met again at 1 P. M. Oil operated once; pains of a slow lingering character; patient very despondent and entreats for relief; the presenting point of foetus has made no advance, and os uteri remains in the same condition. We decided upon giving 10 grs. of ergot every hour, and I remained with the patient to watch the effect. After the fourth dose the pains increased in frequency and intensity, and were of a most distressing character; the right portion of the uterine tumour contracted so actively that I at one time entertained serious fears that rupture of the uterus might occur, but the left orbicular mass still seemed perfectly inert, maintaining all its former characters with the sulcus remaining perfectly defined. We met again at 6 P. M. Pains still continued very violent. After a long and careful examination, Prof. Meigs thought that he felt a small portion of the foetal head compressed within the cervix uteri, and concluded that what we had repeatedly considered an intestinal-like protrusion of the bag of waters was really a portion of the softened putrescent scalp. In an hour after this view was confirmed by the possibility of detaching portions of the scalp with the finger, we decided to allow the pains to push the presenting portion further down, and appointed to meet at 10 P. M.; but finding, at 9, that her pains were almost insupportable, and her entreaties for relief so urgent, I went to Prof. Meigs and requested him to bring his craniotomy forceps and blunt hook. We decided that although the os uteri was not dilated more than an inch and the cervix undeveloped, still, on account of the agony which she was enduring, we would be justified in making careful and gentle traction upon the presenting portion of the head, and thus force it to engage within the circle of the os. This plan of action was based upon the fact that her violent pains seemed to produce no effect, and believing that we had either a monster with two heads or a transverse presentation which nature could not disengage, we deemed it prudent and humane to assist by gentle traction efforts in forcing the presenting portion to engage. The difficulties we had now to encounter beggar all description. The extreme distance of the presenting surface, the exquisite sensitiveness of the canal, the tenesmic resistance of the os uteri, the difficulty in seizing and keeping hold of the small portion of bone which could be reached, seemingly imbedded in the tissues of the cervix uteri, presented such insurmountable obstacles that, after repeated efforts, attended solely with the crumbling and detachment of minute portions of bone, we were obliged to desist about 11½ P. M., especially as the patient had been for so long a period of time under anaesthesia. The condition of the os uteri under this operation was one which caused us the greatest annoyance and apprehension. It was utterly impossible to pass the finger around the anterior and left portion of the os, the presenting bones of the foetal head seemed imbedded in the substance of the uterus itself, and every traction effort was attended with the sinking of a rough saw-like edge of bone deeper and deeper into the tissues of the cervix and lower part of the body of the uterus; posteriorly and to the right the finger could be introduced as far as its length would allow, but to the left the cartilaginous condition of the os and cervix uteri and the steady resistance of the orbicular mass seemed to defy all efforts at delivery. The patient was exhausted, the vagina had become intensely swollen; we were, therefore, forced to desist. An opiate

draught was administered, and Prof. Hodge sent for. We all three met at midnight. Prof. Hodge examined and coincided with us as to the condition of things. We determined to allow the patient a certain period of repose, and then, under the influence of anæsthesia, to make further attempts at delivery. A powerful opiate was then administered, but the constant uterine contractions, superadded to her extreme anxiety, antagonized its effect. Nourishment and stimulus were administered, and at 2 A. M. Prof. Hodge attempted to extract the presenting portion of the fœtus; but after the most powerful efforts, finding that no progress was made, we deemed it more prudent to keep the patient under opiates, and to soothe the vagina and os uteri with demulcent and antiseptic injections.

Adjourned our consultation to Saturday, 19th, at 9 A. M.

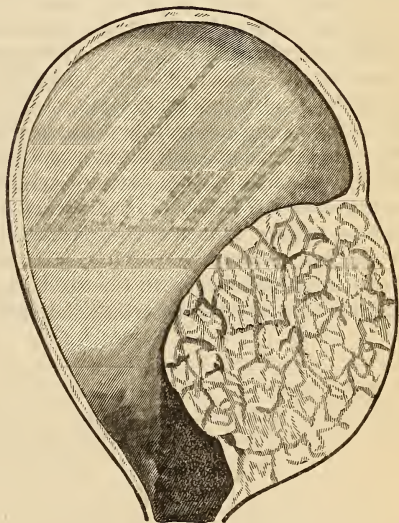
Mrs. J. did not sleep; uterine pains incessant; tongue very much furred; pulse 140; abdomen tympanitic; urine scanty and albuminous, and has to be drawn off by catheter; vagina in a high state of inflammation, and the discharges so fetid as to affect every one in the room. Prof. Hodge suggested that we should attempt dilating the os uteri by the application of Kiwisch's uterine douche; this was tried eight hours without any success, the pains became much more violent under its employment. Colpeurysis was also resorted to. Our patient was now kept under the effects of powerful opiates and a supporting diet. Her condition grew worse and worse every hour, and her agony was such as to fill with anguish all those who surrounded her. New instruments were devised, constant attempts made to effect delivery, but all that science could suggest and art devise seemed totally inefficient.

On Saturday, at 2 P. M., Dr. La Roche was added to the consultation, and the subject of the Cæsarean section was proposed, considered maturely, and rejected on the following grounds. The greatest authorities who have written upon the Cæsarean operation have considered it to be necessary: 1st. In the case of extra-uterine gestation. 2dly. Where the fœtus has escaped entirely into the cavity of the abdomen on account of rupture of the uterus. 3dly. In the case of hysterocele or hernia of the uterus. 4thly. Where tumours, bridles, cicatrices, adhesions, or other affections of the soft parts included in the pelvis obstruct this bony canal, the neck of the womb, or vagina. Lastly. Where the pelvis is in itself so far defective that there no longer exists between its dimensions and those of the head of the fœtus the relation necessary to delivery. After the most careful examination and consultation, not being able to discover the existence in the present case of any of the foregoing conditions, we deemed that the ethics and the acknowledged rules of practice forbade us attempting the operation. Moreover, it was unanimously agreed that, under Mrs. J.'s present condition of intense constitutional excitement, the operation would be necessarily fatal. We were, therefore, constrained to adopt an expectant treatment and to alleviate her pangs as much as possible, hoping from hour to hour that nature might effect some change in her behalf. Finally symptoms of pyæmic intoxication set in and soon banished from us all hope of saving our patient. The uterine pains continued incessantly up to the period of her death, which occurred on Friday the 25th, at 2 P. M. To narrate her symptoms, to describe her agony, or to express the feelings of those whose sad duty called them to her bedside without the slightest power on their part to relieve or deliver her, would be recalling scenes and pangs which, we trust, our professional career will never cause us again to witness and endure. Suffice it to say that our poor patient died in the violent throes of labour, having

suffered uterine pains for ten days and nights with scarce an intermission save the natural intervals between the pains. Those among us who are familiar with the characters of uterine contractions can well imagine the horrors of these ten days of agony to this poor lady, and can well appreciate the gloom and despair of her medical attendants in beholding them.

Post-mortem examination was held nineteen hours after death. Performed by Dr. Ellerslie Wallace, in presence of Drs. Meigs, Hodge, La Roche, Sr., La Roche, Jr., and Keating. Upon uncovering the abdomen the sulcus in the external portion of the abdominal parietes still quite apparent. On removing parietes of abdomen the uterus presented the sulcus, but higher up than it was externally. There were no signs of parietal or intestinal peritonitis. Uterus $10\frac{1}{2}$ inches long and lying obliquely; above the sulcus measures $7\frac{1}{2}$ inches; at the sulcus 7 inches; two inches below sulcus uterus measured 7 inches. The fœtus had never entered the superior strait, and was in a complete state of decomposition. On removing the uterus it was found that the os uteri was almost cartilaginous in consistency and barely large enough to admit the introduction of two fingers. Upper portions of vagina and lower half of cervix uteri in a state of sphacelus. The tissue of the uterus near the cervix was extremely congested. On opening the uterus the lower globe of uterus was found to be an immense interstitial tumour. (See fig. from a drawing by Professor Meigs.) Transverse diameter of the tumour included the whole os uteri; antero-posterior diameter of tumour $5\frac{1}{2}$ inches; vertical diameter $6\frac{1}{2}$ inches. General aspect of tumour ovoidal. Placenta attached to the posterior aspect of the fundus uteri. General surface of the uterus in a complete state of putrescence.

Fig. 1.



Microscopical examination of the tumour by Dr. Da Costa.—I annex the results as embodied in a note to me: "The specimen you sent me for examination was peculiar in several respects, and especially from the fact that the morbid material did not exist by itself, but had become intimately blended with the proper structure of the uterus. Under the microscope I found infiltrated between the smooth muscular fibres of the womb, cells with distinct nuclei, some small, some large, and obscured by granules, but entirely resembling those of cancerous masses. In addition, there were some oil drops and a large number of granules. The muscular fibres were here and there altered in appearance, infiltrated with granules, yet on the whole I was struck with the fact that, whilst so much diseased action had been going on around them, so many of them, by far the largest majority, had remained perfectly unaltered in shape, size, and contents. I have no doubt that the tumour is cancerous; softening had commenced."

The sad result of this case, and the light shed upon it by the post-mortem examination, will undoubtedly suggest to many the propriety, under such circumstances, of resorting to the Cæsarean operation. In answer I would merely recall to your memory the anecdote of Columbus and the egg.

It has undoubtedly created some astonishment in the minds of many here present that this unfortunate lady should have been allowed to die undelivered, especially as the statistics in reference to the Cæsarean section had in latter days been so favourable. It would be an injustice to myself and respected colleagues did I not recall the fact that with the data before us it was absolutely impossible to anticipate or diagnosticate the nature of the obstruction which we had to overcome. I believe that it must be conceded that the most skilful and experienced acumen in diagnosis might be expected to fail on occasions like the present. To justify a resort to the Cæsarean operation there must be present some of the conditions which I have enumerated in another portion of this paper, failing to discover them, and being convinced that the nature of the obstruction was such as to induce us to hope that it might yield from hour to hour, in the doubt and obscurity in which we were groping, we felt bound to act with that conservatism which eschews all hap-hazard treatment, having neither reason nor experience for its basis. "*Melius anceps remedium quam nullum*" may be a justifiable apothegm in the judgment of the charlatan, the ignorant, and reckless, but can never prove a safe guide for the conscientious physician. When it became apparent that a delivery, *per vias naturales*, was impossible, the case had already assumed such an aspect from pyæmic intoxication as to render the Cæsarean section not only useless, but, under such circumstances, cruel. It is a sad satisfaction to us, in reflecting over the results of the post-mortem examination, to find that even in this unprecedented case a strict adherence to the rules and ethics of our art has proved the justice and wisdom upon which they are founded.

ART. XI.—*Summary of the Proceedings of the Pathological Society of Philadelphia.*

1860. Nov. 14. *Completely Adherent Pericardium.*—Dr. CHARLES C. LEE presented this specimen, which was taken from the body of a patient who died in the Blockley Hospital.

The subject, a coloured girl, æt. 20, had been for two years an inmate of the hospital, suffering from hypertrophy and aortic valvular disease, with occasional attacks of pericarditis, but her general health was sufficiently good to enable her to go about the house. One evening she was suddenly seized with dyspnœa, and violent pain and palpitation in the cardiac region, in which condition she expired in a few moments, before any physician could reach her.

The autopsy was made twelve hours after death, by Dr. Weidman, who informs me that when he opened the thorax nothing was visible but an immensely enlarged heart, which appeared to have gradually compressed and displaced the lungs. The heart was more than double its usual size; and so completely obliterated was the pericardial sac, that at no point could the membrane be separated from the cardiac walls. There was marked insufficiency and thickening of the aortic valves, but no atheromatous or other deposits; the mitral valve, with its fleshy columns, was also much thickened. All the cavities of the heart were greatly dilated, and its walls hypertrophied. The lungs were smaller than usual, rather congested, and felt almost solid to the touch. The other viscera were healthy.

Dr. PACKARD referred to a case of a similar kind, reported by him to the Society about three years ago. (See Proceedings, vol. i. p. 52.) In that case, as well as in the specimen now presented by Dr. Lee, there had been insufficiency of the auriculo-ventricular valves, and adhesion of portions of them to the ventricular walls. It would be interesting to ascertain in what proportion of cases of adherent pericardium this fastening down of one or more flaps of the auriculo-ventricular valves is present.

Internal Hemorrhage in a Newly-born Infant.—Dr. KELLER exhibited the respiratory organs, and said: The mother of the subject from whom these specimens were taken was a strong, masculine woman, æt. 38 years, who, after eight years of married life, was for the first time taken in labour on Friday evening, the 26th of October last. Her pains continued without remission until 4 P. M. of the following Sunday, when she seemed so much exhausted that I deemed it advisable to deliver her with the forceps. I was obliged to detach the placenta, a portion of which was slightly adherent, and this operation, though performed with great care, was followed by considerable hemorrhage, which was with difficulty arrested. The child showed but faint signs of life; and was only resuscitated after strenuous exertions on the part of the attendants, when it threw up a quantity of greenish mucus mixed with blood. For a short time after this its respiration continued regular, but soon became laboured. All the other functions were normally performed; but on the following morning I noticed that the abdominal veins were enlarged, and the abdomen itself much distended with flatus. The child died forty hours after its birth.

Autopsy, twenty-four hours after death.—The mucous membrane of the trachea was found deeply tinged, and covered with thin liquid blood. The lungs floated in water, but were highly congested, especially the left at its upper lobe. The heart contained a small quantity of fluid blood. The stomach contained a quantity of dark coagulated blood, mixed with mucus; the intestines and other viscera were healthy.

Cerebral Hemorrhage.—Dr. ROBERT P. HARRIS reported the following case of apoplexy, occurring in an anæmic patient: On Tuesday morning, October 20, 1860, I was hastily summoned to see Mrs. S., aged 34; the mother of four children ranging from five years and a half to six months old. When I reached her bedside she informed me that she had been suffering with a severe pain in the right side of her head since the previous Friday, accompanied by noises in the ears and an occasional numbness in the right arm and leg; that she had taken purgative medicine, under a belief that her symptoms proceeded from indigestion, but without relief; and that just before I was sent for (at 9 A. M.) she had fainted and fallen upon the floor in a state of unconsciousness, soon after coming up-stairs to her chamber, but had in a little while recovered. I found her very pale and weak, with a slow, regular, but feeble pulse, and suffering from a violent pain in the right side of the head. Her consciousness was perfect, eyes natural in appearance, and muscular movements unaffected. Her chief complaint was of headache and extreme debility. Previous to the attack in question she had suffered from weakness for a long time, consequent upon too frequent childbearing and the necessity of constant nursing, together with attacks of epistaxis, to which she had been subject from early childhood, preceded in latter years by hyperæmia capitis. In less than six years she had given birth to four living children, one at a time, and had twice miscarried. Previous and subsequent to her last accouchement she suffered very much with a catarrhal affection, and had frequent attacks of faintness and supra-orbital neuralgia. She was in person spare, of medium height, had a long neck and narrow head, and, when in moderate health, had a bright red blush upon the upper part of her cheeks. She was subject to frequent and obstinate coughs, and her appearance might have led one to fear tuberculosis.

The last time I saw her, previous to this attack, was in the last week in September; and upon that occasion I warned her of the danger to her health to be expected from continuing to nurse her infant, and ordered her to wean it. From that time up to the attack of headache on Friday she had been apparently improving in health, and a cough which she had had for several months had left her. She had not, however, derived all the benefit that she might have had from weaning her infant, for, although it was hand-fed during the day, she allowed it to nurse at night. The day before that upon which she fainted (Monday) she had a severe pain in her back, seated in the lumbar region, and discharged what, judging from description and other signs to be hereafter referred to, must have been a blighted ovum of the first month. Its exit was unaccompanied by any hemorrhage; and her mother, who informed me of the circumstance, did not know whether the body, which she described as oval and of a fleshy appearance, came from the vagina or rectum.

About 10 o'clock, soon after I had left the patient's house, she was seized with convulsions, and the messenger failing to find me, Dr. Hoyt was called in, from whom I have received the following statement of her symptoms

during his visit. She had several spasmodic attacks, lasting some minutes each time, the motion being confined to the right arm and leg. The movement of the arm was not jerking, as we generally see it in the convulsions of children, but was evidently, to a certain extent, voluntary, as the hand was applied to the right side of the head, over the seat of pain, and passed backward and forward over the side of the head, face and neck. The conjunctivæ were slightly injected, and the pupil of the right eye was largely dilated, that of the left being much less affected. After the cessation of spasmodic movements, and during the interval between the attacks, the difference between the pupils was much less marked; both, however, being still slightly dilated. The absence of motion on the left side of the body during the spasms was evidently not due to paralysis; as voluntary movements of the left arm were made during the intervals. The muscles of the eyes and face were unaffected, as well as also those of the neck. During the spasms the patient breathed heavily, with strong stertorous respiration; but in the intervals there was only a very slight snoring sound produced, and she appeared to be in a profound sleep.

At 1½ o'clock P. M. I again saw the patient. She was then apparently asleep; pulse 60, regular and feeble; respiration slightly snoring; right pupil a little more dilated than the left, but the enlargement of neither was much marked. When roused, and asked if she felt pain in her head, she assented with a nod, and was soon afterwards seized with vomiting, the contents of her stomach being ejected with considerable force; after which she relapsed into unconsciousness, and remained in this state until about 4 P. M., when she was seized with a spasmodic contraction of the arms and legs, and in a few minutes expired, just seven hours after the fainting fits took place.

Autopsy, made twenty hours after death.—Body well formed, but quite spare; surface remarkably pale; rigor mortis well pronounced; extremities contracted in a peculiar manner. The forearms were pronated, and slightly flexed on the arms, the hands flexed upon the forearms, the fingers curved and adducted, and the thumbs flexed and turned into the palms, as we see them in cases of laryngismus stridulus. The feet were drawn inwards by a contraction of the tibialis anticus muscles, so that the soles were presented towards each other.

Upon removing the skull-cap, the surface of the left hemisphere of the cerebrum was seen to be intensely congested, particularly at a point immediately above the fissure of Sylvius. The dura mater being opened, a little serum escaped from the left hemisphere. A horizontal section made above the corpus callosum revealed the presence of a large, irregularly-shaped cavity in the centre of the right hemisphere of the cerebrum, filled with a soft black clot of blood. This cavity, which had no communication either with the fissures of the cerebrum or the ventricles, had a capacity of about three fluidounces. The cerebral substance around the clot was very much softened, and easily broke down under the handle of a scalpel; its surface was studded with numerous points of blood. Upon dissecting up the ramifications of the cavity, it was found to have been formed from below upwards and backwards, commencing just above the fissure of Sylvius, in the middle lobe of the cerebrum, and extending up into the centre of the hemisphere, forming at the upper part of the cavity a rounded excavation, which we first cut into in making the section referred to. Upon tracing out the bloody track at the bottom of the cavity, the effused blood was found to have come from a rupture of the middle cerebral artery.

The left hemisphere of the cerebrum was perfectly healthy. The lateral ventricles were not congested, and contained but little serum. The space around the infundibulum was filled with a clot of blood. The cerebellum was in a normal condition. The posterior part of the medulla spinalis was congested. The basilar artery contained no clots of blood. All the portions of the brain not mentioned were in a healthy state.

Chest.—The left lung was slightly adherent to the walls of the chest, the right entirely free; both were somewhat congested, but contained no traces of tubercle or evidences of serious affection. The heart was of normal dimensions, somewhat loaded externally with fat, and softened in its tissues, but the microscope gave no evidences of fatty degeneration. The mitral valve was covered with small red vegetations upon its free margin; the other valves were healthy. The pulmonary artery was thinner and softer in texture than natural; the aorta was healthy.

Abdomen.—The liver was found to be highly congested, so as to be coloured nearly black, and so much softened as to be readily torn by the fingers. The spleen was also congested, marbled externally, but very little larger than natural, and the Malpighian corpuscles were found to be large and well defined. The left kidney was congested, and presented a cicatrix upon its free edge, probably the remains of an old cyst; the right was also congested, and presented three similar cicatrices. The left capsula renalis was healthy, the right was distended with fluid blood.

Pelvis.—The uterus was retroflexed, which may have been post-mortem, as there were no symptoms to indicate such a condition during life, and the neck was very soft and flexible, the broad ligaments relaxed, and the body of the uterus heavy from congestion. Upon opening the uterus by a vertical incision, its walls were found thickened, its cavity enlarged, the lining membrane dark-coloured from congestion affecting the entire organ, and within the body a small whitish object bearing some resemblance to an embryo of two or three weeks old. The appearance of the uterus, taken in connection with the fact that an oval fleshy body was discharged, makes it probable that she may have miscarried the day previous to her death. Whether the retroflexion of the womb was the cause of the abortion, was produced by the fall, or resulted post-mortem from the causes before mentioned, I am not prepared to decide. The extreme flexibility of the cervix would indicate that the retroflexion was of recent date. The ovaries were firm, white, and filled with vesicles. The mesenteric glands were slightly enlarged. The stomach and intestines were not examined, except as to their external appearance, which was natural.

The points most worthy of note in this case are: the comparative youth of the patient; the absence of the apoplectic diathesis; the previous debility, and its causes; the tendency of plethora capitis, notwithstanding the existence of anæmia; and the non-appearance of hemiplegia or paralysis in any form, although the amount of blood effused was large, and the variety of apoplexy cerebral.

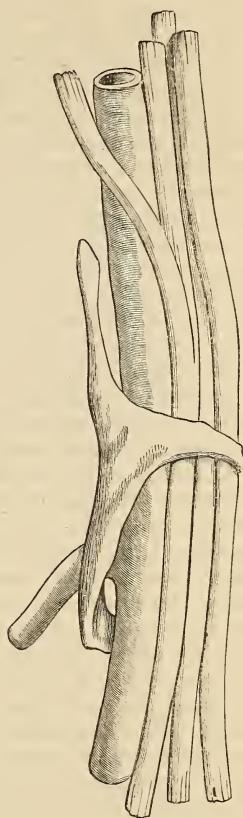
Ring of Bone in the Axilla.—Dr. HARLAN exhibited a specimen of this, and stated that it was obtained from a subject in the anatomical rooms of the University. It is a distinct, firm bony ring, including the axillary artery, with the median, ulnar, and musculo-spiral nerves. The accompanying drawing, by Dr. Packard, taken after the specimen was removed from the body, gives the exact size and shape of the ring, Fig. 1, and shows the parts included within it, Fig. 2. The history of the case is not known. The

subject was a negro, apparently of middle age. There was no adventitious deposit in any other part of the body. The ring was found in the middle of

Fig. 1.



Fig. 2.



the right axilla. It had no connection with the humerus or with the sheath of the artery, but is connected by its posterior surface with the sheath of the musculo-spiral nerve. The limb was well developed, and showed no signs of any interference with its function.

Curious bony deposits are known to occur occasionally in healthy tissues as well as in morbid products, and pieces of true bone of considerable size have been found imbedded in muscle (in the museum at Vienna there is a large egg-shaped piece of bone, which was taken out of the biceps of a woman's left arm), but I have not been able to find any record of a case at all like this. Its shape and position are very peculiar; and it is singular, too, that the pressure to which every motion of the arm must have subjected the nerves has had no apparent effect upon the limb.

Two Cases of Yellow Fever; one sporadic, the other imported.—Dr. LA ROCHE made the following remarks relative to these cases:—

My friend, Dr. Packard, is about to present to the Society the results of

the post-mortem examination of two cases of yellow fever, which my official connection with the Board of Health afforded me the opportunity to notice—the first after death, the second during life. These examinations were made at my request, by Dr. Packard—the first with the assistance of our secretary, Dr. Kane. The history of both cases presents features of an interesting character; while, as you will perceive, the appearances revealed on dissection were sufficiently important to justify their being brought to the attention of the Society, and, indeed, of the profession at large.

The first case occurred in the practice of Dr. A. C. Bournonville, of this city, by whom it was reported to the Board of Health. It was a truly sporadic case (the disease not showing itself in a single other instance during the entire season), and, without a possibility of doubt, it must be regarded as of strictly local origin. The subject of it, a German, resided in Front Street below Coates, and worked in a board-yard situated at the foot of Green Street, on the Delaware front of the city. Some time previous to his attack he was employed on a raft conveying timber from the upper part of the river. He had not communicated, directly or indirectly, with any infected vessel from yellow fever ports; indeed, there was no such vessel to communicate with. He had not been within many hundred miles of a yellow fever case, from which he could, supposing the thing possible, have taken the disease; and had not been in any way exposed to the supposed morbid influence of fomites, whether in the form of merchandise or clothing. It may not be uninteresting to remark, in this connection, that at no great distance from the residence and place of work of the individual in question two sporadic cases (sporadic so far as regards that particular part of the city) occurred in 1853, and three or four a few years after.

The patient was visited by several of the medical members of the Board of Health, who are perfectly conversant with the yellow fever, and from their report of the cases; from the account of the symptoms, day by day, for which I am indebted to the attending physician, Dr. Bournonville; as also from the character of the matters ejected from the stomach and passed off by the bowels, no doubt can be entertained relative to the yellow fever nature of the disease. Indeed, could such a doubt have crossed the mind of any one during the progress of the case, it must necessarily have been dissipated by a view of the body after death, and especially by the inspection of the organs, the implication of which is admitted to characterize the said disease.

The patient was attacked early on the morning of Saturday, September 1, with a chill, followed by high fever, attended with severe pain in the back, arms, legs, and head. These symptoms, together with delirium at night, thirst, grayish diarrhoea, and epigastric pain, continued, with greater or less severity, till Tuesday, when the pulse fell to sixty, and presented some intermittence; the surface of the body became cold, and vomiting set in. The next day the conjunctiva became yellow, petechial spots supervened, and suppression of urine soon after attracted attention. The jaundice from this time gradually spread over the body, and increased in intensity. To these were added, on Friday, black vomit (some ounces of which were sent by Dr. Bournonville to the Board of Health), and, successively, black, tar-like stools, vomiting of unchanged blood, hemorrhage from the gums, and other symptoms equally characteristic. I must, while on the subject of symptoms, call your attention to the circumstance that in this patient the suppression of urine, which, as already stated, occurred on the third day, did not, as is usually observed in fatal cases, continue to the close of life.

Dr. B. writes on the fourth day that the patient "has not passed water for two days; no indication of the presence of urine in the bladder." On the fifth the doctor says: "Has not passed his urine; no discomfort on that account." At the close of the same day the doctor passed a catheter, and drew off two tablespoonfuls of thick, oil-like urine. The next day he again resorted to the catheter, and removed a wineglassful of yellowish-brown urine. On the eighth day about a gill of dark yellow urine, more limpid than the former portion, was removed. The next day, Sunday (ninth day), in the afternoon, Dr. B. "drew off $1\frac{1}{4}$ pint of thin, dark urine." The same quantity was drawn off the next day; and, as we shall see presently, the bladder was found, on dissection, to contain about a gill of fluid.

The case lingered till 11 o'clock P. M. on Monday, the tenth day, when he died at the City Hospital, whither he had been removed a short time previous, in order to allay the excitement produced in the vicinity of his residence by his presence.

The second case occurred in the person of one of the crew of the U. S. steamer *Water-Witch*, which arrived at the Lazaretto on the last day of September, from the south side of the island of Cuba. During the passage the *Water-Witch* lost two men with decided and unmistakable yellow fever, and, on reaching the quarantine station, landed five cases—four with a mild but well-marked form of the disease, while the other, the subject of the present remarks, who at his admission in the hospital had reached the fifth day of the attack, exhibited a combination of symptoms of a most formidable character. The appearance of the disease in the above-mentioned vessel presents some points of interest to which I must be allowed to invite your attention for a few moments. The *Water-Witch* lay for some time at anchor at the distance of about a mile from the port of Cienfuegos. While there, boats were sent to the town morning and evening each day—in the morning for provisions, and in the evening to convey the officers. No yellow fever prevailed in the place or among the shipping, which, unlike the *Water-Witch*, were anchored at the wharves, and consequently close to the town. No sickness occurred on board of the latter vessel, except a few cases of ephemeral fevers (*calenturas*), presenting none of the characteristic symptoms of, and in no way to be confounded with, yellow fever.

The vessel next went to the Isle of Pines, the healthiest spot in all Cuba, and where, so far as I can learn, yellow fever seldom, if ever, originates. There, however, the disease broke out in the vessel, and, after the occurrence of several cases, the captain determined to bring his vessel to the United States, in order thereby to arrest the further progress of the infection. Now, it was matter of observation that, with one solitary exception, none of the men attacked, whether at the Isle of Pines or on the passage home, had been ashore at Cienfuegos, or had manned the boats that plied from the vessel to that place mornings and evenings. Hence, even had the disease prevailed in the town or among the shipping—which, as we have seen, was not the case—those that sickened could not have taken it there. The exceptional case was not the first to suffer, and had so mild an attack as to leave a strong doubt as to the disease being the true yellow fever. I may mention that it is equally certain that though the first cases occurred at the Isle of Pines, they could not, for the reason stated, have originated from a poison derived from that place. My opinion is, and I give it for what it is worth, that the poison originated on board. One thing is certain, that all the cases were traced to the same part of the vessel—the fore division of the main deck, in the vicinity of the galley. This part is small—

much, very much too small for the number of men confined in it, considering, especially, the climate of the region where the vessel was cruising, and the additional heat issuing from the galley. It is, besides, low, badly ventilated, and near the pumps and hatches, from which issued a highly offensive smell, proceeding from the hold.

Of the symptoms noted during the progress of the cases in question I shall say little more than that they could not be referred to any other disease. They were so referred, not only by myself, but also by the medical officer of the vessel, Dr. Engles, by Dr. Filbert, of the Lazaretto, and by my excellent friend, Dr. Dickson, who saw the patient at my request, and whose familiarity with all the forms of yellow fever renders his opinion relative to its diagnosis of the utmost value. All I need add, in this connection, is, that the disease was protracted to the tenth day; that the jaundice was less marked than in the preceding case; that the black vomit was late in making its appearance, but at last assumed its usual characteristics; and that the suppression of urine, after continuing several days, ceased, a fair quantity of fluid being drawn several times a day by the catheter.

1. *Case of Sporadic Yellow Fever.* Examined at the City Hospital, September 11, 1860. Present, Drs. La Roche, Jewell, McCrea, Bell, Kane, and Bournonville.—Rigor mortis very well pronounced. Body yellow, with a good deal of congestion of posterior surface and extremities. A quantity of dark liquid was oozing from the nose, and a flow of yellow, frothy mucus, tinged with blood, took place when the body was moved.

The areolar tissue, subcutaneous, submucous, and subserous, was universally tinged deep yellow, and was remarkably firm. In the costal cartilages it was evident that this tinging was confined to the perichondrium.

Both *lungs* were healthy, but hypostatically congested; the pleuræ full of deep yellow serum. The *heart* was of good size, and its lining membrane stained yellow; its tissue was degenerated, the change being perhaps fatty, but with a granular appearance under the microscope. The pericardium was deeply tinged yellow, and contained about f3j of very yellow serum; at one portion of its parietal layer there was a spot of well-marked extravasation. The left cavity of the heart contained a firm yellow clot; the right auricle was distended with a very large clot, of a dark, currant-jelly-like appearance. Both the aorta and the pulmonary artery were stained red.

The *stomach* contained a black or very dark brown grumous matter, and its mucous membrane presented here and there patches of extravasation. The *duodenum* seemed congested and softened, the rest of the intestine healthy. The *liver* had a decidedly fatty appearance, but did not exhibit to the full extent the oil-globules when examined microscopically; its colour was very light, and its tissue easily torn. The gall-bladder was distended with intensely black viscid bile. The portal vein was distended with fluid blood. The *spleen* had externally a greenish colour, from the yellow tinge of its capsule; its pulpy substance was very red and soft. The *kidneys* were enlarged and congested, the supra-renal capsules apparently healthy. Some enlarged glands were noticed around the pancreas.

2. *Case of Yellow Fever, from U. S. Steamer Water-Witch.* Examined at the Lazaretto, October 3, 1860, with Dr. Engles, U. S. N.—Body examined twenty-two hours after death; very muscular and well formed. It was not yet cold, although the rigor mortis was very well pronounced. The surface was somewhat tinged yellow, and there was slight ecchymosis in the posterior portions, in the ears, penis, and scrotum, not much in the

extremities. About the mouth there was a quantity of dark brownish foam. On lifting the body, and changing the position of the head, a copious flow of bright red blood, apparently arterial, took place from the nostrils.

Thorax.—Old pleuritic adhesions were found at the lower part of the right lung. Both *lungs* were healthy, but presented an intense degree of hypostatic congestion. The *pericardium* was distended with air, a circumstance I do not remember ever observing before, and which I cannot easily explain. The *heart* was healthy in appearance, but its texture seemed degenerated. A microscopic examination subsequently made, with Drs. Darrach and Da Costa, showed that it had undergone fatty change, as was the case also with the liver. Both the aorta and pulmonary artery were deeply stained red. The blood was everywhere entirely fluid.

Abdomen — *Liver* fatty in appearance; gall-bladder excessively distended, and stained with intensely black viscid bile. *Stomach* deeply congested at its lesser curvature; towards its cardiac extremity its mucous membrane presented a dark greenish colour, with an appearance here and there of minute white vesicles. *Duodenum* slightly congested; *intestines* not decomposed at all, but congested at some places for a considerable distance; they were full of a dark, viscid, tarry matter; the solitary glands were enlarged, as were also the lymphatic glands of the mesentery. Peyer's patches were healthy. The *spleen* was rather large, intensely congested, and almost diffuent. The *kidneys* were flabby, but not otherwise abnormal. On the external surface of the supra-renal capsules there was noticed some injection of the vessels.

I shall add but a few words to what you have just heard. In regard to the condition of the liver, you will notice that in neither of the cases did the organ exhibit the fatty degeneration to the extent it generally does in fatal cases of the disease under consideration. Nevertheless, it was in both decidedly oily; at any rate, sufficiently so, in all respects, to justify us in adhering firmly to the opinion, now very generally entertained, that this condition of the liver ranks among the main, if it is not *the* most important, of the anatomical characters of the disease. I may mention that in both instances additional proofs were obtained of the correctness of the opinion that the change is due to a fatty and not to an amyloid degeneration, as suggested by Prof. Joseph Jones, of Georgia, and maintained, at one time at least, by Prof. S. Jackson, of this city. This fact, so far as I can ascertain, was first experimentally demonstrated by our fellow-member, Dr. Da Costa, in a case from the West Indies, which was admitted, under his care, in the Episcopal Hospital some two years ago (August, 1858). In this instance, as in the two before mentioned, the oily matter was partially dissolved by ether, and not acted upon by iodine or sulphuric acid.

In both cases before us this evening the heart was found granular. This condition of the heart in fatal cases of yellow fever was, so far as I can ascertain, first pointed out by Prof. Riddell, of New Orleans. By him it is looked upon as the result of the molecular disarrangement of the muscular fibres of the organ, in virtue of which those fibres lose their natural structure, and assume the non-striated or granular aspect. "In general," as he remarks, "the molecular change is either complete or well marked, rarely it is very slight or imperceptible." "The molecular degeneration of the heart, unlike the fatty change of the liver, can always be satisfactorily made out, affirmatively or negatively, with a good microscope. It is on this account, perhaps, that in fatal cases of yellow fever the molecular change in the heart

has appeared to me to be a lesion much more constant than fatty degeneration of the liver."

Whatever may be the fact relative to the frequency of the change in question, I very much doubt the correctness of the conclusion at which Prof. Riddell has arrived, that it is more constant in the cases mentioned than fatty degeneration of the liver, inasmuch as I did not fail to discover this last lesion in the many autopsies I witnessed in 1853 and 1854, and some I have had occasion to witness since. I know, besides, that the same result has been obtained by pathologists in other places in this country and abroad, and more especially, and on a large scale, in Lisbon, during the severe epidemic by which that city was visited in 1857. Dr. Lyons, the able historian of that epidemic, does not use on the subject language calculated to make us conclude that the change was there only of occasional occurrence, or yielded to any other in point of frequency. His words are worth recording. "The most remarkable, the most constant, and, to my mind—I will frankly avow it—the most inexplicable condition presented in the *post-mortem* examination of fatal yellow fever cases, was the state of the liver. I believe it may be affirmed that *some departure from the normal state of this organ was an absolute condition in all cases which proved fatal*. It was not only so with regard to the cases examined by myself, but the concurrent testimony of all my learned confrères in Lisbon pointed to the same result." "The fawn-yellow coloration usually well indicated the change that had taken place in the hepatic tissue; but it was not only in those cases in which the liver presented this coloration that abnormal states of the hepatic structure existed. The yellowish-brown colour—'*chocolat au lait*'—was attended with similar and fully as well-marked changes. The same may be said of the nutmeg condition of the organ, and even, to some extent, of several cases in which the ordinary liver-brown colour was unchanged."

"Minute fine sections, or matter scraped from the hepatic texture, exhibited the hepatic cells filled with globular oily and fatty matter. The natural appearance of the cell was completely altered, its outline obscured, and its nucleus rendered invisible. It was surcharged with molecular and globular oily matter, while the whole field and the interspaces between the cells were filled with similar and equally abundant oily and fatty elements, were in fact dissipated, and the contour of the cells brought more clearly into view; but it was only rarely that the nucleus could, by long treatment in ether, be made visible."

In view of what precedes, I cannot conceive how the above-mentioned change of texture of the heart can justly be said to be much more constant in fatal cases of yellow fever than the fatty degeneration of the liver. How a particular pathological condition of a given organ can present itself more constantly in the aforesaid disease than the lesion of another organ which is always found on the dissection of subjects who have died of the same disease, is a puzzle, the solution of which I leave to others. But while unwilling, from all I have seen myself or learned from others, to admit that the change in the liver is less frequently noticed in fatal cases of yellow fever than the above-mentioned granular condition of the heart, I cannot but be disposed to regard the latter, I will not say as a pathological change of constant occurrence in such cases—in other words, in the light of an anatomical character of the disease—but certainly as one very frequently noticed, and which deserves serious consideration in a pathologico-diagnostic point of view.

I lay no stress on the circumstance that Dr. Lyons, though describing the condition in which he found the heart in his dissections at Lisbon, says nothing of the one under present consideration, inasmuch as, from his silence on the subject, we cannot conclude that it did not exist. At least such is my belief, founded on the fact that we are nowhere told by that pathologist that he examined the substance of the heart in the only way in which the granular deposits can well be discovered—namely, under the microscope. Be this, however, as it may, this particular morbid change was noticed by Dr. Da Costa in the case already mentioned as having occurred in the Episcopal Hospital. I hasten to state that Dr. Da Costa was not at the time aware of Prof. Riddell's observations on the subject. Dr. D. was kind enough, knowing the interest I take in all matters connected with the yellow fever, to show me the specimen in his possession. I have since seen the same change in another specimen exhibited to me by a friend. Dr. Da Costa, I am told, has also again seen it; and now we find it in both of the cases to which your attention has been invited this evening.

In conclusion I have to state that Dr. Riddell says nothing of the nature or composition of the aforesaid granules. To Dr. Da Costa credit is doubtless due for having decided the question. In the Episcopal Hospital case the heart was found soft and flabby; the muscular fibres were exceedingly granular; here and there oil-drops were seen; the striæ of the muscle were observed; and the granules were to a great extent soluble in ether, but not in acetic acid. I was fortunate enough to witness this examination and experiment, and, were it necessary, which we all know is not the case, could vouch for the truthfulness of the above statement. The same result was obtained in both instances before us. The conclusion from this is that the granules are fatty, the consequence of a commencement of fatty degeneration. I have no hesitation in claiming for Dr. Da Costa credit for the discovery of that fact.

Nov. 28. Medullary Cancer of the Liver.—Dr. CHARLES C. LEE presented a specimen of this, with the following observations: The subject of this case was a girl, 22 years of age, born in Glasgow, Scotland. Her father died four years ago, in his sixty-seventh year, of *cancer of the breast*, as she was told by the physician who attended him; her mother and the rest of her family were healthy. The patient herself had no recollection of any ill health before her twelfth or fifteenth year, when she contracted a severe cough. This continued three years, when she came to Philadelphia for medical advice, and was admitted to the Pennsylvania Hospital in June, 1859. Here she remained several months, recovering in some degree from her cough, which, however, returned after she left the hospital, and in the beginning of last January she entered the medical wards at Blockley for the same trouble. In a fortnight she was much better, and was discharged to the out-ward, or almshouse proper; but on the 5th of May she was sent back, with a severe attack of jaundice, accompanied by sharp, lancinating pains in the hepatic region. From that time until her death, on the 24th of November, menstruation was completely arrested, and her health generally became deranged. Although subjected to various plans of treatment by Drs. Da Costa, Judson, and Ludlow, who successively had charge of the case, the liver advanced steadily in size, gradually extending above the ensiform cartilage, and downwards to within a quarter of an inch of the umbilicus, behind to the spinal column, and deeply into the left hypochondriac region in front. Over this space the skin was exceedingly tense; the

whole body was deeply jaundiced and emaciated, except the lower extremities, which remained œdematous until the last.

Autopsy, thirty-six hours after death.—Body œdematous, but not equably so; right arm and side generally much fuller than the left, particularly the upper extremity. Eyes and skin of entire body deeply injected with bile; no rigor mortis observable. On opening the abdomen, the liver was found immensely enlarged, extending on the left side three inches, and on the right four inches below the ribs; upwards to between the second and third ribs. The diaphragm was adherent at several points. The right lobe was much more enlarged, proportionally, than the left, and was also soft and fluctuating. The entire weight was $15\frac{1}{2}$ pounds. When the right lobe was incised, about a quart of thick, dark brown liquid was discharged, consisting, apparently, of broken-down liver-structure, for no pus was perceptible; floating in this liquid were seen, here and there, small masses of whitish pulp. A portion of this fluid was submitted to microscopic examination by Dr. Woodward, whose report is as follows: "The whitish pulp is composed of large spindle-shaped cells in great abundance, a few free nuclei, and large oval cells with single and plural nuclei and nucleoli. A mass of this character imbedded in the substance of the liver would be *medullary cancer* of the liver, and such cancer masses often soften centrally so as deceptively to resemble an abscess, if studied only with the naked eye. They may be single, or several may exist in the same liver, with or without cancer elsewhere." The piece of liver tissue sent to Dr. Woodward was fatty, and so altered as to be scarcely recognizable. In the peritoneum was found about a quart and a half of serous effusion, strongly tinged with bile. The heart was normal; lungs sound, but compressed, especially on the right side. The kidneys and spleen were natural.

Case of Mola Hydatidosa.—Dr. KELLER reported this case as follows: Mrs. E., 32 years of age, of healthy constitution, the mother of five children, had her menses last on the 6th of January. She felt otherwise quite well until the 18th of November, when, at 5 o'clock P. M., she was taken with regular labour pains. Four hours afterwards the mass I now exhibit was expelled. She lost but little blood, and the os tincæ contracted readily.

The tumour weighed not quite four ounces, was of an ovoid form, $1\frac{1}{2}$ inch wide, and $2\frac{1}{2}$ inches long. On one side it showed a smooth surface, with no appearance of direct vascular communication with the uterus. On the other side there was a perforated empty sac, formed by a thin membrane, covering the undulating surface of the rest of the tumour. In opening that part of the mole next to the womb, it presented a perfect framework of white fibres and cyst-like formations. The fibres were in many instances straight, in others of the rosary-like appearance. They consisted, under the microscope, of connective tissue, covered in many spots with the characteristic villi of the chorion. Some of these were filled with regular cells, others contained only small molecules. In other instances the fibres of the connective tissue ended in cell-formations. The smaller nodules in connection with the fibres were solid, and consisted simply of villi. The larger ones, of the size of small beans, were connected to the framework by minute filaments, and showed on the surface capillaries. I did not examine them, thinking to be able to preserve their characteristic appearance until this meeting of the Society. Next day, however, I found them entirely collapsed. Between this formation and the membrane before mentioned there was an elastic mass, reddish on section, and looking like a fresh fibrinous

clot. Under the microscope this was seen to consist of unorganized granular matter.

Aside from different isolated cell-formations, this mole consisted of—1st, connective tissue; 2d, villi belonging to the chorion; 3d, unorganized fibrin; 4th, the membranous sac covering the free surface.

It may be added that Prof. Virchow has found in the distended villi of a fresh “*mola hydatidosa*” a substance which he calls *mucous tissue*, which he considers as distinguishable from fully-formed connective tissue and colloid matter by its reaction with acetic acid.¹ He has found it also in the umbilical cord, vitreous body, and in many pathological products which have been classed with colloid growths.

Dr. KEATING spoke at considerable length on the subject of uterine moles. His experience led him to believe that they were invariably the result of blighted ova; fatty degeneration then ensued; and they might either be discharged by a slow oozing of an oily substance from the womb, or in a mass; so altered, however, as to give but little evidence of their origin. From the history of the cases which he had met with, he believed that the death of the foetus almost invariably preceded the formation of the hydatiginous mole. The effect of prolonged lactation during pregnancy was instanced, as causing slow and persistent contractions in the uterus, sometimes producing inflammation of the placenta, and consequently death of the foetus from obstruction of the placental circulation; oftener, however, the death of the foetus ensuing as a simple result of the continuous uterine contractions detaching or constricting portions of the placenta, and thus involving the death of the foetus. In two instances which had come recently under his observation the ova were so altered as to present scarcely a trace of their original organization. It was hardly necessary to call attention to the fact that the hydatiginous character of these moles is entirely due to the villi of the chorion, which do not cease developing upon the destruction of the ovum. In one lady under Dr. K.’s care, who had discharged, in the course of four years, three of these hydatiginous moles, he had detected a latent constitutional poison. A continued appropriate treatment was followed by the birth of a healthy child at full term. The three hydatiginous masses just referred to gave evidence, on examination, of placental inflammation, probably the result of the action of the constitutional virus. Dr. K. begged to call the attention of the Society to the fact that constant and violent retching in the early months of pregnancy might also induce slow contractions in the uterus, destroy the ovum, and give rise to formation of hydatiginous moles. He then dwelt upon the frequency with which hemorrhages from the uterus, caused by the *débris* of portions of the foetus remaining in the uterus after abortion, were mistaken by practitioners for repeated miscarriages. He cited, among others, an instance of a lady in whose case he had been recently called in consultation on account of repeated abortions, or floodings from threatening abortion, as supposed by her medical attendant. She had been kept several months in the horizontal position, and had had several applications of the nitrate of silver to the os and cervix uteri, for supposed ulceration. The history of the case led him to suspect the existence of some foreign body within the cavity of the uterus. An exploration with Simpson’s sound having convinced him of the correctness

¹ Mucous tissue containing *mucin*, which is precipitated by acetic acid, and insoluble in an excess of it; connective tissue and colloid matter being affected, if at all, in the way of solution.—*Sec. Path. Soc.*

of his suspicions, he succeeded in extracting a mass of the size of a large walnut, flattened, and consisting of concentrated laminæ of a fatty substance, being undoubtedly the remnants of the placenta of the last aborted ovum. Upon its extraction the floodings ceased; the lady entirely recovered her health, is again pregnant, and has nearly reached the full term. The occurrence of four such cases in Dr. K.'s practice induces him to believe that they are much more common than is generally thought. It behooves the obstetrician, therefore, always to insist upon examining the matters discharged from the uterus; and in cases of doubt, and especially where the hemorrhage from the uterus, as in the cases just referred to, is prolonged, or is constantly recurring, it is incumbent upon him to make such an examination of the uterus as to place all such cases beyond a doubt. In conclusion, he firmly believed that all uterine moles were invariably the results of impregnation, the clots and membranous discharges from the unimpregnated uterus having a distinct character and history.

Dec. 12. Chronic Alcoholism.—Dr. HALL exhibited a heart, the muscular tissue of which was in a state of marked fatty degeneration. The patient was a tall, strongly-built, married female, about 55 years of age, but rather younger in appearance. For nine or ten years past she had been an habitual but secret dram-drinker. Although hardly ever drunk, she could scarcely ever be called perfectly sober.

On the 24th of November she first came under notice, having complained, however, for two weeks previously of pains in her limbs. Before this, although unable to go out of doors, she could go about her house, but now she became so weak as to be obliged to keep her bed. She complained of total loss of appetite, and of sleeplessness; her pulse was 96, and soft; her tongue coated. She had no headache, and her vision was not affected; she had no delirium, no tremor, no pain in the chest or abdomen. Her bowels were constipated. She was quite helpless, and unable to sit or stand without support. The vacant, abstracted manner of this patient would strike the observer as peculiar. Apparently taking no notice of surrounding objects, she answered correctly, though slowly, when spoken to. Although doing so, there was a certain amount of hesitation or difficulty of articulation, as though there was a want of co-ordination between the different organs of speech, exceedingly like a symptom apt to occur in the progressive paralysis of the insane.¹

This, in fact, was the cause of alarm to the family, and led them to call in a physician. At 5 o'clock the previous afternoon she appeared unable to answer a question put to her, although her lips moved in the effort to do so. The affection of the motor and sensory apparatus was peculiar. There was almost entire loss of sensibility in the left leg up to the knee, even when pinched, whilst the sensibility of the thigh appeared to be much increased. This was also the case, to a less extent, in the arm of the same side. In the cases reported by Dr. Huss there was a constant restlessness of the limbs observed, moving them up and down in the bed. Here it was only observed in one of the arms—the right—which was constantly raised up and let down again by the patient. The appearances present were such as would most naturally and readily be referred to the nervous system. Their gravity, however, was not to be accounted for by the actual

¹ On the Progressive Paralysis of the Insane. By Wm. Wood. Brit. and For. Med.-Chir. Rev., July, 1860, p. 187.

amount of disease, and would have rendered the diagnosis a difficult one, had it not been for the elaborate description by Dr. Huss of a disease prevailing among excessive spirit-drinkers in Sweden.¹

Under moderate stimulation some little improvement took place, but it was not of a permanent character. She had a slight convulsion on the morning of December 1, which, however, did not seem to have any effect upon the general symptoms. This condition of things continued a week longer, when she became comatose, but without stertor; the sphincters were relaxed, and the left arm completely paralyzed. The pupils, though contracted, were mobile; and upon pinching the unaffected limb, she was able to draw it up. She died very quietly on the morning of the 10th inst.

Autopsy, thirty-seven hours after death.—Head not examined, the family having refused permission.

Chest.—The lungs presented nothing of importance. The mediastinum was loaded with fat; the pericardium normal in colour and texture, but distended with serum to the amount of six ounces. The *heart* was on its external surface loaded with fat, which also appeared to have encroached upon the proper muscular tissue of the walls. The organ appeared flabby, dilated, and attenuated, the left ventricle more particularly. The valves were in normal condition. The *aorta* was quite healthy.

Abdomen.—The omentum was loaded with fat, extending almost to the pubis. There was an excessive deposit of fat throughout the abdominal cavity. The *stomach* was, in its pyloric half, much reddened, and slightly mammillated; the redness was not altered by washing. Otherwise the mucous membrane appeared to be in a healthy condition, without any evident thickening or softening. The *liver* was partially enlarged, the left lobe equalling the right in size. The entire organ was pallid, and in a state of moderate fatty degeneration. Gall-bladder distended. The *kidneys* were pale, and anæmic-looking; upon the upper surface of the left there was a small cyst.

The above case presents so many points of interest, both to the pathologist and physiologist, and so closely resembles in its symptoms the descriptions of Huss, that it was thought not unworthy of contribution to the literature of the subject.

1861. Jan. 9. *Intestinal Concretion in the Appendix Cæci, causing Perforation and fatal Peritonitis.*—Dr. PACKARD presented this specimen. The patient had been under the care of Dr. J. F. Meigs, who supplied the ante-mortem history of the case.

T. D. S., a healthy, well-grown boy, 11 years of age, rose on the morning of December 25, 1860, apparently quite well. Soon afterwards, however, he complained of pain in the right iliac and lumbar regions, was chilly, and returned to bed. A dose of castor oil was given him. In the course of the day fever came on.

Next day Dr. M. was called, and found him feverish, with a pulse of 132, a hot and dry skin, and a moderately furred tongue. The pain still continued, with tenderness and slight distension of the abdomen on the right side; there was no vomiting. His bowels had been acted upon three times by the oil. Leeches and a poultice locally, and a mixture of blue pill with rhubarb syrup internally, were ordered.

¹ M. Huss, Chronische Alkoholskrankheit, oder Alkoholismus Chronicus. Aus dem Schwed. von Gerh. v. d. Busch. Leipzig, 1852. p. 8.

On the 27th and 28th the symptoms were much the same, except that the tenderness and distension increased. The pain was aggravated by coughing, by a full inspiration, and by motion, especially of the right leg. Bowels slightly moved by the mixture; no vomiting as yet. His fever continued, but his pulse fell to 108, and his skin was somewhat cooler.

On the 29th he was worse. All his symptoms were aggravated, and vomiting set in; his bowels became confined. Small doses of calomel and opium were given, enemata of various kinds were tried, and rhubarb syrup with a little fluid extract of rhubarb was perseveringly employed, but without effect.

The abdomen now became greatly distended, exceedingly sonorous, and painful; the stomach grew more and more irritable, rejecting from time to time, towards the last, with a sudden, spasmodic effort, everything that was taken by the mouth. The bowels were completely obstructed, so that repeated injections of various kinds elicited no discharges, even of flatus. The urine continued to be secreted to the last; and there was at times, in spite of the nausea and vomiting, quite a strong desire for milk and bread.

During the last few days wine-whey and beef-tea were given in small quantities; and opium by enema and by the mouth was used to allay pain. On the third day of the treatment a blister four inches square was applied over the seat of tenderness; but neither this nor any of the other remedies employed seemed to exert the least effect upon the course of the disease.

Death took place on the 1st of January, 1861.

The *autopsy* was made by Dr. Packard, twenty-four hours after death.

Body large, muscular, and well formed; rigor mortis well pronounced. Abdomen only examined.

On making the usual section, several coils of small intestine, very greatly distended with gas, and markedly injected, with flakes of lymph here and there over the surface, at some points gluing the adjacent coils together, were seen concealing the rest of the abdominal viscera. After some search, the colon was found, very much contracted, except at the cæcum. The ileum was in like manner contracted, the narrowing beginning at about the end of the jejunum, which formed the distended coils above mentioned. No cause was assignable for the constriction at this point, but a little lymph was thrown out here, and it may have been that the bowel had been twisted.

The appendix vermiformis was bound down by peritoneal adhesions; within it, near its root, was a mass as large as a small bean, but perfectly oval. Just beyond this mass, at what seemed to have been its position, was an ulcer, extending all round the tube, and of a gangrenous aspect. At the distal end of this ulcer was a perforation, by which matter had found an exit into the peritoneal cavity. The rest of the tube looked as if it had been distended by the pus before the opening was formed.

After its escape from the appendix, the matter seemed to have caused a circumscribed peritonitis, in addition to the general one already indicated. The adhesions bounding this peritonitis had extended up to the liver, the convex surface of which was hollowed to a slight depth in an oval shape, the depression being lined by false membrane. The whole quantity of the pus was perhaps fʒiv.

The liver was pale in patches, but was not degenerated. Rather a larger number of oil-drops existed in a dark, inflamed portion of its substance just beneath the depression above mentioned, but even here the quantity was not great.

Mesenteric glands swollen, and injected over the surface.

No other lesions were observed.

Jan. 23. Serous Cysts of the Breast.—Dr. H. LENOX HODGE, in presenting this specimen, said: This breast was removed by Dr. Norris from a lady about 35 years of age. She first noticed a tumour in her right breast some seven years ago. It did not appear to increase much in size until last fall. It then began to enlarge, and at the time of the operation presented the appearance of an irregular tumour about two inches in length and breadth. Both breasts were small; the lady had been married for four years, but had never been pregnant. The whole gland was removed, as other cysts were probably scattered throughout it. During the operation the largest cyst was opened. It contained an ounce or two of thin serous fluid. There were found to be some ten cysts, all apparently situated in the midst of the tissue of the gland. They varied much in size, the smallest being about as large as the little beads of children's toys. They were provided with a distinct membrane, and could be readily turned out entire from their bed of fibrous tissue. The contents of most of them consisted of a light-coloured, thin serum; but in a few the fluid was of a deeper colour, and much more glutinous. The microscopical examination of the cysts, their contents, and the surrounding tissue, has been made by Dr. Woodward.

Dr. WOODWARD said that simple serous cysts of the mammary gland were so rare, that he had examined the present specimen with considerable interest. Cysts associated with neophytes of connective tissue (cysto-sarcoma), or with cancer (cysto-carcinoma), were far more common. A portion of the specimen before the Society had been presented to him by Dr. Hodge, and he had examined it, with the following results:—

Imbedded in the morsel of gland-tissue was what at first appeared to be a single cyst, but which more exact observation showed to be three cysts; the largest about the size of a small cherry, and the smallest that of a pea. They were in the closest juxtaposition, and only separated by the thinnest septa.

On puncturing the largest of these cysts, a *clear yellowish fluid* escaped, which became slightly turbid on boiling, showing the presence of a *very small* quantity of albumen. Repeated examinations showed no forms in this liquid, except a few minute oil-globules. The *walls* of this cyst were vascular, and composed chiefly of well-developed white fibrous tissue, in which were imbedded a considerable number of more or less atrophied fat-cells, in which the *nucleus* and a number of drops of diverse size, and composed of high-coloured fat, could be distinguished readily.

The fluid of the second cyst was more viscid, coagulated more abundantly, and therefore contained more albumen than the first. It also contained large numbers of large, pale cells, with delicate contours and dimly granular contents, $\frac{1}{800}$ to $\frac{1}{800}$ of an inch in diameter, and each containing a clear nucleus of from $\frac{1}{3300}$ to $\frac{1}{2500}$ of an inch in diameter. Some of these cells were partly destroyed, and the nucleus set free, with or without a portion of the contents adhering to it. They were sometimes adherent by their edges, and were evidently derived from the *distinct pavement epithelium* with which the cyst was lined. This consisted of tolerably regular hexagonal cells, rather smaller and more granular than the bloated ones floating in the fluid—viz., $\frac{1}{1200}$ to $\frac{1}{1000}$ of an inch in diameter, with nuclei $\frac{1}{5000}$ to $\frac{1}{3000}$ of an inch. The epithelium reposed on a fibrous layer, constituting the wall of the cyst, and, like that of the first, vascular, and containing atrophying fat-cells. The third cyst was not examined microscopically. The tissue of the mammary gland was, in every respect, perfectly normal, so far as examined; the gland-vesicles presenting the

usual characteristics. Groups of gland-vesicles, with the accompanying milk-ducts, also perfectly healthy, were imbedded in the outer layers of the walls of the cyst.

With regard to the origin of mammary cysts in general, Dr. W. expressed the opinion that any exclusive doctrine on this subject must inevitably fall to the ground in the presence of individual cases. Mammary cysts may undoubtedly arise by the distension of one or more of the ultimate gland-lobules; perhaps even a lactiferous duct may dilate into a cyst in consequence of obstruction. But besides these methods, by which the gland itself might contribute to cyst-formation, cysts may arise in the areolar tissue connecting the lobules together, by a dilatation of one or more of the areolæ. Rokitsky's idea, that cysts are special heterologous products, which begin as microscopic nuclei, and dilate into cysts, had not met with general credence.

Besides the theories of cyst-development with which he was familiar, one had suggested itself to him which he considered worthy of mention, and that was the possibility of the formation of cysts, by a process of *vacuolation*, in an intercellular matrix, similar to that by which the areolæ of areolar tissue, the intercellular passages in leaves, and perhaps all the closed cavities of the body, are developed.

He had seen in the villi of the chorion, in a case of so-called "hydatid mole," microscopic cysts which appeared to be merely cavities formed by the solution of the jelly-like intercellular tissue, pushing the cells aside, and limited apparently by no distinct wall, other than the substance in which they were excavated.

In the mammary cysts above described, he believed, the presence of the atrophying fat-cells in the walls was sufficient to show that the tissue of the cyst-wall was not of new formation, but represented the normal connective tissue of the gland, pushed aside, and distended by the accumulation of the liquid. The cyst described as lined by epithelium he was disposed to regard as having originated in the dilatation of one or more of the gland-lobules; the epithelium being furnished by the overgrowth of the secreting cells of the lobule. The first cyst, which was larger, might represent one of similar origin in which the epithelium had perished; or perhaps it had originated in the connective tissue between the lobules, and not in these themselves.

FISKE FUND PRIZE ESSAY.

ART. XII.—On “*The Morbid Effects of the Retention in the Blood of the Elements of the Urinary Secretion.*” By WILLIAM W. MORLAND, M. D. (With two woodcuts.) The Dissertation to which the Fiske Fund Prize was awarded, July 11, 1860.¹ (Published by request of the Rhode Island Medical Society.)

No organs in the human body play a more important part in the economy of life and health than the kidneys—their office is *the depuration of the blood*. In however slight a degree their function is interfered with, some untoward effects are produced. These may often be barely noticed, and easily recovered from; in many instances, however, although disregarded at first, they are sure of their ground, hard to be dislodged, and too frequently insidious and widely and surely destructive. The more open and overwhelming attacks of disease, which, by rapidly disabling the kidneys, and extensively injuring their tissue, at once and distinctly tell upon the constitution, reveal in plain characters the close connection between the vital torrent and its purifying agents.

The subject, as proposed by the Trustees of the Fiske Fund, necessitates, *first*, the enumeration of “the elements of the urinary secretion;” and *secondly*, the recital of the effects produced by the undue “retention” of each of them in the blood.

By the expression “elements of the urinary secretion,” as here used, we understand its constituents in a state of health. These constituents, by a vital law, are to be eliminated from the blood; and their retention therein, beyond a certain time, will certainly cause “morbid effects.”

The following enumeration of the urinary elements is taken from one of the latest and most reliable authorities.² The analysis is made up from an average of the composition of all the urine passed in twenty-four hours. *Average quantity* from twenty-four hours, 1400 to 1600 cubic centimetres;

¹ The Trustees of the Fiske Fund, at the annual meeting of the Rhode Island Medical Society, held in Newport, July 11, 1860, announced that the premium of one hundred dollars offered by them on the following subject: “The morbid effects of retention in the blood of the elements of the urinary secretion,” had been awarded to the author of the dissertation bearing the motto—

“Prius cognoscere, dein sanare.”

And upon breaking the seal of the accompanying packet, they learned that the successful competitor was Wm. W. Morland, M. D., of Boston, Mass.

JAMES H. ELDRIDGE, M. D., East Greenwich,

CHARLES W. PARSONS, M. D., Providence,

HENRY E. TURNER, M. D., Newport,

Trustees.

S. AUG. ARNOLD, M. D., Providence, *Secretary of the Fiske Fund.*

² J. L. W. Thudichum, M. D., Lecturer on Chemistry at the Grosvenor Place School of Medicine, &c. “A Treatise on the Pathology of the Urine, including a Complete Guide to its Analysis.” London, 1858.

49 to 56 fluidounces. *Average specific gravity*, 1.020. *Mean amount of solids*, 55 to 56 grammes (a gramme is 15.4440 grains, English).

Constituents.

Water	1345 to 1534 grammes.		
Urea	30 to 40	" =	463 to 617 grs.
Uric Acid	0.5	" =	7.5
Creatine	0.3	" =	4.5
Creatinine	0.45		7.0
Sarkine	} Undetermined.		
Uræmatine			
Uroxanthine			
Hippuric Acid	0.5	grammes.	7.5
Chlorine	6 to 8	" =	92 to 123 "
(or Chloride of Sodium	10 to 13	" =	154 to 200 "
Sulphuric Acid	1.5 to 2.5	" =	23 to 38 "
Phosphoric Acid	3.66	" =	56
Potash and Soda	} Undetermined.		
Lime and Magnesia			
Earthy Phosphates	1.28	" =	19
Iron	Undetermined.		
Ammonia	0.7	" =	10
Trimethylamine	} Undetermined.		
Carbonic Acid			
Phenylic Acid			
Damaluric Acid			

"The minor estimates account for 48 out of 55 grammes of solids, the larger estimates for 62 out of 66 grammes of solids."—THUDICHUM, *op. cit.*

From an examination of the above table, in connection with the requisitions of the subject, it will be evident that we have only to indicate the pathological effects arising from the undue retention in the blood, of the following constituents of the urinary secretion: Water; Urea; Uric Acid; Creatine; Creatinine; Hippuric Acid; Chlorine; Chloride of Sodium; Sulphuric Acid; Phosphoric Acid; Earthy Phosphates, and Ammonia. The other ingredients of the urine, mentioned as being found in "undetermined" proportions, cannot enter into the list, in a practical consideration of the subject.

WATER.—Taking up the urinary constituents in succession, we first examine the results to be observed when that amount of *water* which should be excreted through the agency of the kidneys, is not so evacuated. This portion of our subject may be comprehensively disposed of.

A very variable amount of fluid is evacuated from the bladder at different seasons of the year, and under peculiar and differing circumstances. Thus, in cold weather, the amount of urine is greater, because the cutaneous transpiration is less. Again, when large amounts of liquids are ingested, somewhat corresponding quantities are excreted by the kidneys. The action of abnormally produced sugar occasions diabetes; certain medicines induce or augment, whilst others restrict, or nearly suspend, the urinary flow. Organic disease, or accidental obstruction, may cause almost complete cessation of urination; and entire *anuria*, although rare, occurs, from well-known causes.

Whatever, therefore, essentially diminishes, or actually suspends, for a longer or shorter time, the urinary evacuation, causes the retention in the blood of *all* the constituents of the urine, or of a goodly proportion of them. The deleterious effects consequent upon such a retention, will be

referable, in the main, to the presence of the solid constituents of the urine, rather than to that of an unusual supply of the watery vehicle. A certain amount—a redundance, even—of water, is absolutely necessary in the circulation, in order to eliminate, wash out, and bear on, as through a sewer, the effete, nitrogenous products, foreign to life, and incompatible with the integrity of the blood. And, besides this necessity for a surplus amount of water, it is rare that enough more than a normal amount is retained in the blood, to be of essential consequence, compared with the effects arising from the presence of the solids of the urinary secretion, prevented from issue by the same cause or causes which retain the watery portion. It is true, however, that “when urea is retained, water is also mostly retained in part, and, by its effusion into the cavities and cellular tissue, causes dropsical disease.” (*Thudichum*, op. cit., p. 75.) But the action of the causes just alluded to is rarely or never sufficiently long maintained to be efficient in producing a deteriorated condition of the blood, *referable to excess of water alone*, the kidneys being healthy. Other morbid influences, arising from the presence of the solids of the urinary secretion, and the persistent action of the retaining cause upon the organs themselves, would produce far more rapid and appreciable effects upon the system at large, and upon the blood, than a simple increase of water, only, could do. It is acknowledged, however, that scanty urine—diminished both as to solid and fluid constituents—is indicative of a greater or less degree of anæmia. On the other hand, symptoms of hydruria may be favourable in certain diseased conditions—as where hydræmia and dropsy exist—and its actual establishment, either naturally or by artificial diuresis, may carry off the misplaced water, and restore the balance of the circulation. The profuse flow of watery urine in hysteria is often critical—at all events, *per se*, it indicates no blood-disease. Co-existent anæmia, in such cases, doubtless depends on some other cause than retention of the water of the urine in the blood, or its mere redundance. Often, also, where the quantity of urine excreted is very small, the skin, the bowels, and even the lungs, act *quasi* vicariously, and thus prevent or diminish any ill effects attributable to scantiness of evacuation of the watery portion of the urine. It is well known, also, that the skin will eliminate urea, in cholera, in such quantities that it not only can be detected, but the amount appreciated. (*Thudichum*, op. cit., *et alii*.)

We may now dismiss the watery element from our subject, and proceed at once to the consideration of the undue retention of the solid constituents of the urine in the blood.

UREA.—(Symbol: $\overset{+}{\text{U}}$.—Formula: $\text{C}_2\text{H}_4\text{N}_2\text{O}_2$.)¹

This substance, “the principal product of the metamorphosis in the body of nitrogenized food,” and always a constituent of healthy urine, is considered a blood-poison when retained in the circulation. Some observers

¹ Bird, *Thudichum*, *et al*.

Chemical Composition of Urea.

	THUDICHUM.	G. BIRD.
2C . . .	20.000	$\text{C}_2\text{N}_2\text{H}_4\text{O}_2 = 60.$
4H . . .	6.666	
2N . . .	46.667	
2O . . .	26.667	
	<hr/> 100.000	

believe its action to be direct, others that it is indirect—or exerted through the agency of a product of its decomposition. It forms the most considerable portion of the solids of the urinary secretion, and is purely excrementitious matter, the elimination of which by the kidneys is absolutely necessary to health and life. It is true that, in certain exceptional instances, large quantities of it have been ascertained to be present in the blood, for a long time, without compromising life, or even exciting those cerebral symptoms usually observed under such conditions; but it is to be presumed, either that the persons were, to a great extent, insusceptible of the action of urea, or else that the peculiar fermentation supposed to give rise to uræmic poisoning, by producing a noxious substance from the urea, did not take place. It is certainly very possible that certain persons may be less impressed by the presence of urea in the blood than others; but, we repeat, such cases must be entirely exceptional. With regard to the constancy of decomposition of urea when retained in the blood, and the consequent formation of another and a toxic substance, we have, as yet, too few facts to enable us to determine. If ever proved to be the rule, however, the intervention of certain unknown agencies might, in isolated instances, prevent its execution; and thus account for an apparent, or at least a temporary, immunity from morbid consequences.

After extirpation of the kidneys in animals, and in Bright's disease and some other affections, urea is found pervading many of the fluids of the body—as, the dropsical effusion, the blood, the perspiratory secretion, the vitreous and aqueous humours, and the liquor amnii.¹ Dr. Thudichum, who refers to its detection in the latter fluid by Wöhler, considers its presence there as exceptional; and is inclined, moreover, to throw doubt upon many of the reported instances of its occurrence in other fluids—the reports being in several instances merely assertions by the authors, and not ratified by proof, or else erroneously or partially quoted. Urea has also been declared to have appeared in the milk,² in the serum from blisters, and in the alvine evacuations of patients with diseased kidneys.³ Dr. Rees states that he has “found most unequivocal evidence of its presence in peritoneal, pericardial, and pleural effusions, and also in the fluid of the arachnoid.”⁴

With regard to the question, already alluded to, whether urea retained in the blood is directly or indirectly deleterious—that is, whether it acts *per se* as a poison, or becomes such by a process of decomposition, in which case the carbonate of ammonia is believed to be the injurious agent—there has, of late, been much discussion. The latter view has its zealous advocates, and their theory seems to be somewhat gaining ground. As we have already intimated, there are significant facts adverse to the conclusion that urea alone, as such, is a blood-poison. Dr. Bright remarked that urea may long exist in the blood, in renal disease, and yet no cerebral symptoms arise until the very last of life. He mentions one case which lasted from four to five years. Dr. Rees gives even stronger testimony. He found, in a patient who had no uræmic symptoms whatever, but who retained his cerebral functions to the last moment of his life, the blood more highly charged with urea than he had ever known it in Bright's disease. Dr. Johnson, of London, in his

¹ The presence of urea in the fluids of the body was first announced by Dr. Christison in the *Edinburgh Medical and Surgical Journal*, October, 1829.

² Rees, *Diseases of the Kidney*, London, 1850, p. 46. Albuminuria existed in the patient.

³ Dr. Golding Bird—after the action of elaterium.—*Urinary Deposits*.

⁴ *Loc. cit.*

justly celebrated work on diseases of the kidneys, affirms that no actual proof exists that urea is the poisonous agent, or, at least, that it is the only one. If admitted to have a poisonous influence, he holds that some peculiar, unknown condition of the blood must exist, to favour its toxic action. Frerichs is the author of the theory that carbonate of ammonia, resulting from a decomposition which the urea undergoes in the blood, is the poisonous agent. Its presence in the blood he indubitably ascertained, and by injecting it into the bloodvessels of dogs, he produced convulsions. Dr. Hammond, U. S. A., has made some interesting experiments, with the intention of testing this matter. He injected urea, vesical mucus, sulphate of soda, nitrate of potash, and carbonate of ammonia into the blood of dogs; in some instances removing the kidneys previous to injecting the substances. He did not detect ammonia in the breath of any of the animals operated on with *urea* by injection. He inclines to pronounce its presence in Frerichs's cases purely accidental. The animals from whom the kidneys were removed all died, after strong convulsions; and Dr. H. infers an analogy between animals deprived of the kidneys and patients affected with Bright's disease. Such analogy, it is true, may be predicated; but many attendant circumstances attaching to the cases of persons with diseased kidneys do not, of course, affect animals *without* kidneys, and consequently the analogy is not perfect, and hardly as safe to reason from as even the proverbially insecure foundation derived from analogical reasoning generally. Dr. Hammond does not find that urea or carbonate of ammonia, injected into the bloodvessels of sound animals, causes death; if they have suffered extirpation of the kidneys, such injection proves fatal. He does not discover from his experiments that urea, introduced directly into the circulation, becomes converted into carbonate of ammonia.¹ The experiments, at all events, go to prove the deleterious agency of urea, or of the product of its decomposition, when not promptly excreted from the blood, whether it be due, as in the case of the animals experimented upon, to loss of the kidneys, or, as in certain conditions in the human subject, to its retention in the blood by diseased, perverted, or obstructed action of those organs. While the question as to the exact material acting poisonously is still in abeyance, the *facts* relative to urea retained in the blood as productive of various "morbid effects," are indisputable, and as such we shall now proceed to examine them.²

GENERAL PHENOMENA REFERABLE TO THE PRESENCE OF UREA IN THE BLOOD.—From the fact that Bright's disease is the affection in which urea is most frequently retained in the blood, it will be all the more necessary not to refer any of its concomitant phases to the action of urea solely; although it is very plausible, and some of the best medical observers of the present day are beginning to teach, that many of the so-called *sequelæ* of Bright's disease may legitimately be referred to the presence of urea in the circulation. Thus, Dr. Watson remarks the extreme readiness of various organs of the body to become inflamed during an attack of Bright's disease. Especially is this found to be true, as all observers will testify, in reference to the serous and mucous membranes. Dr. Watson himself calls attention to this fact, and cites Drs. Bright, Christison, and Gregory to the same effect. He mentions, also, that M. Solon does not, in his volume on albuminuria, consider this tendency especially prominent in France.

¹ North American Medico-Chirurgical Review, March, 1858.

² See Appendix, note A.

Bronchial, pleural, pericardial, peritoneal, gastric, and intestinal inflammation are well-known and common *sequelæ* of Bright's disease, and occur, as to frequency, very nearly in the order above named. Now, it is very plausible to suppose that the abnormal condition of the blood, caused by the presence of urea, may be productive of many of these manifestations. Dr. Watson, while suggesting this, speaks particularly of disorder of the stomach and bowels, which so often follows or is concomitant of Bright's disease, and considers it may be explained by the action of "the poisonous material retained in the blood, and seeking a vent through supplementary channels of excretion."¹ He then refers, as corroborative proof, to the *post-mortem* appearances observed in these cases; "most commonly evident traces of disease are met with in various organs" besides the kidneys. This distinguished observer adds, that these manifestations "prevail with irregular frequency in different places. They are probably determined, in some measure, by local and peculiar agencies. Thus, vomiting and diarrhœa have been more familiar to the Edinburgh observers, than in London to Dr. Bright, or in Paris to M. Solon; while the headaches and coma so often witnessed by the British physicians have been comparatively uncommon in France."²

Although, in abnormal retention of urea in the blood, the vital tissues and fluids are all more or less affected, and, by a concomitant disturbance of the watery as well as of the solid portions, dropsical effusion may arise, yet the spinal cord and brain are the organs chiefly affected whenever urea becomes a blood-poison; and we have seen that the instances where it is not thus morbidly efficient, when retained in the blood, are exceptional. Generally speaking, when the amount of urea thus traversing the system is considerable, its effects are decided and rapidly disastrous. The affection is, properly, a cachectic condition; in other words, the system is, throughout, evidently depressed by a poison.³ Were it not, moreover, that the disease of the cerebro-spinal system, consequent on uræmia, is usually so severe, persistent, and fatal, the blood would become very seriously altered in its constituent parts, and finally devitalized, by the retention of urea; and, in addition, the whole play of the vascular system would be disturbed. The large quantities of albumen often eliminated during the retention of unusual amounts of urea in the blood cannot be said to be referable to the presence of urea alone. Albuminuria and uræmia may coexist, but the union is not a necessary one; neither is directly causative of the other. This fact we find pertinently referred to by Prof. G. S. Bedford, in his short but instructive chapter on uræmia.⁴

If we were to investigate thoroughly the disturbed chemistry and proportions of the blood which might properly be imputed to retention therein of the solid constituents of the urine, the limits of an essay, such as the re-

¹ M. Claude Bernard (*Leçons sur les Propriétés Physiologiques et les Alterations Pathologiques des Liquides de l'Organisme*. Paris: J. B. Baillière, 1859) remarks that in renal disease, when the urine is suppressed, intestinal disorder supervenes. The bowels have taken up, so far as they can, the elimination of urea. The rule is, that there is an elective affinity manifested by certain glands in the elimination of certain products from the blood. When anything interferes with their action, others fulfil their office to the best of their ability.

² Lectures on the Principles and Practice of Physic, fourth edition, London, 1857, vol. ii. p. 682.

³ Notwithstanding that many of the cerebro-spinal phenomena observed are those of irritation or excitation.

⁴ Clinical Lectures on the Diseases of Women and Children.

quirements of the question now proposed seem to demand, would be soon attained, and, by the addition of the practical details, largely exceeded. We have interpreted the terms of the subject as indicating a desire for an exposition of the phenomena of disease believed to be legitimately referable to the abnormal retention of the solid urinary constituents; and whilst endeavouring to present these, most of the results of the disturbed proportions and composition of the blood will, in fact, be made evident. And, in concluding these general remarks, it is well to say that accomplished observers, some years since, have been inclined to ascribe many manifestations of disease of slight intensity, and previously obscure and very imperfectly understood, to the presence of abnormal quantities of urea in the blood.¹

I. CEREBRO-SPINAL PHENOMENA ATTRIBUTABLE TO UREA RETAINED IN THE BLOOD.—(A.) A drowsy condition is often the first distinctly declared manifestation of toxæmia by the presence of urea or of the product of its decomposition in the blood. There are sometimes premonitory symptoms of less clear significance, but not always. Some writers, however, class them among the recognized *prodromata*.² This drowsy state ordinarily deepens into stupor and true coma, if it be impossible to relieve the blood of the offending element. The re-establishment of free diuresis, or a tendency to recovery from renal disease, may effect so desirable a result; but, unfortunately, the tide too frequently sets in the opposite direction. Coma may prove the final phase of the affection, and be simple or unaccompanied by disordered motility; or it may be combined with convulsions, and life may be terminated even more rapidly than if the complication had not existed.

(B.) Convulsions of an epileptic form may be the sole manifestation; there being no sopor, and consciousness being intact. The state is, in every respect, fully as unpromising as either of the two just indicated.

Among the first symptoms of uræmia may be mentioned—œdema in various parts of the body;³ lowness of spirits, amounting at times to me-

¹ Urea is always present in healthy blood, but in very small proportion. It may sometimes even not be readily recognized. Morbid effects, consequently, depend upon its presence in large quantities, and upon its *accumulation*, in the blood. The abnormal increase, therefore, even if small, will exert an influence. For valuable chemical and physiological information on this point, see the works of Simon, Thudichum, and Carpenter.

² These will be specified hereafter; they are of a character less arrestive of attention than the others; we will only mention—confusion of ideas, failure of memory, unusual sluggishness, general *malaise*.

³ This peculiar feature of the affection deserves especial notice. Its seats are chiefly the upper part of the body, the face, and the extremities—both upper and lower. The *labia majora* not infrequently exhibit it. Change of the patient's position often causes its disappearance, temporarily; and it frequently becomes less marked, or even vanishes, towards the end of pregnancy, even while the albumen of the urine and the structural disease of the kidneys is increasing. (Braun.) "The skin of the non-œdematous parts of the body appears very dry, and as white as chalk (chlorotic, hydræmic, leukæmic), and has a low temperature. Only those œdemata of pregnant women which exist contemporaneously with albumen, fibrin cylinders, and fatty degenerated scales of Bellini's epithelium in the urine, have a connection with uræmic eclampsia. The œdema of the lower extremities, ascites, and hydramnios, which are not complicated with albuminous urine containing fibrin cylinders, are not followed by uræmic eclampsia in pregnancy and labour. The affection of the kidneys with disease cannot certainly be inferred from the appearance of dropsy, as distinct causes may, at the same time, or one after the other, produce dropsies." (Braun, *Uræmic Eclampsia*; Duncan, p. 17.)

lancholia; restlessness; dizziness; headache; fretfulness; partial anæsthesia and delirium. Nausea, retching, vomiting, and rigors, are likewise noticed. There are often, also, impaired vision, amblyopia, muscæ volitantes, and amaurosis. These latter symptoms, as indeed all the others, are especially mentioned by Dr. Braun, of Vienna, in his late work on Midwifery; a chapter from which, devoted to Uræmic Eclampsia, has been ably translated by Dr. Duncan, of Edinburgh, and has furnished us with a great deal of new and valuable information. While Dr. Braun's views with regard to the subject of uræmic eclampsia will, doubtless, not be at present received as a whole by the profession, and are, indeed, questioned in many points by his translator, yet his extensive research, accurate observation, admirable description, and ingenious reasoning, render the chapter to which we refer at once entertaining, instructive, and full of practical suggestions. We acknowledge our great indebtedness to the author and his translator.

When stupor and coma finally supervene, a greater or less degree of apoplectic stertor accompanies the respiration. It is uniformly noticed that this stertor has a peculiarly *high tone*—a sort of shrillness, distinguishing it from ordinary apoplectic snoring. Reference was first made to this fact by Addison (*Guy's Hospital Reports*, 1839, No. VI.), and is repeated by Reynolds (*Diagnosis of Diseases of the Brain, &c.*, London, 1855) and by Rees (*On Diseases of the Kidney*, 1850).¹

The action of the poisonous agent in uræmia is believed by the best authorities (Tyler Smith, Braun, Reynolds, Churchill, *et al.*) to be first directed to the spinal marrow; and hence the sensitive impressions which make themselves morbidly apparent, as dizziness, headache, and subsequently convulsive movements. We are not to inquire into the *modus operandi* or etiology of uræmia, or of the other diseased conditions supervening in the human subject, upon retention of the elements of the urinary secretion in the blood; but—as we understand the question—to state the “morbid effects” only; consequently, we shall not occupy time and space by setting forth the received views and theories as to the direct or indirect modes of transmission of the deleterious influences, but will endeavour to state succinctly the disordered vital phenomena observed, and the pathological appearances, if any, which are noted *post mortem*.

¹ Dr. Reynolds, commenting upon this characteristic, writes thus: “The stertor exhibits a peculiarity first noticed by Dr. Addison. It is not of low, guttural tone, but of much higher pitch, and appears to be caused by the mouth rather than the throat, either by some position of the tongue against the roof of the mouth or teeth, or by some movement of the arches of the palate, not like that causing ordinary stertor, from which (although its mechanism is obscure) it presents the most obvious difference. (In several obscure cases—*i. e.*, obscure from the fact of the patient's not having come under notice until cerebral symptoms had appeared and consciousness was so far lost that no commemorative history could be obtained, and in which no œdema of the ankles was perceptible—this peculiarity of the respiratory stertor has at once awakened my suspicions; has led to an examination of the urine and the breath, and to the discovery in the former of albumen and fibrinous casts, and in the latter of an undue quantity of ammonia.”) (*Op. cit.*, p. 110.) The latter fact is significant, in view of the doctrine of Frerichs as to the agent which proves poisonous in uræmia; and the experiments and observations of many others go far to confirm the opinion.

Dr. Reynolds adds: “The peculiar muscular condition causing this stertor, I am disposed to consider as the result of spasm rather than paralysis, and the spasmodic contraction may be either of sensori-motor, simply reflex, or tonic origin, forming only one of many phenomena which indicate excessive or perverted conditions of those groups of motor action. This hypothesis is, of course, as unimportant as the fact of the difference is valuable.”

Uræmic symptoms may, of course, arise in both sexes from renal disease, or from mechanical obstruction to the excretion of the urine, as in hydro-nephrosis, retroversion of the uterus, urethral stricture, and closure of the ureters; which latter, if dependent on an unrelievable cause, must soon prove fatal. The occurrence of what has been termed "uræmic eclampsia" has been witnessed in non-pregnant females, and in males, and so cannot be considered as invariably belonging to the parturient state when urinæmia exists. The portion of Dr. Braun's work already cited, is devoted to the exposition of his belief that the convulsions observed during pregnancy are almost exclusively dependent on urinæmia.

After the appearance of the premonitory and of the earlier symptoms of uræmia, the progress of the mischief will, of course, be variable in different patients, and also according to the amount and *cause* of the retention of urea. Thus, it would seem natural that a large amount of urea being somewhat *suddenly* thrown into the circulation, and kept there by the continuance of the cause, should prove rapidly disastrous, and be accompanied with marked and violent phenomena. When gradually introduced, as in the slower advances of renal disease, or by the action of a progressive obstruction, the system may become somewhat accustomed to the presence of the deleterious agent. May not this be, in some degree, the explanation of the innocuousness of those very considerable amounts of urea the presence of which in the blood, and for a prolonged period, was ascertained by such accurate observers as Bright, Christison, Frerichs, and Rees; in conjunction, as we have previously intimated, with a possible greater power of resistance to the urea-poison in some constitutions than in others?¹ If this explanation be not in any degree admitted, the only alternative seems to be to accept the theory of Frerichs, that carbonate of ammonia is the toxic agent. In support of this view, we have the experiments upon animals, already referred to, in part, where extirpation of the kidneys was practised—as by Prevost and Dumas, Segalas, Tiedemann, Gmelin, Mitscherlich, Claude Bernard, Barreswil, Stannius, and Frerichs, all cited by Dr. Bedford to prove this point (*op. cit.*); and the test by injection tried by Bichat, Courten, Gaspard, Vauquelin, Segalas, Stannius, Frerichs; both methods without inducing convulsions. (*Idem.*) Dr. Bedford also mentions the significant fact that Vauquelin and Segalas proposed to give urea as a diuretic, so little did they consider it a poison! It is, under the present aspect of the subject, as well not to try the experiment.

Orfila—to come to direct experiments—caused fatal convulsions in an animal by the administration of carbonate of ammonia; and Bernard and Barreswil found carbonate of ammonia in the stomach and intestines of animals after extirpation of the kidneys.

Dr. Rees's idea that a peculiar "tenuity" of the blood may be requisite, in order to have full toxæmic action, when urea is retained, is certainly plausible; for we may at least suppose that the poisonous matter will be more readily and abundantly distributed through the circulating medium, and will consequently more thoroughly pervade and act upon the system.² And

¹ Frerichs states that the presence of the as yet unknown *ferment* in the blood is necessary, in order to the production of toxæmic symptoms by generation of the carbonate of ammonia. He thus explains the toleration of so much urea in certain cases.

² Dr. Todd ("Lumleian Lectures on Delirium and Coma," *Med. Gaz.*, 1850) also favours this idea. He believes the poisonous action of urea is facilitated by impoverished blood.

here we cannot refrain from adducing the exceedingly acute and ingenious remarks made upon this point by Prof. Simpson, of Edinburgh (*Obstetric Works*, vol. i. p. 371, American edition), in the article containing his statements in reference to puerperal convulsions, which latter, as we have already mentioned, recent observers have distinctly referred, in a large majority of cases, to toxæmia by the retention of urea, or of the product of its decomposition, in the circulation. In this particular connection, however, the patients were children—so that here we have remarkable instances of direct uræmic poisoning in connection with albuminuria—convulsions being the prominent symptom. The account of the first case we transcribe entire, together with a foot-note of much interest.

“A few weeks ago, I saw an instance in which convulsions in a child after birth were connected with the presence of albuminuria in its urine; or connected, as it should be, perhaps, more correctly stated, with that condition of the blood-poisoning or uræmia which is the result of albuminuria—whether that condition consists in a morbid accumulation of urea, or is produced, as Frerichs supposes, by the presence of carbonate of ammonia in the blood, produced by decomposition of the urea, or is, as is more probable, the effect of some other morbid agent in the circulating system, capable, like strychnia, of increasing the centric irritability or polarity of the spinal system to such an excessive degree that, under this super-excitability, comparatively slight eccentric causes of irritation in the stomach, intestines, uterus, bladder, &c. &c., readily induce convulsive attacks of a general form, like those of puerperal eclampsia.” (*Loc. cit.*)

In the foot-note appended to the above passage, and in reference particularly to the theory of Frerichs, Professor Simpson makes the following important and interesting suggestions:—

“If the blood-poison, which in albuminuria produces convulsions and coma, be, as Frerichs believes, carbonate of ammonia, resulting from decomposition of urea, can we account for the power of chloroform in restraining and arresting, as it does, puerperal convulsions, upon the ground of its preventing this decomposition? The inhalation of chloroform produces, as various chemists have shown, a temporary diabetes; sugar appears in the urine, and hence, probably, also in the blood. The addition of a little sugar to urine *out of the body*, prevents, for a time, the common decomposition of its urea into carbonate of ammonia.”

After mentioning the death of another child from convulsions supervening on the third day after birth—the mother having had puerperal convulsions and recovered—Dr. Simpson states that Dr. Weir, of Edinburgh, and himself found the urine of the child, like that of the mother, highly albuminous. He also says he is unaware of any reported observation of the coexistence of albuminuria and infantile convulsions; and then hints at the possibility that the albuminuria may be common as a pathological condition in certain forms of the convulsions of infants—as in trismus nascentium. Other infantile diseases, he thinks, may be powerfully influenced by albuminuria—as, for instance, sclerema, the “*endurcissement ou l’œdème du tissu cellulaire*” of French writers. Dr. Simpson had only seen two cases of this in Edinburgh, but was led, at the time of observing them, to believe and “to suggest that the skin-bound disease itself, or at least some forms of sclerema, may be a variety or effect of Bright’s disease in early infancy; the effusion into the cellular tissue, which constitutes the marked feature of the affection, being so far analogous to the anasarca occurring with albuminous nephritis.”

In reference to the use of chloroform, and the explanation which Dr. Simpson attaches to its mode of action in overcoming puerperal or uræmic

convulsions, may we not ask whether the subduing power of the anæsthetic agent, acting as it does upon the cerebro-spinal system, directly, is not sufficient, of itself, to explain the control of the convulsive manifestations, without a resort to the exceedingly ingenious suggestion of Professor Simpson as to the chemical explanation of the result?

We may here remark that, in one case, Dr. Duncan, of Edinburgh, found that the inhalation of chloroform aggravated the stertor and lividity of countenance observed in a case of puerperal convulsions ("uræmic eclampsia" of Braun and others). We observe that the chloroform was administered in "small quantity"—perhaps Dr. Simpson might say the amount was not sufficiently large.

In a valuable note to a portion of the chapter of Dr. Braun's work which he has translated, Dr. Duncan has virtually enunciated the same opinions as Dr. Simpson's, previously cited—both in reference to increase of the nervous irritability acting on various organs, and to the analogy of action to be predicated from the experiments instituted by zealous students of these phenomena upon animals. We append his comprehensive and apposite remarks:—

"In uræmia, the most important point is the circulation of a morbid fluid in the nervous system, which probably does not act as a direct excitant of the convulsive motions, so much as it increases the irritability of the nerves, and the consequent liability to convulsions from exciting causes, which, under other circumstances, would produce no noticeable disturbance. Ingenious experiments have, as is well known, been performed on frogs, which seem to demonstrate an analogous condition to exist under poisoning by strychnia, at least when moderate quantities of the poison are administered." (*Loc. cit.*, pp. 59, 60.)

When the peculiar conditions to which the retention of urea in the blood is due, can be relieved and removed, we may witness rallying, and final recovery, even from very unpromising states. Persistence of the cause, however, by maintaining the presence and increasing the amount of the poison, soon induces the gravest accidents, and must terminate fatally, sooner or later, according to the violence of the attack, and the power of resistance manifested by the patient. It will serve at once the purpose of illustrating this fact, and of furnishing a synoptical view of the effects of urinæmia, to recapitulate and condense the phenomena observed under the established morbid conditions of the affection.

1. *External Appearance of the Patient. (Early Stage).*—Aspect, that of general feebleness; and, if the depraved state of the blood follow scarlatina or Bright's disease, a more marked pallor, than when other causes are operative in retaining the urea in the blood—together with a puffiness about the cheeks and eyelids. Generally, sallowness, and anæmic hue, but sometimes blueness and congested appearance of the skin. More or less œdema of the extremities. Listless, confused, semi-idiotic manner.

(*Second Stage*).—Appearance that of a person apoplectically somnolent; degree, partial or complete; *modification*, by clonic contractions of the muscles.

(*Third Stage*).—Appearance that of one suffering from epileptic convulsions.

Either of the last two stages may be present singly, the other not occurring; or they may be combined and alternate.

2. *Disturbed Sensorial Manifestations. Early Stage.* ("Premonitory" of certain writers).—Impaired vision; transient, partial and incomplete

amaurosis (Reynolds, *op. cit.*); muscæ volitantes; tinnitus aurium; temporary deafness.

(*Later Stages*).—Deficient, and sometimes entire loss of sensibility; complete amaurosis; permanent deafness—the latter less common. Sensation is seriously impaired, but not very frequently wholly lost. Distinct cognizance of impressions not taken; but usual appreciation of injury to the corporeal surface, felt. (Reynolds, *et al.*)

3. *Motorial Manifestations*. (*Early Stage*).—More or less severe clonic contractions of the muscles; heavy and unwilling motions; slight stertor, “even when the patient is awake.” (Reynolds.)

(*Later Stages*).—Voluntary movements mainly absent; sometimes to be provoked by excitation; continuance of clonic spasms; epileptic convulsions, more or less strongly marked. Dr. Reynolds (*op. cit.*) remarks that the rigidity of the muscles observed during this period varies greatly, being sometimes excessive, “and much increased by movement of the limb.”

4. *Mental Condition*. (*Early Stage*).—Listlessness; fretfulness; uneasiness; confusion of ideas, impairment of memory, or its entire loss; partial or complete, but light, delirium—noticed often during sleep, or “when falling asleep.” (Reynolds, Braun, *et al.*)

(*Later Stages*).—The profound insensibility of true coma, but at first capable of dispersion—the patient can, by persistent efforts, be aroused; soon, merging of this state into that of complete and irrecoverable carus. Frerichs notices the fact that the usually mild delirium which may, but does not uniformly, attend this state, is characterized by reiteration of the same word for a long time. A maniacal state may follow the coma, when that disappears.¹

The species of coma first referred to—whilst the patient can yet be aroused—very much resembles that arising from opium, or other narcotic poisons, acting with full force. Dr. Reynolds, referring to this fact (*op. cit.*, p. 109), says he has noticed this sort of coma in the great majority of urinæmic cases he has observed. He writes:—

“The urinous element (whatever it may be) in the blood acts probably in a somewhat similar manner [*i. e.*, to that in which narcotics act]. There is not, however, in all cases of urinæmia, the notably contracted pupil that is observed in poisoning by opium. It is interesting to observe that the sensori-motor system appears to resemble, in its pathologic conditions, the spinal (or reflective) centre, rather than the cerebral (or intellectual). It is in a state of exalted rather than depressed activity, although both sensation and motion are severed from their purely cerebral relations (*i. e.*, from forming parts of perceptible and effective volition). There are several poisons which appear to act in a directly opposite manner upon cerebrum and spine (inducing at the same time coma and convulsions), but whether they contain different elements, whose action is thus separated, as Dr. Walshe once suggested, in a clinical lecture, the poison of urinæmia might be, I leave for future researches to decide.”

5. *Special Functional and Organic Manifestations*.—In addition to the external appearances of the patient, as exhibiting deranged function of the skin, and perverted nutrition, the stomach and bowels may become excessively irritable. The vomited and other excreted matters, we are told by several observers, exhale ammonia when tested by hydrochloric acid; and the air expired from the lungs sometimes reacts similarly under the

¹ See foot-note, page 34.—“Some cases of puerperal mania, accompanied by albuminuria, and where no eclamptic attacks had occurred, are alluded to by Dr. Simpson.”—Duncan (note to Braun, p. 136).

same agency (Frerichs, Johnson, Litzmann, Braun, *et al.*). The *pulse*, in the comatose state somewhat slow, rises, and is, at the same time, weak and irritable, in the convulsive periods.¹

6. *State of the Urine*.—Confirmatory of the existence of obstruction to elimination and excretion. Depuratory processes at fault. The secretion is generally *acid* in reaction to tests, and *albuminous*—although cases of *urinæmia* occur in which albuminuria is not an element—casts of the *tubuli uriniferi*, and also blood-corpuscles and mucus-corpuscles are discovered by the microscope; and the urea is notably diminished in the specimens of urine passed. (Frerichs, Thudichum, Rees, Braun, Reynolds.)

A febrile condition, very similar to that of genuine typhus, is observed; and especially in connection with diminished excretion of the urine, or with its entire suppression. This is denominated by Frerichs, *febris urinosa*; the French writers designate it by the same term—"fièvre urinaire." There is delirium, excessive prostration, and a urinous odour pervading the excretions; and death is then imminent. Death may, in certain cases where the blood has been exceedingly impoverished and contaminated, follow epileptiform convulsions which are due simply to the deteriorated and devitalized blood. These convulsions should be distinguished from those arising from other causes. Sometimes, even in such cases, rupture of cerebral vessels may cause apoplectic coma, by effusion of blood.²

Should life be prolonged, and in cases where recovery is possible, and occurs, there may remain permanent injury to the general health; or special organs may be particularly acted upon. Hemiplegia, hemeropia, amaurosis, and impaired mental vigour may be mentioned. Œdema of the lungs and serous effusion into the cavities are noticed—the cerebral ventricles, even, being invaded by an urea-bearing serum. Life is too frequently destroyed, however, to enable observers to enumerate many cases and facts bearing upon this portion of our subject.³

II. POST-MORTEM APPEARANCES.—It is universally conceded that very few structural changes, of consequence, and often *none whatever*, are found on necroscopic examination—after death from the mere action of uræmic poisoning—in the cerebro-spinal system. The apoplectic effusions and lacerations of the cerebral substance are only indirectly, if at all, referable to uræmia. The condition of this system is that which chiefly concerns us at this time; for we are seeking strictly for the effects, both vital and *post-mortem*, legitimately due to the toxic agent derived from the presence of urea in the blood. A full description, therefore, of what is found after death in the *kidneys*, does not seem to us pertinent to the question; for the usual and well-known renal lesions of Bright's disease are not the product, but simply the frequent cause, of the uræmic condition. We shall, therefore, endeavour to particularize the appearances presented after death by those organs which during life most strongly manifest the effects of the toxic agent, and with which it is most intimately brought into con-

¹ It may be very much accelerated, and sometimes remains frequent throughout the affection.

² Drs. Watson, Todd, and George Johnson, have called attention pointedly to the fact of epileptiform convulsions springing from the circulation of impoverished blood in the cerebral vessels.

³ The *mania* which sometimes follows the comatose state in uræmic eclampsia, is generally well recovered from; but should not be confounded with that symptomatic of puerperal pyæmia.—Braun; who refers to Helm, Litzmann, "and others."

tact and relation—mentioning more succinctly such other concomitant lesions as have been observed. And in noting the necroscopic manifestations in the cerebro-spinal system, we shall speak of such cases as have manifested the gravest indications of disturbance of the nervous centres during life, and especially decided coma and convulsions.

Brain.—Anæmic appearances, and a somewhat infiltrated condition, are noted; the consistence being most frequently diminished.¹ This state occurs irrespective of any abstraction of blood during life. The membranes of the brain are not commonly congested or “hyperæmic.” Dr. Braun says, also, that inter-meningeal apoplexy is even more rarely observed than hyperæmia. He adds, that Helm and Kiwisch² very justly consider inter-meningeal apoplexy “as a secondary phenomenon produced by impeded circulation of the blood;” while it is looked upon by Litzmann as “a result of the uræmia.”

Spinal Cord.—Examinations of the cord have been but infrequently made. (Romberg knew of *none* at the time of writing his *Treatise on Nervous Diseases*, the first edition of which was published in 1840 and the second in 1851.)

Braun states that Bluff, at one examination of the spinal cavity, “found much serum in it.” Dr. Duncan reminds us, in this connection, that—as Dr. Christison first showed—the serum discovered in different regions of the body often has urea in it.

Dr. Todd, speaking of the condition of the cerebrum and spinal cord, after death following renal disease accompanied by coma and convulsions, says:—

“The organic disturbance of the brain which accompanies and causes the comatose tendency, is, as I have already remarked, much less than the pulmonary affection. There we find nothing which the most zealous morbid anatomist could call inflammation; and, except the patient may have died in convulsions, we do not even find congestion—that most fertile of causes with a

¹ Romberg (*Nervous Diseases of Man*, London, 1858, Syd. Soc. Ed.) when mentioning the results of cadaveric inspections in cases of *eclampsia parturientium*—which affection, it should be borne in mind, is now referred by such high authority to uræmic intoxication—says—rather adversely to what we find recorded by others—that in the cranial cavity we generally find considerable congestion, increased density of the cerebral tissue, plastic and sanguineous extravasations between the membranes, and in the ventricles, in the latter chiefly when apoplectic symptoms, a profound sopor, stertorous breathing, &c., have been associated with the convulsive affection.” (*Loc. cit.*) The points in which this account chiefly differs from that given by Dr. Braun, are the frequency of congestion, and the increase of the density of the cerebral substance, which the latter observer distinctly denies, as indeed do others. Romberg, whose whole description of the epileptiform convulsions of the parturient female is admirable, clear, and vivid, refers on the above points to Hauck (*Einiges aus dem Gebiete der praktischen Geburtshülfe*; in Casper's *Wochenschrift der gesammten Heilkunde*, 1833, vol. i. p. 133; and Velpeau, *Die Convulsionen der Schwangerschaft während und nach der Entbindung*. Uebersetzt von Bluff: Köln, 1835, p. 86). Romberg, as well as other authors, refers to the paucity of necroscopic facts connected with eclampsia puerperalis; and especially with regard to the spinal cord.* This is, even at the present day, true; but the latest observations, coming as they do from reliable sources, must be allowed the greatest weight. With regard to the appearances in the *brain*, then, a certain difference of statement exists between Romberg and other authors we have consulted. As to the *spinal cavity and the cord*, there are no new facts within our cognizance.

² Helm, Th., *Med. Jahrbücher*. Wein, 1839, bd. xx. s. 202; Kiwisch, *Beiträge z. Geburtsk.* Würzburg, 1846.—Braun, *op. cit.*, chap. VI.

* Never had been examined when Romberg wrote; but has been since.

school of pathologists which is, I hope, fast disappearing. Indeed the brain is generally anæmic," &c. * * * (*Op. cit.*)

Dr. Simpson has reported (*Obstetric Works*, vol. i. p. 732) some cases of "puerperal convulsions connected with inflammation of the kidney," wherein effusion of blood and serum into the ventricles was discovered, with destruction of the right corpus striatum and outer portion of the optic thalamus, in one patient; together with encysted and degenerated kidney (*morbis Brightii*), and purulent-like matter, with adherent lymph, or false membrane, in another. In a third case, purulent-looking matter could be pressed out from the renal papillæ; no effused fibrin or coagulable lymph was discovered. The microscope did not decide the "whitish turbid fluid" to be pus—only epithelial cells, in large quantity, were found. The last patient had repeated convulsions, and died comatose, but no mention is made of any cerebro-spinal lesions being observed. The effusion of blood, and the laceration of cerebral substance in the first case related by Dr. Simpson, were truly apoplectic, and not referable to the intrinsic action of urea contained in the blood; although doubtless the amount present therein must have been considerable, since Bright's disease existed.

Dr. Watson (*op. supra cit.*, vol. i. p. 493), speaking of the appearances observed after death from apoplectic coma, after having referred to such a result from the action of retained urea, uses the following language:—

"On examining the brain, we may find a large quantity of extravasated *blood* spread over its surface, or lying within its broken substance; or a considerable effusion of *serous fluid* collected within its ventricles; or we may detect *no* deviation whatever from the healthy structure and natural appearance of the organ. The congestive pressure (if, indeed, it existed) has left no prints of its action."

The following observations, from the *Manual of Pathological Anatomy*, by Drs. Jones and Sieveking, are exceedingly apposite in this connection:—

"The researches of Bright, Frerichs, and others have demonstrated the close relation of the state of the blood to cerebral disease; and science has shown, what, previously, was purely hypothetical, that the most fatal conditions may be thus induced without any palpable changes being wrought in the cerebral tissues. It does not, however, follow, that because we see no changes, none have taken place. The poison that we know to be in the blood may elude our chemical tests, and yet cause death. Then, seeing how limited our knowledge of the nervous system is, it is not to be wondered at that, although the manifestation of altered function is so great as to force the belief in its altered constitution, it is not in our power to prove the latter to the perception; but, as Dr. Watson remarks, 'whatever may be the nature of the unknown, and, perhaps, fugitive physical conditions of the nervous centres, thus capable of disturbing, or abolishing their functions, it is useful to keep in our minds a distinct and clear conception of the fact, that there must be some such physical conditions.'"

Mere uræmia, therefore—presuming the occurrence of convulsions and coma—it would appear, leaves the brain anæmic in appearance, and possibly somewhat softened (more dense, according to Romberg; refer to page 35); the more decided and destructive appearances are owing to rupture of vessels, and consequent extravasation, with its consequences, and to dilatation of the ventricles with serum.

Lungs.—These organs are constantly found in an œdematous condition, and sometimes emphysematous. Dr. Braun, recording the fact that emphysema was long since observed by Böer, says that it is now considered

"as always the secondary result of the fits"—i. e., uræmic convulsions. *Op. cit.*, p. 62.

Heart.—This organ is reported to be usually "empty and flaccid." (*Braun.*) We may thence infer feebleness of circulation, and impairment of its own tonicity, and of that of the bloodvessels, by reason of the impoverished state of the blood, their natural stimulant.

Kidneys.—Generally, and according to some authors, always, the kidneys exhibit more or less extensive and advanced signs of Bright's disease. In cases where the retention of urea in the blood has been caused by some other agency—such as obstruction, etc., there would naturally be traces of congestion, and perhaps of inflammation, although not uniformly; a nearly natural state might well enough exist.

As we previously intimated, it does not seem necessary to give, in this place, an elaborate account of the changes wrought by granular and fatty degeneration of the kidney, in connection with uræmia; and for reasons already stated. Moreover, these general appearances are well known, and abundantly set forth in many treatises. Those who would see, however, an admirable and somewhat condensed account of the changes of this nature effected in the renal tissue in urinæmic cases connected with pregnancy (*eclampsia puerperalis, seu gravidarum*), should consult the chapter of Dr. Braun's work, to which we have so frequently referred. A few extracts only will be made by us, and those chiefly to call attention to certain prominent points in the renal necroscopic phenomena.

Dr. Braun bases his descriptions on the three forms of Bright's disease proposed by Frerichs.

In the first stage, that of hyperæmia and commencing exudation, the surface of the kidney is smooth, the capsule is easily removed, the plexus of veins on the surface of the kidneys is dilated, and full of dark blood." (*Loc. cit.*, p. 62.)

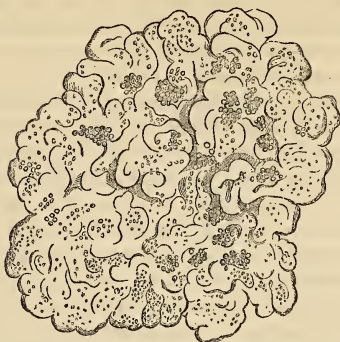
"The pyramidal masses [renal papillæ] are likewise hyperæmic, and their injection is striped. The mucous membrane of the pelves and infundibula is swollen, and covered with vascular arborescence; and they contain a bloody fluid. Apart from hyperæmia, the finer structures of the kidneys do not appear to be essentially injured. Hemorrhagic effusions are very frequently observed, which sometimes take their rise from the glomeruli; sometimes from the vascular plexus of the tubuli uriniferi, sometimes from the veins on the surface of the cortical substance." (p. 63.) In the first stage, the epithelial lining of the uriniferous tubes is stated not to be essentially altered; the tubes themselves are often filled with exuded blood—"fibrin-cylinders."

In the second, or exudative stage, fatty degeneration commences and progresses, the kidneys becoming large, and heavier than they are normally. The capsule of the kidney can be easily separated. The pyramidal masses are dark red. The infundibula have a dirty-red mucous surface. The glomeruli (vascular knots, Malpighian corpuscles), which may be drawn out with a curved pin, are covered with a fine granular matter, and partly with solitary or grouped fatty corpuscles, which, by the addition of acetic acid, become transparent.¹ Between the glomerulus and the capsule lies a thick stratum of firm exudation, of granular structure, and having fat droplets, and sometimes crystals of cholesterine.

¹ Wedl mentions that acetic acid rendered the fat-globules more distinct.

The contents of the epithelial cells of the tubuli uriniferi next become degenerated—they are “decomposed in aggregations of granules.”

Fig. 1.



Malpighian Corpuscle, from the kidney of a patient who died of *Eclampsia Parturientium*.—(From Wedl's Pathological Histology, p. 260.) “The surface is covered, partly with a fine granular substance, partly with solitary and aggregated fat-globules, which were not further changed by acetic acid or carbonate of soda. * *”—(*Loc. cit.*)

In the third stage, that of retrogradation, atrophy of the kidney is progressively induced. The pyramids of Malpighi and Ferrein are observed to be less atrophied than the cortical layer. At their bases, granulations occur between the straight tubuli, and press the latter apart.

The width of the renal pelvis is increased, and their lining mucous membrane swollen and covered with a network of “varicose vessels, of an uniform grayish-blue colour.”

“In those who die of uræmic eclampsia during pregnancy, atrophy of the kidneys is less frequently observed than the first two stages of Bright's disease.” (*Op. cit.*, p. 69.)

III. URÆMIA IN CONNECTION WITH PREGNANCY AND PARTURITION.—If we receive the opinions of certain modern pathologists as true, the retention of urea in the blood of the pregnant female is productive, both to herself and to the fœtus, of the most disastrous results. The views of Dr. Braun, already referred to, have been adopted by a large number of able and practical men in our profession; and, since the publication of his volume—and mainly, in Great Britain and this country, through Dr. Duncan's translation of the chapter on uræmic eclampsia—have elicited great attention, and will doubtless prove an incitement to yet more extended and close observation.

Whilst many have joined Dr. Braun in this new theory, there were at the time his work appeared, and probably still are, several distinguished names arrayed against it. These are all mentioned by the author himself, who also states the objections made by them, and gives what he considers the refutations thereof. Dr. Braun's belief, as declared in his treatise, is simply this: *that the convulsions caused by uræmic intoxication in acute Bright's disease, and puerperal eclampsia, are identical.* This proposition, as he tells us, has been energetically defended by Frerichs, Litzmann, Wiegner, Oppolzer, himself, and many others; but it has been assailed by Marchal, Siebert, Depaul, Legroux, L'Huillier, Stoltz, Seyfert, Levy, in very valuable articles, and also by Scanzoni. With much anxiety these observers have tried to prove that the Brightian degenerations of the kidneys, which, it cannot

be denied, are found in the bodies of those who have died of eclampsia, are consequences merely of the convulsions—only accidental, secondary phenomena of the hyperæmia caused by the eclampsia, and of hydræmia (*plethora serosa*).

Scanzoni, whose arguments against Dr. Braun's views are summed up and given by the latter author, refers the true *eclampsia parturientium* to an "irritability of the motor system of nerves which has been induced by pregnancy, and increased by the act of delivery."

The arguments *pro* and *con* have been actively carried on; and Scanzoni's conclusions were replied to by Wieger in June, 1854, and by Litzmann in 1855. In addition, the industry and zeal of Dr. Braun have enabled him to collect a truly imposing array of facts from *post-mortem* observations made by himself and other reliable practitioners, and in the majority of which abundant evidence of Brightian renal lesions existed. In some of the cases, where hyperæmia of the kidneys was found, microscopical examination was prevented by "accidental obstacles;" but the author does not consider this "any proof of the absence of Bright's renal exudation." Dr. Braun, while setting forth these investigations, says that Wedl' explains the non-discovery of fatty degeneration in the kidneys in several instances of death from eclampsia, "by the fact that in many cases a dissolution of the Malpighian bodies is effected by the fluid exudation, and hence in every diffuse inflammation of the kidneys an evident fat-metamorphosis of the contents of the Malpighian capsules does not ensue."²

Hasse, according to Dr. Braun, has never seen eclampsia puerperalis without Bright's disease. The necroscopic and microscopic observations of Wedl, Gustav Braun, Lumpe, Hecker, Devilliers, Regnauld, Simpson, Blot, Cahen, Wieger, Litzmann, Credè, Sabatier, and Hohl have contributed to establish the theory propounded in Dr. Braun's volume.

In reference to the *etiology* of "*eclampsia parturientium*" ("*uræmic eclampsia*" of Braun), Romberg³ only hints at the agency which Braun has distinctly announced as, in his opinion, the chief efficient cause. Thus, while the latter ably demonstrates his views as to the retention of urea, and the accompanying albuminuria, the former refers to "retention of urine owing to pressure of the gravid uterus upon the bladder;" and afterwards adds: "Future investigations must determine whether albuminuria, which often supervenes during pregnancy, may, when fully developed during the last months, possess any etiological influence." The "investigations" of Braun and others certainly seem, if not already to have determined this point, likely to lead shortly to a satisfactory settlement of the whole question.

Supposing, then, in conformity with the new doctrines thus announced, that the convulsive attacks and intervening coma observed in puerperal patients are owing to toxæmia by retained urea, let us examine the collateral results of such a condition—first, in regard to the mother; and next, as respects the fœtus. Having already detailed the general effects produced upon the system by uræmic intoxication, we may properly direct our notice, at present, to the influence exerted upon the *puerperal state*, and upon the *life* of the mother and that of the fœtus.

Referring to our previous enumeration of the results, both vital and

¹ Grundzüge der pathol. Histologie, Wien, 1854, S. 306.

² Loc. cit., pp. 74, 75.

³ A Manual of the Nervous Diseases of Man; Sydenham Society's edition, London, 1853, vol. ii. p. 189.

post-mortem, which are observed after genuine uræmic convulsions, we would only add thereto that the *uterus and its appendages* are generally found healthy after eclampsia puerperalis, or, at all events, do not necessarily deviate from their usual condition after labour has terminated; unless, of course, there has been some pre-existent or concomitant disease in, or alteration of, the organs. The infrequency of metritis and peritonitis is mentioned by some observers;¹ but Churchill² speaks of the great tendency to abdominal inflammation after the labour is over, and quotes Denman, who first mentioned this, and Collins and others, as confirming it. Braun also refers to the danger of puerperal affections coming on after eclampsia, especially if "an epidemic of zymotic diseases prevails."

The *spleen* is said to exhibit "the large dimensions it possesses in pregnancy and childbed."³

We now proceed to the consideration of the special influences exercised by uræmic intoxication during the puerperal state.

1. *Influence of Uræmic Eclampsia on the Duration of Pregnancy.*—Uræmic eclampsia is generally sudden in its accession, prompt in its results; and the testimony of those who have had the most experience in regard to it, is that it either causes death rapidly, or else that it is completely and with considerable celerity recovered from. Long consequent illness and sequelæ, *from the eclamptic state merely*, are not common. The true uræmic eclampsia occurs in the majority of cases in the latter part of the period of pregnancy. It may, also, only appear at the time of the labour itself; after the child is born, and before the after-birth has been delivered; or, finally, during child-bed. When supervening within the latter half of utero-gestation, premature labour is the result—as a rule.⁴

The latter two months of pregnancy are stated to be peculiarly obnoxious to the occurrence of convulsions. (Churchill, Romberg.) The *cause* of premature labour is either excitation of uterine contractions by the power of the abnormal action going on in the nervous centres,⁵ or it may be, partially at least, ascribed to the presence of a dead fœtus, whose destruction is referable to the eclamptic condition. Ramsbotham, we observe, does not think the latter occurrence a cause. Churchill queries whether it may not be such; and the supposition is at least plausible. Dr. Copland (*Dict. of Med.*) speaks of cases where "the child has been unexpectedly born during the violence of the convulsions, as if expelled by them with unwonted celerity." Again, he states that the worst forms of the attack are often attended by a firm, spasmodic constriction of the cervix uteri, preventing the expulsion of the fœtus.

2. *Influence of Uræmic Eclampsia upon the Life of the Mother.*—Although puerperal convulsions—we here use the term, let it be remembered, as synonymous with uræmic eclampsia—are comparatively a rare affection, yet they make up for the element of infrequency of occurrence, by violence of manifestation and an alarming ratio of fatality. As the liability

¹ Romberg, Velpeau, *et al.*

² Theory and Practice of Midwifery.

³ Braun, *op. cit.*, p. 62.

⁴ It is well known that Bright's disease, without any other influence, will cause premature labour; Braun says this is true in about 25 per cent. of cases. Add *convulsions*, and the danger is manifestly increased.

⁵ Blundell (*Principles and Practice of Midwifery*, Am. edit., p. 418), referring to this point, says: "Sooner or later, * * if the fit continue, parturition commences of itself, without the interference of the accoucheur; and * * a sudden emersion of the fœtus occurs." He also refers to the occurrence of delivery during convulsions, unknown to the attendants.

to an attack is greater towards the last of the period of utero-gestation, so, generally speaking, is the danger to the life, both of mother and child, at that time. Not only is this true if we merely refer the eclamptic condition to the increased sources of irritability to which the nervous system has become liable, by reason of the pregnant state and its advanced stage; but also it is easy to see that a poisoned blood is all the more likely to act with increased morbid force—especially upon fœtal life. Again, the danger from convulsions diminishes according as they approximate to the term of delivery; and we are told, by a most competent authority, that the fits diminish in force in 31 *per cent.* of the cases, cease entirely in 37 *per cent.*, and only continue of the same intensity in 32 *per cent.*, *after the uterus is evacuated.* (Braun.)

Dr. Braun refers to 15 deaths out of 45 cases, of which he has published accounts—being exactly *one-third*. He speaks of nine cases occurring in his practice within the last three years, all of which terminated in recovery. Thirty *per cent.* of the cases, it is estimated, prove fatal. Romberg gives a higher ratio of mortality. His statement is, that above one-half of the women attacked, died within twelve, twenty-four, or thirty-six hours. Churchill finds from his statistical investigations, that the proportion of fatal cases is above one-fourth. He intimates that there has been a tendency, of late, to diminution in the mortality-rate, which, at one time, he intimates, was very much larger. It has not been thought that much, if any difference as to fatality can be ascribed to early or late supervention of the affection; at all events, lateness of attack has not been allowed more weight, in this respect, by observers, when it is a first case. Some patients have several attacks in successive pregnancies, and finally die in one. A reiteration, therefore, of the accident, must be deemed of unfavourable augury. The concomitance of coma, with apoplectic stertor, and the approximation of the fits, so that they become, as is sometimes the case, almost continuous—and especially when these conditions obtain in plethoric and strongly constituted patients—are, almost without exception, fatal elements.

Recovery from the mere eclampsia may take place; and there may be some extensive dropsical effusion, some injury to the brain or spinal cord, or a permanent and increasing œdema of the lungs, disease of the heart, etc., which will compromise life at a later period; but, as a rule, if the patient escape the eclampsia, and its *immediate* results, recovery is usually not tardy, and, moreover, is complete. The occurrence of rupture of the uterus, in itself sufficiently grave at any time, we need scarcely say, very greatly aggravates the unfavourable prognosis attaching to eclampsia. Dr. Copland remarks (*Dict. of Practical Medicine*), that puerperal convulsions “should never be considered devoid of danger, more especially when they occur after delivery, or in consequence of great exhaustion of vital power, or of uterine hemorrhage. When they are slight, are unattended by stertorous breathing, or by paralytic or apoplectic symptoms, and when parturition is so far advanced as to readily admit of its completion by art, less danger may be feared.”

The balance of prognostic opinion, it will be seen, is against recovery; and Blundell speaks of *post-partum* convulsions as being the most dangerous.

3. *Influence of Eclampsia on the Life of the Fœtus.*—We have already intimated the danger arising from eclampsia parturientium to the life of the fœtus. Relatively, it is even greater than that threatening the mother.

In referring to this point, those who have had the largest experience, use such expressions as the following: "In almost all cases the child is still-born, often putrid." (Churchill, *op. cit.*) "The life of the fœtus is endangered so long as it is nourished by the uræmic blood of the mother." (Braun, *op. cit.*) "The infant almost invariably dies when the disease occurs during the last months of pregnancy; it may be saved when the eclampsia supervenes during parturition." (Romberg, *op. cit.*, p. 189.) "The infant is generally, though by no means universally, born dead, when the woman has been the subject of convulsive seizures, especially if the attack has occurred early in the labour, and continued for any length of time." (F. H. Ramsbotham, *Principles of Obstetric Medicine and Surgery.*) The latter author pointedly and happily refers to *toxæmic action* on the blood of the fœtus as the most likely cause of its death *in utero*; and mentions a case from Spence, where the child being removed alive by Cæsarean section from a mother just dead from convulsions, died itself, in convulsive paroxysms, in less than an hour. The latter statement leaves us to presume a poisoned condition of the blood. M. Ménard states, that, in the majority of cases of death by convulsions, previous to delivery, the child has been found dead; the contraction of the features and extremities denoting that it had participated in the affection of the mother. Dr. Copland, who notes this remark (*Dict. of Med.*), says that it "wants confirmation."

We thus see that the likelihood of the life of the fœtus being maintained after eclampsia has been declared in the mother, is extremely small, as might, indeed, be expected; and if the child be born alive, there is great probability that it may either not survive long, or that it will be more or less morbidly affected by the accidents occurring during its intra-uterine existence.

Hereditary transmission of eclampsia, uræmia, or Bright's disease of the kidney, to a suckling, says Dr. Braun, "has not yet been demonstrated, and only Simpson has found albuminuria in a suckling born of an eclamptic mother." Dr. Duncan, in a foot-note to the paragraph from part of which the last remark is taken, says that "if the uræmia persists in a nursing woman, urea may be present in the milk, as has been shown by several observers, and may disturb the health of the suckling."

URIC ACID.—(Symbol: \overline{U} . Formula: $C_5H_4N_2O_6 + HO$.)¹

Synonymes.—Lithic acid; Urylic acid.—PROUT, BIRD, *et al.*

This substance is the next constituent of the urine, in the order of enumeration we follow; and the effects of its retention in the blood will therefore now engage our attention.

It is recognized as a constant ingredient of healthy urine, and holds a very intimate relation to urea. (Thudichum, *op. cit.*) "It forms less than

¹ Thudichum.

Chemical Composition of Uric Acid.

THUDICHUM.				G. BIRD.	
Carbon	.	.	35.714	$C_{10}, N_4, H_4, O_6, C_2, H_4, N_2, O_2 + 2C_4, NO_2 = 168$	
Hydrogen	.	.	1.191		
Nitrogen	.	.	33.333		
Oxygen	.	.	19.048		
Water.	.	.	10.714		
<hr/>					
100.000					

$\frac{1}{2000}$ part of the urine in man ; but its proportion varies much in different animals." (Dr. George Johnson, *op. cit.*, p. 50.) In the blood, it is always found in union with an alkaline base ; and it is not soluble in that liquid. It appears in the blood in the form of urate of soda, or of urate of ammonia. (Liebig, Simon, Thudichum.) Dr. Thudichum states that the urates are always acid salts—*i. e.*, that there is excess of uric acid—and he has advanced some ingenious reasons for denying the accuracy of Dr. Golding Bird's theory and explanation of the secretion of uric acid in the form of urates. With this chemical discussion we have no concern ; the indication of the pathological states induced by retention of the acid in the blood, in the form above specified, being now our object. Before approaching the subject in detail, however, it does not seem particularly out of place to allude to a practical remark by Dr. Thudichum (*op. cit.*, p. 100) upon the deposition of the urates from the urine. Researches and observations in reference to this point, cannot fail to be of importance in the treatment of such cases. Dr. Thudichum says :—

"If the presence of a deposit of urates be taken as an indication of the saturation of urine by these salts, and if the latter be assumed ordinarily to be of the usual amount, deposits of that kind become more valuable as signs of a diminished secretion of water by the kidneys than of any other symptom. As the appearance of a deposit of urates is always accompanied by morbid sensations and objective symptoms—in the healthy by thirst at least, if by nothing more—the conclusion is simple enough. *The individual whose urine has deposited the urates does not drink water enough, and must drink more, and must drink so much that the urine, at the ordinary temperature of the air, shall remain clear.* Of course, in some cases this will be neither possible or advisable ; but in most cases of acute and febrile disease it should be a plan of treatment. I have certainly seen it attended by beneficial results in many cases ; I have also observed the contrary—want of water in the system—to be a source of disease."

GOUT.—Whenever, from failure to eliminate the uric acid from the blood, that substance accumulates therein, the abnormal effects of its presence do not long delay their manifestation.¹ It is well known that the ingestion of large quantities of highly-azotized food, and a rich diet generally, together with the free use of malt liquors and of acid wines—Madeira amongst others—is productive of an abnormal increase of uric acid in the system ; and, consequently, luxurious livers have long constituted the majority of sufferers from gout—a malady which abundantly declares its *fons et origo*, by the tendency to abundant deposition of the *urate of soda* in different parts of the body ; its seat of election being the smaller joints—as those of the toes and fingers. In the latter, the deposit is often very plentiful. We have seen not long since—and the occurrence may not be very uncommon—a patient who could write his name with his "chalk-stone" knuckles ; a woful example, truly, of diverted and arrested excretion !²

¹ Bernard recognizes the accumulation of uric acid in the blood, either by arrest of the renal function, causing its elimination to cease, or by an exaggerated production of it, as in gout. (*Liquides de l'Organisme.*)

² An instance of this is related by Dr. Watson (*op. cit.*), and Dr. Todd has alluded to the condition. After all, cases of this extreme nature may not be so common as we have intimated.

Dr. Garrod (*On the Treatment of Gout and Rheumatic Gout*) remarks upon this point : "Comparatively few gouty patients become the subjects of visible chalk-stones, at least to any extent, or such as to induce deformity ; at the same time, I am convinced that their occurrence in a slight degree is by no means so rare as has been generally assumed. Sir C. Scudamore stated, that in 500 cases of gout he only found them 45 times, or in less than 10 per cent. From my own expe-

As has been previously mentioned, Dr. Garrod, of London, first called attention, not only to the presence of uric acid in healthy human blood, but also pointed out the fact of its abnormal amount in connection with gout. He did not, however, then wish to be distinctly understood as declaring the acid the sole *materies morbi* in that disease, as may be seen by his remarks in a "Postscript" to his highly interesting paper, communicated to the Medico-Chirurgical Society, upon the subject. This important contribution to scientific medicine was read before the Society,¹ February 8, 1848, and the postscript just alluded to bears date July 26, 1848. (Vide *Medico-Chirurgical Transactions*, vol. xxxi.)

Daily observation tends to show that the relation of cause and effect may more and more safely be predicated in regard to the presence of an excess of uric acid in the blood and the phenomena of gout. Writers upon the subject, both near the time of Dr. Garrod's first researches, and later, have varied somewhat as to the completeness with which they have expressed themselves in respect to establishing uric acid as the active agency in gout. The majority of testimony seems to be affirmative. Dr. Watson (*Principles and Practice of Medicine*, 1848) seems to have then regarded the morbid agent as recognized. We find, indeed, in the edition of Dr. Carpenter's *Physiology* published in 1846, very positive language as to the conspicuousness of uric acid in gouty affections; he says: "When it [uric acid] is imperfectly eliminated, we are assured of its accumulation in the circulating fluid, by its deposition, in combination with soda, in the neighbourhood of the joints—forming gouty concretions, or chalk-stones." He thus appeared to recognize the cause of the diseased condition as lithic acid. There are those, however, who, even at the present day, speak with less distinctness as to an excess of uric acid being the sole and sufficient *materies morbi*. Thus, Dr. Barclay (*A Manual of Medical Diagnosis*, London, 1857) writes: "The researches of recent times have gradually led to the discovery of an important element in gout—the presence of an excess of uric acid in the blood. This knowledge holds out a prospect of our arriving ultimately at more accurate diagnosis; at present, it is only in the hands of a few that such a chemical test can be relied on." The opinion is a very guarded one—decidedly non-committal; we think more influence than it implies may safely be allowed to the "element" in question.

It will, at least, not be disputed that gout is a blood-disease. Amongst many other observers who might be cited on this point, we select Dr. George Johnson, as furnishing comprehensive testimony. Referring to gout as a cause of renal disease, he says: "It would be useless to occupy the time of my readers by lengthened arguments to prove that gout is a blood-disease, since all the phenomena of the disease clearly indicate such an origin, and can be explained on no other supposition." (*Op. cit.*, p. 78.) He then alludes to the intimate connection between gout and the uric acid diathesis. Thus, then, when such a diathesis prevails, or when, by some obstructing agency, the elements of the urinary secretion are retained and accumulate

rience, I consider these numbers far below the real proportion, being confident that their existence is frequently overlooked, as they are sometimes deposited in parts of the body scarcely to be expected." Dr. Garrod thinks that gouty concretions are more frequent upon the cartilages of the ear than anywhere else; contrary to what has usually been recorded. He refers to the *Medico-Chirurgical Transactions*, vol. xxxvii., 1845, pp. 74, 75.

¹ By Dr. C. J. B. Williams, upon some of whose patients Dr. Garrod's experiments were made.

in the blood, the gouty accidents, amongst others, prevail. If uric acid be prominent, the corresponding series of symptoms seems as sure to occur, as does that following the retention of urea when *that* substance is retained, in excess, in the circulation.¹ If the uric acid, therefore, is received as the true *materies morbi* in gout, we have, at once, the following easily-deduced sequence of morbid effects:—

First, deficient depuratory action; next, accumulation of uric acid in the blood; *dyscrasia*, chemical and physiological; deposition of urate of soda in various parts; the objective phenomena of gout; the *sequelæ* of gout—amongst others, as Dr. Johnson points out, renal disease. Under the light of modern pathology, we do not think the above any too weighty a burden to lay to the charge of retention and accumulation of uric acid in the blood.

There may remain a certain quantity of uric acid in the urine, at the same time that the analysis of the blood shows an abnormal amount therein. This would indicate a large supply from some source—either from waste of the tissues, or from the excessive ingestion of highly-azotized food, to which latter cause we some time since alluded. Dr. Copland notices, in a comprehensive and satisfactory manner, the “Pathological Relations of Uric Acid and Urate of Ammonia” (*Dict. of Pract. Med.*); and M. Becquerel (*Séméiotique des Urines*) agrees with him in his views. Copland, referring to this, considers the chemical explanations offered by Liebig, in connection with certain of these pathological points, “opposed to clinical observation.”²

Dyspepsia, with mal-assimilation of food and consequent deficient nutrition, or arrested cutaneous function and habitual costiveness, no less than obstructed excretion of the urine, may engender an excess of uric acid, and the latter may be retained in the blood. (Copland, *loc. cit.*; Chambers, *Digestion and its Derangements; et al.*) It is likewise true, conversely, that in depraved, deteriorated, and anæmiated states of the circulation, uric acid is diminished in the urine. If this ill-adjusted balance implies the throwing of an unusual quantity of the substance into the blood, the latter circumstance may have no small amount of influence, if not in causing, at least in continuing, the disorder of the blood itself and of the constitution generally. Gout occurring under such circumstances—as it is not very infrequently known to do—would be appropriately termed *atonic gout*, or what Dr. Todd (*Clinical Lectures*) terms “*asthenic gout*,” in contradistinction to his “*sthenic gout*,” where the affection occurs in robust, well-constituted, and richly-nourished individuals.³

In reference to the dyspeptic condition of gouty patients, often so exceedingly troublesome, Dr. Chambers (*op. cit.*, American edition, 1856, pp. 294–95) refers rather scornfully to the influence of uric acid as a noxious element. He is remarking upon the tendency of the food to become acid after ingestion, and to lie unchymified—not “passing onwards.” This

¹ The blood contains, as Dr. Garrod remarks, “mere traces” of uric acid in health. This fact, however, in no degree invalidates the agency of the acid as a *causative element in gout*.

² Dr. Copland does not believe that the presence of urate of soda in the blood of gouty patients precludes “the elaboration of a portion of the uric acid and its compounds, or the modification and metamorphosis of one or more of their elements by the kidneys.” (*Dict.*)

³ These two forms of gout, we conclude, are those termed by Dr. Druitt “*high*” and “*low*” gout. (*The Surgeon’s Vade Mecum*, 1859.)

state of things, so common in hereditarily gouty persons, Dr. Chambers is inclined to explain by considering its pathology "to be a slight flux of mucus, deficient gastric secretion, and yet a vigorous, sometimes even excessive appetite. Hence, they have not that check of failing desire for food which makes the meals of other invalids moderate, and eat more than their imperfect gastric juice can digest. This is a simpler, and, therefore, more probable explanation than the usual chemical talk about uric acid, &c., which might be substituted for it." It seems there is some fault in the working of the hidden *chemistry* of the body, however; and, although Dr. Chambers's explanation is doubtless very correct in reference to the influences and agencies of which he speaks, yet the overt action and manifestations of excess of uric acid, in the visible form of urate of soda concretions, sufficiently show the importance of the part it plays in the *tout ensemble* of gouty affections.

The original conclusions of Dr. Garrod, given in the admirable paper to which we have referred, were these:—

"1st. The blood in gout contains *uric acid* in the form of urate of soda, which salt can be obtained from it in a crystalline state.

"2d. The uric acid is diminished in the urine immediately before the gouty paroxysm.

"3d. In patients subject to chronic gout with tophaceous deposits, the uric acid is always present in the blood and deficient in the urine, both absolutely and relatively to the other organic matters, and the chalk-like deposits appear to depend on an action in and around the joints, &c., vicarious of the 'uric-acid-excreting' function of the kidneys.

"4th. The blood in gout sometimes yields a small portion of urea (no albumen being present in the urine)."

These conclusions were all duly sustained by analyses and experiments upon patients in University College Hospital; and the record of these demonstrations is at once satisfactory and highly interesting.

In respect to the supervention of gout in low and debilitated states of the system, to which allusion has previously been made, Dr. Garrod very clearly explains "two facts" then considered opposed to referring the pathology of gout to the humoral doctrines. We may say, *en passant*, that the humoral pathology seems the only reasonable mode of explaining the affection, and that, as may be distinctly perceived, it is coming more into favour of late, than it has been for a long time, in respect to many diseased conditions.

Dr. Garrod's remarks on the above point are:—

"Any undue *formation* of this compound (urate of soda) would favour the occurrence of the disease; and hence the connection between gout and uric-acid gravel and calculi; and hence the influence of high living, wine, porter, want of exercise, &c., in inducing it." Then, speaking of the "two facts"—viz., *hereditaryness* and the supervention of gout in *low states* of the system—he says: "We can understand that the peculiarity of the kidney, with reference to the excretion of uric acid, may be transmitted; and likewise that, when the function in question is permanently injured [viz., the 'uric-acid-excreting' function], it will not require an excessive formation of the acid to cause its accumulation in the blood." (*Loc. cit.*, pp. 93, 94.)

Dr. C. J. B. Williams (*Principles of Medicine*), referring to the fact that gout had been generally admitted, by inference, to depend on the existence of an excess of uric acid in the system, chronicles Dr. Garrod's experiments and analyses, which, as we have stated, were first made on patients under Dr. W.'s care, in hospital. He says:—

"Gout, and the commonest kind of urinary gravel are now generally considered to depend on the production in the system of an excess of lithic acid." (*Loc. cit.*) The case from experiments upon which Dr. Garrod was first enabled to draw his reliable conclusions, "was one of chronic gout; and further illustrated the pathology of the disease, by a total absence of lithic acid in the urine, until during the exhibition of colchicum, when its characteristic crystals appeared under the microscope."

Sufficient testimony, it would appear, has been adduced, to render the position tenable which ascribes the gouty paroxysm to an excess of lithic acid, circulating in the blood, and finally deposited in various parts of the body.¹ It does not seem to devolve upon us to describe the phenomena of a fit of the gout—expected as we are, merely to signalize the "effects" of retention of the various elements of the urinary secretion in the blood, we should strictly confine ourselves to such specification, and to the adduction of the best evidence afforded in its support.

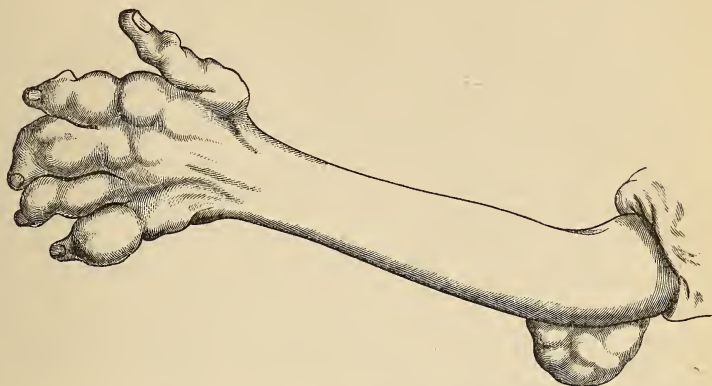
In the first place, then, we may refer the phenomena of gout, more or less completely, to that disorder of the eliminating function of the kidneys, which permits the latter organs to refuse the excretion of uric acid. The latter substance is then necessarily thrown into the circulation, and its tendency—under the circumstances—is to accumulation in the blood. As it accumulates, it is converted, most commonly, into urate of soda, and the deposition of the latter substance upon and into various tissues of the body is a *quasi* vicarious discharge of the uric acid, not excreted by its legitimate channels, the kidneys. This condition is accompanied by the objective phenomena of gout, viz., pain, of an exquisitely acute and torturing character; feverishness, dyspeptic symptoms, and general *malaise*;² at the close of the paroxysm, tense, shining, and often excessive swelling of the affected part; finally, entire remission of the symptoms, and better health than before the attack—owing, of course, to the elimination of the *materies morbi*. The *subsequent course* of things will depend very materially upon the habits of the patient, and upon the power he has of restraining his appetites; the fact of hereditary predisposition, or the contrary; and whether the management of the initial attack is judicious, or otherwise. By renewed attacks—chronic gout—the system of course becomes more shattered, less capable of resistance, and less amenable, too often, to remedies. Locally, a variable, often an excessive, amount of injury supervenes. It is at these periods that the lithate of soda—the morbid material—is deposited in various places—principally about the smaller joints. Nature, in her efforts to eliminate this material by other channels than those which are the legitimate ones, does the best she can, but often terribly cripples the subjects of this trying affection (see Fig. 2). Connected with this chronic form of the complaint, we are apt to notice the most troublesome combinations of dyspeptic ailments; and in these the condition of the patient becomes the most unpromising possible, both from the local and from the systemic difficulty. As Dr. Williams states, the uric acid, in such cases, "seems to be engendered in great abundance, and although thrown off in large quantities in the urine for an indefinite period, yet never leaves the body free. Such cases are commonly either hereditary, or those which have been ren-

¹ See Appendix, Note B.

² "An impure state of the blood, arising principally from the presence of urate of soda, is the probable cause of the disturbances which not unfrequently precede the seizure, and of many of the anomalous symptoms to which gouty subjects are liable." (A. B. Garrod, *op. cit.*, p. 341.)

dered inveterate by intemperate habits, or neglect of proper treatment.” (*Op. cit.*)

Fig. 2.



Elbow and Hand deformed by Gouty Enlargements.—From Dr. Garrod's work (*Nature and Treatment of Gout.*)

Sequelæ of Gout.—Considering gout as one of the affections ascribable to the retention of uric acid in the blood, let us inquire what are the principal subsequent results. These may, of course, be properly referred to the same cause, as “morbid effects.”

We have seen that symptoms of general febrile disturbance, which are of course accompanied by nervous apprehension, fretfulness, heat of skin, more or less sleeplessness, and scanty urine—and which at last is loaded with the lithates, whenever the paroxysm of gout comes on, and at its decline, especially—with hypochondriacism, cramps, flatulence, diarrhœa, but more commonly costiveness (Watson), and more or less general and indescribable *malaise*, both precede and accompany gouty attacks. The dyspeptic symptoms may long remain; although, with care, they may be made sooner or later to disappear.

One of the most serious sequelæ of gout may be considered that wherein the kidneys are affected. Perhaps we may best describe the state we wish to indicate, by terming it the result of a sort of *recoil* upon the kidneys, of the antecedent morbid action in the economy. This is of course entirely the opposite condition of that obtaining when the kidneys fail to eliminate the uric acid, *i. e.*, when their “uric-acid-excreting function” is suspended. In the latter condition of things, we have the blood-disease, the results of which we have sketched above; but when the uric acid begins again to be excreted by its natural passages, the kidneys are very likely to be more or less irritated in the process. There may then be violent pain (*nephralgia*), and nephritis or true inflammation of the substance of the organs may occur. Fully as unfortunate is that state when there is such an amount of uric acid as to be thrown down from the fluid part of the urine—the latter not being sufficient to hold it in solution, and thus carry it out of the body—when it is exceedingly apt to become concreted in various parts of the urinary passages—thus producing obstructions more or less seriously endangering health, and vitiating, in different degrees, the integrity and usefulness of the organs involved. Permanent irritation may thus be maintained; or serious and fatal inflammation be set up; and concretions may be found in

the urinary bladder, which will call for surgical interference for their removal.

Dr. Williams (*op. cit.*), referring to the renal irritation frequently caused in these cases (p. 130, American edition, foot-note), says: "I have in several instances found in the cortical and tubular structure of the kidney, clustered crystals of lithic acid, which, under the microscope, exhibited such sharp angles and dagger-shaped projections, as would afford an easy explanation of the pain, inflammation, and hemorrhage, often attendant on an attack of renal gravel, even when none is obvious in the urine." The same writer reminds us (p. 187), of "the proximity in composition between lithic acid and urea," and that it is probable, according to Liebig, that the former may be converted into the latter. He likewise calls attention to the fact that both gout and rheumatism may give rise to fluxes and catarrhal affections, as oliguria does. Rheumatism and Bright's disease are also, often, nearly related. In respect to this fact, Dr. Garrod announced the following opinions and conclusions, in his paper already quoted (*Med.-Chir. Transactions*, vol. xxxi.), "Blood from patients suffering from Bright's disease and albuminuria after scarlatina, was then examined; the results of these analyses appear to show that—

"1st. Uric acid is always present in the blood in albuminuria. The quantity, however, greatly varies: when the functions of the kidneys are much impaired, it exists in quantities almost as great as in gout; in other cases its amount is small, but it usually exceeds that found in ordinary blood.

"2d. Urea always exists in large quantities in this blood (a fact which has been long since proved), and no relation is found between the amounts of urea and uric acid.

"3d. The kidneys are always deficient in their power of throwing off urea; but with regard to the uric acid, their excreting function may be impaired or not." (*Loc. cit.*)

We thus see what serious disturbances may arise in the system by a perverted condition of secretion, arrest of excretion, and attempts at vicarious elimination of a product which must, in order to the preservation of health, be discharged from the body. The diseases thus produced come clearly under the head of *disordered vital chemistry*. Thus, when the above vital functions are weakened, or totally disabled, there must be not only general disturbance, but, after a time, some special manifestation of disease, and the results of vitiated secretion, decomposition, and over-worked and irritated organs.

The *contracted kidney*—called, by the late Dr. Todd, "the gouty kidney"—and which, as the term he has given it implies, he considered due to the effects, at once irritant, inflammatory, and destructive, arising from gout, we may mention as a result primarily dependent on disorder of the "uric-acid-excreting" function. Dr. Todd has given satisfactory proof (*Clinical Lectures on Certain Diseases of the Urinary Organs*) from cases of patients, of the existence of this form of renal disease. He mentions, also, that many might be inclined to refer it to advanced Bright's disease; but he has signalized it in those who he believes never had that affection. The kidney is shrunken, "and its structure condensed—a condition which, while it may also occur in other states of the system, is

¹ A term objected to by Dr. Barclay (*op. sup. cit.*); who, however, very distinctly recognizes the close connection between gout and renal disease. Valleix, writing in 1853, would not admit that special form of nephritis which is referred to the gouty diathesis.

peculiarly apt to be developed in the inveterate gouty diathesis." (*Loc. cit.*, p. 313.) In one case, necroscopy disclosed hypertrophy of the heart with dilatation—partly due to incrustations on the mitral valve—hardened, condensed, and somewhat contracted liver—the Glissonian capsule being hardened and thickened. The morbid alteration in the latter organ is explained partly by the intemperate habits to which the patient was addicted, "but partly likewise by the share which the liver had in the elimination of the morbid poison of gout." This latter result, which is not infrequent, it is important to notice, as one of the secondary effects of the diverted and retained urinary element. The kidneys in this patient were very much contracted, being hardly one-third of their natural size; they were granular and shrivelled upon their surfaces; the investing capsule was apparently denser and whiter than usual, and was easily detached from the glandular surface. Diminution of the cortical renal substance was the source of the decrease in the size of the organ; two-thirds being estimated to have disappeared. The granulations were noticed likewise upon the cut surface. Dilated and scantily-lined *tubuli uriniferi* were observed on microscopic inspection; from some of them all the epithelial lining had vanished; others were collapsed, folded, and crumpled up; and looked like "fascicles of fibrous tissue." A few fatty epithelial cells were detected in certain *tubuli*; and others of the latter were seemingly healthy, especially those in the pyramids of Malpighi. Dr. George Johnson¹ has described, very minutely, this condition of the kidney, and particularly notes the changes which supervene in the vascular system of the gland. In another instance, Dr. Todd has recorded the discovery, in an inflamed "gouty kidney," of "*opaque streaks of deposit of lithate of soda*" in some of the renal cones. These streaks took the direction of the tubes, and certain of the latter were probably occupied by them.² We may well exclaim with Dr. Todd, as we reflect upon these vital manifestations and post-mortem revelations—

"How strikingly do these consequences of the long continuance of the malady comport with the humoral view of the pathology of this disease! Not only are those parts which the morbid matter of gout is most prone to affect, materially damaged, but likewise the emunctories through which the poison would make its escape out of the system—the liver and kidneys: these organs have become poisoned by the morbid matters which have escaped, or tried to make their escape from the system through them; and, therefore, it is natural to expect a considerable change in their nutrition."

It is notorious, however, that in most, if not all, of the *metastases* of gout, there are no traces of morbid action upon the organs affected by the repercussion of the disease. The reason of this doubtless is, that when a fatal result occurs in this manner, the morbid action lasts too short a time, notwithstanding its violence, to leave structural traces. Probably a true *spasmodic* action often destroys the patient, in the thoracic and abdominal varieties of retrocedent gout. It is, however, very likely that in many cases of sudden death from these causes, the vital organs have been previously weakened by disease of some sort; and in many cases chronic gout has inflicted a certain amount of injury, for, as we have already seen, the continued and recurring malady *does* leave its indications, very decidedly, behind it.

¹ On Diseases of the Kidney.

² This appearance is also noted by Dr. Garrod, who saw "streaks of white matter at the apex of each pyramid and running up in the direction of the tubuli. Kidneys pale and contracted; cortical portion shrivelled." (p. 199.)

After gout in the *stomach*, Dr. Todd signalizes a "dilated and flaccid state of the organ" as existing, and this is the more marked in proportion as the attacks have been frequent.¹

In addition to the abundant and often astonishingly copious deposits of the urate of soda into and around the joints, that salt has been found to cover and even to penetrate into the texture of the cartilage investing the affected joints (Watson), and to insinuate itself into the substance of tendons and ligaments (Dr. Wm. Budd). A curious locality is at other times chosen by it—viz., under the skin covering the cartilages of the ears. It has been remarked also over the cartilages of the *alæ nasi* (Todd).² Pus sometimes forms around the variously located depots of urate of soda, and occasionally the discharge of this liquid is quite abundant. Generally speaking, also, the joints of the hands and arms exhibit more plentiful deposits of the urate than those of the lower extremities, except in some forms of acute sthenic gout, when the reverse may be observed. The interference with the motion of the various joints thus affected is so evident and so familiar to practitioners, and indeed so well known to nearly every one, that we hardly need do more than allude to it as a "morbid effect" of the disease. The fact, moreover, has already been made sufficiently prominent.

¹ Op. cit.

² Dr. Garrod (op. sup. cit.) states that "sometimes small nodules of urate of soda are found upon the eyelids, especially the lower, now and then in the integuments of the face." He also refers to the great care which is necessary in ascertaining the precise nature of the "white-looking deposits" often occurring about the eyelids and face. Dr. H. Barker saw them on the nose. Dr. Garrod has "observed a true gouty deposit as large as a split pea, apparently attached to the fibrous structure of the corpus cavernosum penis." (p. 86.)

In relation to gouty affections of the eye and ear, Dr. Garrod further remarks: "A form of ophthalmia connected with gout has long been noticed." He adds that it is liable to be confounded with rheumatism, when that is directed to the ocular region. Cases of ophthalmia evidently connected with the gouty diathesis have been observed by Dr. G.; these were instances of conjunctivitis and scleritis. "Gouty iritis is also said to occur." The nodules found upon the cartilages of the ear have been mentioned. "Deposits are not unfrequently found upon the drum of the ear, and have been especially pointed out by Mr. Toynbee, but I have failed to discover a trace of uric acid in several examinations of them. Whether they are ever connected with gouty inflammation, is at present a matter of uncertainty; they should especially be sought for in gouty subjects in whom the joints are much affected with chalk-stones, for if not found in such cases, it is not probable that they would occur in others." (pp. 515, 516, op. cit.)

Accomplished aurists have pointedly alluded to the connection between gouty and rheumatic affections and deafness. Mr. William Harvey (*The Ear in Health and Disease*, London, 1856) specially considers this subject; and we have heard him insist upon the frequency of the connection, whilst observing his aural practice.

In his elaborate work on *The Diseases of the Ear*, just published (London, 1860), Mr. Toynbee refers to the subject, and furnishes interesting illustrations of the reality of the connection, as observed by himself. He says: "The poison of *gout* may also give rise to deafness and other peculiar symptoms in the head." (See the work cited, page 362, English edition.)

(To be concluded in next No.)

REVIEWS.

ART. XIII.—*Statistical Report of the Sickness and Mortality in the Army of the United States, compiled from the Records of the Surgeon-General's Office; embracing a period of five years, from January, 1855, to January, 1860.* Prepared under the direction of Brevet Brigadier General THOMAS LAWSON, Surgeon-General United States Army. By RICHARD H. COOLIDGE, M. D., Assistant Surgeon U. S. A. 4to. pp. 515, with a map.

THE issue of this volume, following those of 1840 and 1856 (the former comprising the record of twenty, and the latter of sixteen years), completes the sanitary history of the United States army for forty-one years—from 1819 to 1860. An identity of arrangement between the report published in 1856 and the present one, has enabled the careful and industrious compiler, Dr. Coolidge, to consolidate all the aggregate results for the period of twenty-one years. The meteorological register for the term of five years, ending with 1859, is also appended; giving, with those contained in the previous reports, a continuous series of meteorological observations, taken in all parts of the country, from the organization of the Medical Department of the army to the present time.

The abundance of topographical details presented in the volume of 1856¹ has rendered unnecessary the accumulation of much matter of the same kind in this work. But, in several of the local reports, especially those from Florida, New Mexico, California, Oregon and Washington Territories, and Utah, very interesting information is afforded, in regard to the natural history and population of distant regions.

Among the reports from the Northern Division, an account is furnished by Assistant Surgeon La Fayette Guild, of the occurrence of yellow fever in July, August, and Sept. 1856, at Fort Columbus, on Governor's Island, in New York harbour. No meteorological peculiarities of the season were noted; and, although the buildings in which most of the cases occurred were somewhat deficient in cleanliness, yet the fact that *one side only* of those buildings (that towards the quarantine) was thus infected, conspired with other circumstances to show that the disease was the result of the atmospheric transmission of its cause from the numerous vessels congregated at quarantine. The same opinion has been elsewhere asserted by Assistant Surgeon J. H. Bailey, in regard to the contemporaneous epidemic at Fort Hamilton. Surgeon C. McDougall explains similarly the occurrence of yellow fever at Fort McHenry, Maryland, in September, 1858; but Surgeon B. M. Byrne prefers the theory of contagion, to account for its existence at Fort Moultrie, S. C., the same season.

In the fourth quarter of 1858, an epidemic catarrh prevailed at West Point, thus described by Surgeon S. P. Moore: "The stress of the disease fell upon the fauces, tonsils, and adjacent parts. An examination of

¹ Reviewed in this Journal, July, 1857.

the throat showed an extensive erysipelatous condition of the mucous membrane." The same affection, proving fatal in several cases among the inhabitants of the neighbouring country, was designated by them "the black tongue," or "malignant sorethroat." At Fort Ridgely, Minnesota, in November, 1857, a similar epidemic was described by Assistant Surgeon N. S. Crowell:—

"In the majority of cases, the tonsils were more or less inflamed, and sometimes enormously swollen. The inflammation in many cases was intense, and not confined to the tonsils, but involved the uvula, velum palati, and posterior fauces generally, and in a few instances extended even to the salivary glands, occasioning a disagreeable spontaneous ptyalism."

Assistant Surgeon B. I. D. Irwin reported the prevalence at Fort Buchanan, Arizona, in March, 1858, of "a most severe epidemic catarrh," with, "in every case, tonsillitic or pharyngeal inflammation." All of these epidemics appear to have presented some resemblance, at least, to what is now usually called diphtheria; although no mention is made of the observation of the diphtheritic exudation in the throat—of the pathognomonic importance of which it is possible that practitioners generally are now making too large an estimate.

Army service, especially at remote stations, affords excellent opportunities for the study of *scorbutus*. Many of the reports in this volume give account of it. Besides deficiency in the supply of fresh vegetable food, it is evident that other causes are promotive of scurvy; and are even capable in some instances of producing it, notwithstanding the use of articles of diet supposed to be antiscorbutic. Prominent among these causes are drunkenness, filth, despondency, ennui, and monotonous guard duty at night. The fullest investigation of the subject is presented in the reports of Assistant Surgeon E. W. Johns, at Fort Laramie, Nebraska. All of the reporters agree that potatoes are the best of the antiscorbutics; that citric acid, and even lemon juice, as well as the salts of potassa, frequently disappoint, both in prevention and cure; and that, although the "desiccated vegetables" sometimes answer an admirable purpose, yet the amount of their supply must be liberal in order to be efficient. The wild artichoke, water-cress, wild onion, cactus opuntia, agave Americana, phytolacca decandra, and various species of rumex, are named as indigenous remedies and prophylactics of ascertained value.

The anticipation entertained by some, that cholera might become *naturalized* or endemic in the valley of the Mississippi, does not appear to have been realized. In 1855, it prevailed to a serious extent at Jefferson Barracks and St. Louis Arsenal, at Fort Leavenworth, and elsewhere, in the Middle and Northern Divisions west of the Great Lakes, and in the South Interior Division east of the Lakes. But after that year no considerable number of cases was reported at any of the stations; the largest amount being ten cases and five deaths, at Fort Columbus, in New York harbour, in the fourth quarter of 1857.

In Florida, Fort Dallas is highly praised for its salubrity, by Assistant Surgeon R. F. Simpson; while a similar encomium, for equability of temperature and freedom from local causes of disease, is passed upon Fernandina and Cedar Keys, by Surgeon General Lawson, in a letter to Hon. D. L. Yulee.

Assistant Surgeon A. K. Smith, at Fort McKavett, Texas, gives some interesting information in regard to the serious effects of the bite of the *tarantula*. At Fort Buchanan, Arizona, Assistant Surgeon Irwin describes

tarantulas eight inches in length, with fangs an inch long; and centipedes ten inches long by one inch in breadth. These, as well as the vinagrilla and the scorpion, or *alacran* of Sonora, produce poisoned wounds, attended by severe suffering and even danger to life.

Dr. Irwin and several of the other contributors to the volume under notice, furnish ethnological details in regard to the native Indian tribes; whose number is still rapidly diminishing. Intemperance, syphilis, small-pox, and miasmatic fevers are their principal destroyers; the mortality among them from disease being greater than among the whites. According to Assistant Surgeon C. C. Keeney, this is especially the case with those fixed against their will on *reservations*. The latter writer notices, also, in his report, the great prevalence of *ophthalmia* in Scott's Valley, California; and the remarkable tendency to *prolificacy*, under the influence of the same climate, not only in human females but among the domestic animals.

Perhaps the most interesting portion of the work before us is that comprising the reports by Assistant Surgeons Bartholow, Moore, Ridgely, Milhau, and Brewer, of the sanitary history of the military expedition to Utah, in 1857 and 1858. In July, 1857, about seven hundred men, of the Tenth Infantry, commenced their long march of twelve hundred miles, from Fort Leavenworth, in Kansas, across the prairies to Salt Lake City, Utah Territory. Half of the command were new recruits; much of the disease occurring during the expedition being due to the broken-down constitutions or bad habits of those immature or untrained campaigners. The previously healthy, prudent, and temperate men constantly improved in physical condition during the march.

Diarrhœa, under dietetic and climatic causes, largely preponderated over all other forms of disease. Several cases of scorbutus also occurred.

In August, while passing through the Platte Valley, intermittent and remittent fevers appeared. After leaving Fort Laramie, no new cases presented themselves until they reached Ham's Fork, a stream running through an elevated valley 6,640 feet above the level of the sea. Can malaria be the product of local circumstances at such a height? Or, were the cases of periodical fever among these troops the result of the incubation of miasma introduced into the system in the Platte Valley, which they had left but ten days before? Either of these suppositions is tenable; the question as to which is the more probable, is ably discussed by the writers of these reports; although an exclusive settlement of it is hardly yet possible. Except in elevation, the valley of Ham's Fork is described by Assistant Surgeon Bartholow as resembling in all respects, in soil, temperature, and productions, the valley of the Platte River, where miasmatic fevers are rife.

The disease which is endemic in the high region alluded to, and elsewhere in the elevated country among the Rocky Mountains, has been called by some the "mountain fever." In its general character it appears to resemble the autumnal fever of the low country, with a decidedly adynamic tendency; but there are some discrepancies in the accounts given of it. Thus Dr. Ewing, in a paper in the *St. Louis Medical and Surgical Journal*, March, 1855, asserts the absence of nausea in mountain fever. Assistant Surgeon Bartholow observed constant nausea and occasional vomiting in the cases under his charge. Diarrhœa is, by the latter writer, spoken of as "in all cases a persistent symptom." Assistant Surgeon J. J. Milhau states that in those seen by him the bowels were "nearly always

constipated." The hot stage, according to Dr. Bartholow, was "in but few cases followed by a sweating stage." In the experience of Assistant Surgeon C. Brewer, "throughout the course of the malady, previous to the remissions, copious perspirations occur." Drs. Bartholow and Brewer agree as to the *irregularity* of the paroxysms and remissions or intermissions. But Dr. Milhau remarks that one of its most striking features is "the uniformity of the train of symptoms in the different cases." A similar variation of opinion exists in regard to the treatment, although all unite in asserting that the mild cases tend toward recovery in six, eight, or ten days. A relapse often takes place on the fifth or sixth day. Dr. Bartholow "had the most satisfactory evidence" of the curative power of quinine in the serious cases. The mildest he did not consider to require it. On the contrary, Dr. Milhau, having prescribed quinine, found that its action was not favourable, and abandoned its use. He believes quinine to be injurious in this disease. It was used by Dr. Brewer in the intermissions and decided remissions. Larger experience is evidently wanting, before "mountain fever" can take its definite place in our nosography. In some respects, a resemblance may be imagined between it and the "relapsing fever" of Scotland.

The following extracts from the report of Assistant Surgeon Bartholow, in regard to the physiological influences of Mormonism, will probably interest the reader:—

"Isolated in the narrow valleys of Utah, and practising the rites of a religion grossly material, of which polygamy is the main element and cohesive force, the Mormon people have arrived at a physical and mental condition, in a few years of growth, such as densely populated communities in the older parts of the world, hereditary victims of all the vices of civilization, have been ages in reaching. This condition is shown by the preponderance of female births, by the mortality in infantine life, by the large proportion of the albuminous and gelatinous types of constitution, and by the striking uniformity in facial expression and in physical conformation of the younger portion of the community."

"The activity of the reproductive function, as a rule, is not diminished by polygamy; on the contrary, the women are remarkable for fecundity; but in the harems the proportion of children arriving at maturity is much less than in the rural districts of our country."

"If Mormonism received no additions from outside sources, these influences continuing, it is not difficult to foresee that it would eventually die out, the increase of population, independently of large annual accessions from abroad, has not been coequal with the increase in other portions of our country. The results of polygamy here are not to be compared, without some limitations, to the results of the same institution elsewhere; its decadence must follow more speedily. In eastern life, where it has been a recognized institution for ages, women are prepared for its continuance, and do not feel degraded by their association with it. The women of this Territory, how fanatical and ignorant soever, recognize their wide departure from the normal standard in all Christian countries; and from the degradation of the mother follows that of the child, and physical degeneracy is not a remote consequence of moral depravity."

In the very full and carefully systematized tables and abstracts of disease and mortality, which occupy about a hundred pages of this large quarto volume, the sanitary statistician will find a vast deal of material, important especially on account of its *definiteness* of statement. We cannot avoid the expression of deep regret, that sufficient interest has not yet been awakened in the subject of medical statistics, to cause an effort, in civil practice, to obtain similar results, by the accurate registration, on a large scale, of the amount of disease as well as of mortality.

H. H.

ART. XIV.—*Die Cellular Pathologie, in ihrer Begründung auf physiologische und pathologische Gewebelehre.* Von RUDOLF VIRCHOW, o.ö. Prof. der path. Anat., &c. &c. Mit 144 Holzschnitten. Zweite neudurchgesehene Auflage. Berlin, 1859.

La Pathologie Cellulaire, basée sur l'étude physiologique et pathologique des Tissus. Par R. VIRCHOW. Traduit de l'allemand sur la seconde édition, par PAUL PICARD. Edition revue et corrigée par l'auteur, avec 144 figures. Paris, 1861.

Cellular Pathology, as based upon Physiological and Pathological Histology. Twenty lectures delivered in the Pathological Institute of Berlin, during the months of February, March, and April, 1858, by RUD. VIRCHOW, Public Professor in Ordinary of Pathological Anatomy, &c., in the University of Berlin, &c. &c. Translated from the second edition of the original, by FRANK CHANCE, with notes and numerous emendations, principally from MS. notes of the author, and illustrated by 144 engravings on wood. London: J. Churchill, 1860.

PERHAPS no man living at the present time enjoys a higher reputation for honesty of purpose and keenness of observation in pathological research than Professor Rudolph Virchow. The history of medical progress in this department during the last ten years and more is full of his name. To the progress which scientific medicine has achieved during this period he has contributed perhaps more than any other single individual. To ignore his papers scattered in various German journals, and especially in that of which he is the editor (*Virchow's Archiv*), would be to ignore some of the most prominent landmarks of medical advance. Full of the spirit of investigation, himself an investigator of the first ability, Virchow is remarkable among the moderns for the reverence with which he regards the past. It is one of the sins of the present of medicine to despise the past, to think of it lightly or contemptuously, to overlook completely our own deficiencies in our inordinate perception of the deficiencies of our predecessors. This is an error as hurtful and not so noble as the blind adoration of the ancients, so common fifty years ago. In this Virchow is conservative.

"I have already often been reproached," he says, "with endeavouring to rehabilitate antiquated views in modern science. In respect to this, I can, I think, say, with a safe conscience, that I am just as little inclined to restore Galen and Paracelsus to the position they formerly held, as I am afraid openly to acknowledge whatever truth there is in their views and observations."—*Preface to first edition.*

The *Cellular Pathology* consists of twenty lectures which were originally delivered in 1858 to a considerable number of the physicians of Berlin, and which were subsequently published from short-hand notes taken for the purpose. A second edition soon followed, in which, however, the author limited himself to "improving the language, expressing in more precise terms what was liable to be misunderstood, and expunging repetitions." (*Pref. to 2d edit.*) The second edition was speedily followed by French and English translations, both of which were made under the supervision of the author.

These lectures created a sensation. We are told by Paul Picard, the French translator, that "all the physicians who followed his course, carefully examined his preparations, and seriously heard his lectures, were con-

vinced." In their printed form they have already been circulated extensively throughout Europe, and have everywhere been received with respect, even by those who still dissent from many of the views expressed.

Perhaps many of our readers, however, will be surprised when we tell them that the *Cellular Pathology* contains very little that is new. Even those facts and views which more particularly belong to Virchow have for the most part been published by him elsewhere. Its author acknowledges this in the most modest manner.

"I trust, therefore, that what I offer," he says, "may not be taken for more than it is intended to be. Those who have found leisure enough to keep up their knowledge by reading the current medical literature will find but little that is new in these lectures."—*Preface to first edition.*

This fact, however, in nowise impairs—on the contrary, it in every way increases—the value of the work; it may be regarded as rendering accessible to the general medical reader not merely the richest fruits of the twenty years' scientific labours of its distinguished author, but also as a reflex of the progress of the whole German mind in this direction during the decennium which has just expired. For the doctrines of the *Cellular Pathology* are not the product of any one single mind alone. Step by step, almost from the very day of the publication of Schwann's researches, such investigators as Hugo von Mohl, Remak, Reichert, and others, as well as Virchow himself, have collected the materials without which such generalizations as these would have been impossible.

The fundamental idea which underlies the cellular pathology is a total denial of the possibility of the spontaneous generation of cells in a blastema, or, as Schwann's disciples have called it, "free cell-development."

"Even in pathology we can now go so far as to establish as a general principle that no development of any kind begins *de novo*, and consequently as to reject the theory of equivocal (spontaneous) generation just as much in the history of the development of individual parts as we do in that of entire organisms. Just as little as we can now admit that a tænia can arise out of saburral mucus, or that out of the residue of the decomposition of animal or vegetable matter an infusorial animalcule, a fungus, or an alga can be formed, equally little are we disposed to concede, either in physiological or pathological histology, that a new cell can build itself up out of any non-cellular substance. Where a cell arises, there a cell must have previously existed (*omnis cellula e cellula*), just as an animal can spring only from an animal, a plant only from a plant."

And again:—

"An eternal law of *continuous development* prevails."—p. 27.

How fundamentally these views are opposed to the notions generally received of the formation of pathological tissues will be best appreciated by comparing the following quotation from the Sydenham translation of Kölliker's *Microscopical Anatomy*, which well represents the view at present received as orthodox by the majority of American physicians:—

"Free cell-formation is exceedingly frequent in pathological productions, and the cells in pus and in exudations of all kinds arise in this manner; in fact, all pathological cell-formation properly comes under this head."

This, however, was Kölliker's opinion for pathological formations only; with regard to the *physiological* production of tissues he writes, in the same page:—

"With regard to physiological processes, as has been already shown, free cell-development has been much too readily taken for granted."

And again:—

“Botany knows no free cell-development.”¹

In order to arrive at a correct appreciation of these contradictory dogmas, it is necessary to glance briefly at the historical evolution of the theory of cell-development. The “cell theory” may be regarded as having originated, in very nearly its present form, with the distinguished botanist Schleiden, whose investigations on this subject were given to the world in 1837.² These researches referred to the development of *vegetable cells*. Schwann’s investigations (*Mikroskopische Untersuchungen*), published the two following years, had for their object, by an investigation of the animal tissues, and especially of the tissues of the embryo, to render Schleiden’s doctrines applicable to the animal world. Schwann started upon his investigation from Schleiden’s stand-point, full of the preconceptions that would naturally spring from such a source. In fact, therefore, it is to Schleiden’s researches that we are to look for the data on which the theory of free cell-development is to be based.

But although Schleiden and Schwann, by their publications, have given a great impulse to minute investigation, and have added to the general stock of our knowledge innumerable details, we cannot regard either of them as deserving the credit, usually ascribed to them, of originating the idea that all animal and vegetable tissues, even the most dissimilar, originate out of cells. In this idea they were more or less completely anticipated by others, and, above all, by Raspail, the father of micro-chemistry, a man whose labours in this direction have been too generally ignored. In Raspail’s *New System of Organic Chemistry*, the first edition of which, in one volume octavo, was published in Paris in 1833, four years before the publication of Schleiden’s researches, will be found the germ of the more elaborate views of Schleiden and Schwann.

“In the course of this work,” says Raspail (for the convenience of the reader, the quotation is from Henderson’s English translation, London, 1834), “we shall more than once have occasion to prove that the type of organized beings may be reduced in its simplest expression to an imperforated vesicle, possessed of the property of elaborating for its own nourishment, to an indefinite extent, the gaseous or liquid substances which it takes in by aspiration, and of throwing off by expiration such of their elements as are incapable of being assimilated.”

These vesicles Raspail described as originating in “a liquid tending to organize,” in which we at once recognize the cytoblastema of Schwann. He even compares the process of their formation to crystallization, a comparison elaborated by Schwann, who has been generally believed to have originated it. Raspail himself, however, by no means observed *de novo* even the large majority of the microscopical facts on which his vesicular theory reposed. The isolated microscopical observations of the eighteenth century had been gradually accumulating since the beginning of the nineteenth, and even systematic attempts had been made at classification, notable among which may be noticed that of Treviranus in 1816, and of Heusinger in 1824.

Although, however, Schleiden’s views were thus far anticipated by Raspail in their general results, the special observations upon which he relied for their support were original and valuable. Schleiden, we have already said, was a botanist; the situations in which he studied cell-development

¹ Kölliker’s *Mic. Anat.*, Amer. edit., p. 49.

² *Untersuchungen über Phytogenesis*, Müller’s *Archiv*, 1838.

were especially the large cell (embryonal sac) which subsequently contains the so-called albumen of the seed, and the extremity of the pollen-tube from which the embryo itself is developed.¹ According to him, in both these situations there exists a solution of sugar or gum, which at first becomes cloudy from the formation of innumerable granules; then, by the aggregation of the granules, nucleoli are produced; around these are formed the nuclei (cytoblasts), and around these the cells. When, however, Schleiden endeavoured to extend these observations to other parts of the plant which are the seat of active growth, he found the observations full of difficulty. Especially does he acknowledge his inability to make out the process in that remarkable layer (cambium) beneath the bark in which proceeds the growth of exogenous plants in thickness. It will at once be seen that these observations of Schleiden demonstrate, if they be accurate in all particulars, the formation of cells within cells, but do not in themselves justify in any way the idea of the free development of cells in a formative liquid outside of cells. Schleiden himself perceived this; and in summing up the probable deductions from his observations, we find him, therefore, saying:—

“If we have found the growth of the embryo to consist only in the *formation of cells within cells*, we may expect to find the same result also in the process of growth of the whole plant.”²

And again:—

“The entire growth of the plant consists only of a *formation of cells within cells*.”³

Turning now from Schleiden to Schwann, we are at once struck with the fact that while this observer traces out with considerable accuracy, and with perhaps fewer errors of interpretation than might have been expected in a new field of investigation, the process by which various tissues are produced out of cells, he, nevertheless, mainly relies for his account of the fashion in which *cells* arise upon Schleiden. But Schwann went further than Schleiden; finding innumerable cells in the embryonic tissues, lying in a semi-solid, more or less transparent substance, of various consistency, he too hastily concluded that this had acted as the blastema or formative substance out of which the cells had been generated by a process similar to what Schleiden had described as going on *within* cells. Schleiden had taught what is now called endogenous cell-formation; Schwann went further, and taught free cell-formation.

No one can read Schwann's book carefully without being at once convinced that this notion, though rendered exceedingly plausible by his ingenious reasoning, is based upon a very slender stock of facts; in truth, the whole strength and value of the work, and of the vast mass of its original observations, is to be found in connection with the processes by which the cells, once produced, are variously transformed; for the initial production of the cells he acknowledges his reliance upon the observations of Schleiden.

The doctrines which Schwann had announced for normal tissue genesis were speedily applied to pathological new formations by Johannes Müller.⁴ The work of Müller constitutes an era in the history of pathology. To him belongs the credit of having placed the classification of pathological

¹ Contributions to Phytogenesis, Sydenham translation, p. 237.

² Loc. cit., p. 253.

³ Loc. cit., p. 257.

⁴ Bau der krankhaften Geschwülste.

products, and especially of morbid growths, upon a rational basis. To him, also, we owe the enunciation of that fundamental law of the similarity between pathological and normal tissue development which has since been so triumphantly established by Wedl and Virchow. But while Müller's remarkable work fully deserves this high praise, we are obliged to censure him for having too blindly and without sufficient evidence received Schwann's doctrine of free cell-development. In fact, Müller's own observations are chiefly connected with the processes by which the several pathological products are developed out of cells, while for the formation of these he mainly relies upon Schwann.

Müller's pathological researches were soon followed by those of Vogel, who also, with the majority of pathologists—as Lebert, Rokitansky, Paget, and others—accepted Schwann's views of cell-development.

But while the pathologists were thus contented with the doctrine they had received from the botanists and anatomists, these were revising their labours, and variously modifying their doctrines, as the stock of facts was increased by observation.

It would not be possible, within the limits we have assigned ourselves, to describe step by step the progress made in these particulars. From the time of Schwann, observers multiplied. Indeed, it is the highest praise of his labours, that they were well calculated to stimulate the observations of others. A bare enumeration of the names of these investigators would be but a barren task. To specify the labours, and define the scientific position and value of each, to sift from the mass of the writings of every one the golden grains of truth—the *new facts*—which each contributed to the common stock, would fill a moderate-sized volume. It is only here possible to allude to one or two prominent names. And thus, among the botanists, honorable mention should be made of Hugo von Mohl, whose memoirs will be found in the *Linnæa*, the *Botanische Zeitung*, &c. &c. (or translated in the *Annales des Sciences Naturelles*, the *Annals and Magazine of Natural History*, and Taylor's *Scientific Memoirs*). This writer, whose earlier observations almost immediately followed those of Schwann (see *Botanische Zeitung*, vol. ii., 1844), arrived at conclusions differing from his in a remarkable manner. Not only did he deny free cell-development, affirming that "the formation of cells in plants occurs only in the cavities of older cells,"¹ but even in his account of the method in which cells arise within other cells, he differs materially from Schwann and Schleiden; thus, for example, he denies that the formation of the nucleolus precedes that of the nucleus.

So, also, among the anatomists, Henle, Mandl, and Reichert raised doubts, in animal cell-development, as to the pre-existence of the nucleolus, and as to other details of Schwann's doctrine, until at length, in 1852, Remak, in a valuable paper, denied in toto free cell-development, and announced, as his fundamental conception of cell-growth, the doctrine "*omnis cellula in cellula*."² A doctrine which was reiterated by Virchow in his former great work,³ in almost the very words—"*omnis cellula e cellula*"—and which has become the basis of the cellular pathology.

We have referred to these historical points, at the risk of being thought

¹ Die Vegetabilische Zelle, in Wagner's Handwörterbuch, vol. iv. p. 211.

² Remak, Ueber extra cellulare Entstehung thierischen Zellen, u. die Vermehrung derselben durch Theilung, u. über Entstehung des Bindegewebes, u. d. Knorpel. Müller's Archiv., 1852.

³ Virchow, Beitr. z. Spec. Pathologie und Therapie, 1854.

tedious, because the rejection of free cell-development, if correct, must necessitate a thorough pathological reform; for the humoral pathology of Rokitsansky and the Vienna school, which has been so generally received in Europe and America, falls with Schwann's doctrine; and, therefore, although no doubt the foregoing points will be familiar to many readers, we have presented them, with the desire to impress upon those who are not so well acquainted with the literature of the subject, the fact that Virchow is not to be regarded as a reckless innovator, whose propositions rest only upon his own *ipse dixit*, but that his leading doctrines are the legitimate fruit of the labours of many investigators, and that probably at the present moment, however much they might differ about minor matters, the majority of the investigators in this direction will heartily indorse the doctrine "*omnis cellula e cellula.*"

The medical reader will at once inquire—if this be so, if free cell-development is to be rejected, how shall we account for the various pathological products (lymph, pus, tumours, cancer, tubercle, &c.) which have been supposed to be evolved by this process in the blastema or plasma exuded from the vessels of the diseased part? To this, Virchow at once replies:—

"We may, therefore, with trifling restrictions, *substitute for the plastic lymph—the blastema of the earlier, the exudation of the later, writers—connective tissue with its equivalents as the common stock of germs of the body*, and directly trace to it, as the general source, the development of new formations." (p. 398.)

The rôle played, in Virchow's comprehension of pathological processes, by connective tissue is, it will be perceived, in marked contrast with those entertained of this tissue up to the present time by very many physiologists, who have looked upon connective tissue as a very passive structure, having purely mechanical functions, and but little liable to the diverse morbid processes.

To understand Virchow's views of the genesis of pathological formations, it will be necessary to refer to his ideas of the structure of normal connective tissue, which differ in many respects from those ordinarily received in this country. The dominant view with us is, that both the white fibrous and the yellow elastic elements, which, variously intermixed, constitute this tissue, are developed *out of cells*; and especially with regard to white fibrous tissue is it believed that the renowned bundles are developed out of cells which elongate, become spindle-shaped, finally fibrillate, and thus produce *the bundles*, which are to be regarded, therefore, as transformed cells. According to Virchow, on the contrary, the white fibrous bundles do not represent transformed cells, but are produced by the direct fibrillization or transformation of the intercellular substance, which Schwann regarded as the blastema out of which the imbedded cells were developed.

As with his negation of free cell-development, so here, Virchow does not stand alone. As early as 1843, L. Mandl¹ criticized Schwann's comprehension of connective tissue, and expressed the opinion, based upon his own investigations, that the white fibrous tissue is produced by the direct transformation of the intercellular material. In fact, from the very date of Schwann's work, connective tissue was made the object of a host of investigations; and the labours of Henle, Reichert, and Remak paved the way for the more complete view of Virchow.

While the intercellular substance is being transformed into a more or less distinctly fibrillated material, some of the cells are developed into elastic fibres, others into fat or pigment cells; but a large proportion of them,

¹ Manuel d'Anatomie Générale. L. Mandl, Paris, 1843.

having assumed a spindle shape, or sometimes a more or less stellate form, abide as permanent elements in the connective tissue of the body. These stellate—frequently anastomosing—minute elements it is which serve as the germ for the majority of pathological productions, and it is to them that Virchow applies the designation of "*connective tissue corpuscles*."

As may be supposed by what has been said, a large part of "The Cellular Pathology" is occupied by descriptions of processes and transformations of a purely physiological kind, but which, nevertheless, must be comprehended in order to appreciate the new doctrines. Passing by these points, let us glance at the new doctrines with regard to some of the most important pathological processes.

And first, of irritation and inflammation.

Irritability is, according to Virchow, the property which every living cell or cell derivative possesses of being compelled into action, by influences coming from exterior sources. It is the very criterion of life.

"Vital activity is, as far at least as we are able to judge, nowhere, in no part whatever, carried on by means allotted to it from the very beginning, and entirely confined to it, but we everywhere see that a certain excitation is necessary for its production" (p. 286); and as, whenever "a given action is called into play, we have to deal with a manifestation either of the *function*, the *nutrition*, or the *formation* of a part" (p. 288),

he discriminates between *functional*, *nutritive*, and *formative* irritability; which, nevertheless, may complicate each other.

Functional irritability is displayed when, in consequence of some influence exterior to itself, a part performs its function, a muscle contracts, a gland secretes, &c. It is evidently, in its nature, a purely physiological phenomenon, and only becomes morbid when excessive. The stimuli capable of calling into activity the functions of particular parts are not indifferent. "Nearly everywhere do we find *specific relations* or *affinities* to exist." (p. 293.)

Nutritive irritability manifests itself when, in consequence of some stimulus, the nutritive activity of parts is augmented. By Virchow, the term is limited to the conditions in which there is an increase in the size of the individual cells of the part affected; they take into their interior an unwonted quantity of nutritive material, and thus increase in dimensions while their contents becomes turbid. Thus—

"If we draw a thread through a cartilage, so that merely a traumatic irritation is produced, we see that all the cells which lie close to the thread become enlarged through an increased absorption of material." (p. 298.)

Such an enlargement and turbidity of the cells of the part, without other change, constitutes a frequent form of corneal opacity (parenchymatous keratitis). (p. 301.)

Formative irritability is discriminated from nutritive by resulting in an increase in the number of elements.

"If, namely, we follow up the higher degrees of irritation which take place in a part, we find that the cellular elements, shortly after they have experienced the nutritive enlargement, exhibit further changes which begin in the interior of the nuclei, generally in such a manner that the nucleoli become unusually large, in many cases somewhat oblong and sometimes staff-shaped. Then, as the next stage, we usually see that the nucleoli become constricted in the middle, and assume the form of a finger-biscuit; and, a little later, two nucleoli are found." "About such a divided nucleolus, the finger-biscuit-like constriction, and afterwards the real division of the nucleus takes place." "This may be repeated so

that three, four, or more nuclei arise." (pp. 306-7.) "If, now, we advance a step farther in these processes, we come to the new formation of the cells themselves." (p. 310.)

The cell most frequently, but not always, parts so soon as two nuclei are formed, two new cells resulting; and this process may be repeated until large groups of cells have thus been produced from single ones.

Each of the above forms of irritation may coexist with vascular or nervous disturbance; but these are collateral or consequential, as the case may be. They are not, as they have been so generally believed to be, the starting-point of the processes; for they may be manifested in nerveless and non-vascular parts (cornea, cartilage, &c.); and in parts supplied by both vessels and nerves, the phenomena manifest themselves, not in the track of these, but in the track of the irritant.

"If we make the experiment with the thread upon the skin, a whole series of nerve territories are intersected by it. Still, the whole of the territories belonging to the nerves which lie along the thread are not thrown into the same morbid condition, but the nutritive irritation is limited to the immediate vicinity of the thread." (p. 299.)

The two latter of these forms of irritation play an important part in the phenomena of *inflammation*.

Virchow describes "two forms of inflammation which can be separated from one another: the *purely parenchymatous inflammation*, where the process runs its course in the interior of the tissue, without our being able to detect the presence of any free fluid which has escaped from the blood; and the *secretory (exudative) inflammation* (which belongs to the more superficial organs), where an increased escape of fluid takes place from the blood, and conveys the peculiar parenchymatous matters along with it to the surface of the organs. That there are two different forms, is clearly shown by the fact that they occur, for the most part, in different organs." (p. 393.)

With regard to secretory or exudative inflammation, this may be accompanied by the accumulation, upon surfaces or in cavities, of *fibrin*, or by the production of mucus upon the mucous membranes. Now just as, "although a mucous membrane produces incredibly large masses of mucus in a short time, they are, nevertheless, products of the membrane itself," "the peculiar mucin matter" being "a product of the membrane," "conveyed to the surface by means of the fluid oozing through from the blood;" so, also, instead of regarding the fibrin (coagulable lymph) "as a real transudation from the liquor sanguinis, as the outflowing plasma, I have proposed the explanation that the fibrin, like the mucus, is a local product of those tissues on and in which it is found, and that it is conveyed to the surface in the same way as the mucus of the mucous membrane." (p. 392.) Absorption of the fibrin, thus locally produced, into the blood, is regarded as the cause of the increased quantity of fibrin observed in the blood in inflammatory fever. "The fibrinous crisis is just as much a product of the local disease, as the fibrinous exudation is a product of the local metamorphosis of matter." (p. 392.) The cellular elements (lymph, pus, mucous corpuscles, &c.) observed in the fibrin or mucus, are not new formations in the blastema afforded by the exudation, but are produced out of the cells of the parts involved, by the powers above described under the head of formative irritability.

In *parenchymatous inflammation*, on the other hand, there is nothing withdrawn from the bloodvessels which does not accumulate in the cells of the parts involved; these increase in size, become turbid, and even division

of nuclei and production of new elements may take place, or at any stage of the process various degenerative changes may set in (*e. g.*, fatty degeneration). This form of inflammation is especially observed in the liver, kidneys, and other parenchymatous organs.

It will thus be seen that for Virchow the starting-point of an inflammation, as well as an irritation, is to be found not in the bloodvessels, nor yet in the nerves, but in the *cells of the part affected*.

"If now, in the next place, we investigate the history of SUPPURATION, we immediately discover that we must distinguish two different modes of pus-formation, according, namely, as pus proceeds from tissues of the first two kinds mentioned in our classification—*i. e.* from *epithelium* or from *connective tissue*." (p. 445.)

In the first instance the pus-cells are produced by the multiplication of the cells of the younger strata of the *epithelium* (*e. g.* the rete Malpighi for the skin).

"The more deeply-seated pus-formation regularly takes place in the *connective tissue*. In it there first occurs an enlargement of the cells (connective tissue corpuscles); the nuclei divide, and for some time multiply excessively. This first stage is then very soon followed by divisions of the cells themselves. Round about the irritated parts, where before single cells lay, pairs or groups of cells are subsequently found, out of which a new formation of an homologous kind (connective tissue) usually constructs itself. More in the interior, on the contrary, where the cells were early abundantly filled with nuclei, heaps of little cells soon appear, which at first still preserve the direction and forms of the previous connective tissue corpuscles. Somewhat later we find here roundish collections, or diffuse 'infiltrations,' in which the intermediate tissue is extremely scanty, and continually liquefies more and more in proportion as the proliferation of the cells extends." (p. 451.)

After the abscess produced by such a process discharges, it is healed by granulations, for the production of which it is only necessary that the intercellular substance should retain a certain degree of consistence. So that the essence of the diversity between the formation of pus and the formation of granulations consists in this—that where suppuration is going on, the degenerative processes predominate over the formative, causing the intercellular substance to liquefy, and depriving the cells of further formative capacity; while in the case of granulations the formative processes predominate.

By a similar cell-multiplication in connective tissue are those granulations produced by which injuries are repaired.

If these views are admitted, it is no longer necessary to attribute to pus a corrosive action in order to account for the phenomena of *ulceration*.

"*Pus is not the dissolving, but the dissolved—i. e. the transformed tissue.*" (p. 446.) Wherever new elements are produced, they are produced *out of old ones, which cease to exist in this very act*. If, then, as in a progressing ulcer, the product of the cell-multiplication be constantly pus, a solution of continuity must result, and the ulcer will continue to increase in size so long as this transformation continues in excess.

If, now, from the inflammatory processes we turn to the production of morbid growths, tumours innocent or malignant, we find the same general principle maintained by Virchow. Everywhere the elements of the new growth arise out of the cells of the involved tissue, and especially out of the connective tissue corpuscles, by a process quite like that given above for pus and for granulations. If this statement be true, we need not be surprised to find asserted as its legitimate corollary—

"There is a stage when it is impossible to decide with certainty whether we have in a part to deal with simple processes of growth, or with the development of a hetero-plastic, destructive form." (p. 454.)

And again:—

"The first development of cancer, of canceroid, and of sarcoma, exhibits the same stages; if the course of their development be traced sufficiently far back, we at last always come across a stage in which, in the younger and deeper layers, indifferent cells are met with, which do not until a later period, according to the particular nature of the irritation to which they are exposed, assume the one or the other type." (p. 454.)

And here, in fact, is the explanation of the thousand and one errors of diagnosis which have been made by over-confident microscopists, and, above all, in the differential diagnosis of cancer.

In the classification of the several new formations, Virchow still preserves the division into *heterologous* and *homologous*. But he uses these terms in a very different sense from that in which they are commonly employed by those who apply the term heterologous only to the elements of cancer and tubercle.

"Heterologous we may call not only malignant, degenerative neoplasms, but we may also thus designate every tissue which deviates from the recognized type of the part; whilst we should call all that homologous which, though new-formed, still reproduces the type of its parent soil." (p. 442.)

Thus, for Virchow, the cells of pus, even the cells contained in the catarrhal mucus of an inflamed mucous membrane, are heterologous forms, because they are unlike the normal cells by the proliferation of which they were produced. At the same time, he distinctly proclaims—

"*Heterologous tissues also have physiological types.*" (p. 480.)

Perhaps the following passage best explains Virchow's views on this mooted question of heterology:—

"But if we ultimately arrive at such a simple view of the matter, the question of course arises, What becomes of the doctrine of the *heterology* of morbid products, to the upholding of which we have long been accustomed, and to which the most simple reflection almost inevitably conducts us? Hereunto I can return no other answer than that there is no other kind of heterology in morbid structures than the abnormal manner in which they arise, and that this abnormality consists either in the production of a structure at a point where it has no business, or at a time when it ought not to be produced, or to an extent which is at variance with the typical formation of the body." "But we must be very careful not to connect this kind of heterology, in the more extended sense of the word, with the notion of *malignity*." (p. 64.)

On the question of *malignity*, views are expressed very similar to those advocated by Bennet in his *Clinical Medicine*.

"Cancer is not malignant because it contains heterologous cells, nor canceroid benignant because its cells are homologous; they are both malignant, and their malignity differs only in degree." (p. 484.) Thus, "Cartilaginous tumours (enchondromata), which were formerly described as unquestionably benignant, sometimes occur in soft and rather gelatinous forms, which may occasion just such internal metastases as cancer, properly so called." (p. 486.)

So sarcomata, under certain circumstances, may "appear secondarily in the lymphatic glands, and in many cases occur throughout the whole body metastatically to such an extent that scarcely any organ is spared by them." (p. 487.)

If, now, we investigate the method by which morbid growths infect distant organs, we shall find that Virchow admits two distinct methods: either, on the one hand, the cellular elements of the growth may pass into the blood, as occasionally, but probably with great rarity, happens; or, on the other, contaminating juices originating in the focus of the disease, containing, however, no organic forms, may sweep into the lymphatic or venous circulation, and excite in distant parts the same action as existed at the seat of the primary disease.

The doctrine of a special blood disease preceding the first tumour is denied, as resting on insufficient data, a criticism which is certainly just, but which applies with equal force, in the present state of the investigation, to the view proposed by Virchow as a substitute.

The recurrence of certain growths after extirpation, however, is much more logically and satisfactorily accounted for by the interesting statement that in such cases—

“The zone produced at the latest period of the disease extends to a considerable distance beyond the zone of degeneration that can be discerned by the naked eye. If we examine any proliferating tumour of a cellular character, we often find, three to five lines beyond its apparent limits, the tissues already in a state of disease, and exhibiting the first traces of a new zone. This is the chief source of local recurrence after extirpation, for it proceeds from the zone that cannot be detected with the naked eye, beginning to grow in consequence of the increased supply of nutritive material which results from the removal of the original tumour. No new deposit from the blood takes place there, but the new-formed germs, which already lie in the neighbouring tissue, run through their further development in the same manner that it would otherwise have taken place, or perhaps even still more quickly.” (p. 458.)

Notwithstanding the similarity of the conditions presented in the early stages of the most diverse new formations, such evident diversities are established sooner or later, that *Virchow is of the opinion that a classification of tumours should be based upon their microscopical anatomy*, and not upon their coarser external features alone; but he cautions especially against haste in inventing new names by which we may render “things which have long been known strange to the mind of people in general.”

Such are the leading doctrines of the *Cellular Pathology* with regard to the genesis and relations of all the so-called new formations; but, besides the doctrine of new formation, many other themes are discussed in these lectures: the classification of physiological tissues; a *résumé* of the latest discoveries with regard to their minute anatomy; the doctrine of nutrition; the minute structure, the physiological and pathological relations of the nervous system; fatty, amyloid, and other degenerations; leukaemia, pyaemia, metastatical dyscrasiae, &c. It is not possible to present even the briefest abstract of most of these subjects; and, indeed, the object of this review was chiefly to call attention to the new views of the genesis of morbid products. But an account of this book would be most incomplete should it pass by in silence the views expressed with regard to tubercle, leukaemia, and the doctrine of thrombi and emboli.

The designation TUBERCLE is limited by Virchow to the miliary form. He includes under this head the tubercles of the arachnoid, which, as is well known, were excluded from the category of tubercle by Robin because they contained cells. In fact, however, for Virchow, there is a period in its history when every tubercle contains cells. For tubercle generally, like other new formations, has its origin in connective tissue, by a process of cell-proliferation very similar to that above described for pus, and differing

mainly in the circumstance that the cells and nuclei of a young forming tubercle are smaller than those seen in the case of pus, just as those of cancer or sarcoma are larger. By and by fatty degeneration sets in in the centre of the tubercle, then complete cheesy metamorphosis.

Tubercles, such as above described, never attain any great size; the large tubercles, of the size of a cherry, a walnut, or even larger, are not simple tubercles.

"You will generally find the tubercles in the brain described as being solitary, but they are not simple bodies; every such mass (tuber) which is as large as an apple" (a Borsdorf apple, $1\frac{1}{2}$ to $1\frac{3}{4}$ inch in diameter), "or even not larger than a walnut, contains many thousands of tubercles; it is quite a nest of them, which enlarges, not by the growth of the original focus, but rather by the continual formation and adjunction of new foci at its circumference. If we examine one of these perfectly yellowish-white, dry, cheesy tubera, we find immediately surrounding it a soft vascular layer which marks it off from the adjoining cerebral substance—a closely investing areola of connective tissue and vessels. In this layer lie the small young granules" (miliary tubercles), "now in greater, now in less number. They establish themselves externally, and the large tuber grows by the continual apposition of new granules (tubercles), of which every one singly becomes cheesy." (p. 478.)

But the cheesy metamorphosis is not peculiar to tubercle; pus may undergo a cheesy metamorphosis, and the same may befall cancer or sarcoma. In each of these cases a product is ultimately attained which is quite one in its coarser and minute characters, no matter what its origin. It is in the cheesy matter thus produced that we are especially to look for the so-called tubercle-corpuscles, rendered so famous by the researches of Lebert. Virchow, however, by no means regards these corpuscles as immature or imperfectly-formed elements, "the first bungling products, unfortunate essays of organization;" everywhere, he believes, the tubercle-corpuscles are formed out of pre-existing elements by a shrivelling and fatty degeneration.

"You may with certainty assume that, where you meet with a largish corpuscle of this description, a cell had previously existed; and where you find a small one, there once had been a nucleus inclosed perhaps within a cell." (p. 473.)

When, then, tubercle-corpuscles occur as the product of the cheesy metamorphosis in cancer, they represent degenerated and shrivelled cancer-cells and nuclei; when they occur in the products of the cheesy transformation of pus, they represent degenerated and shrivelled pus-cells. In fact, according to Virchow, those tubercular infiltrations of the lungs, regarded as so characteristic, are, as the rule, not tubercular, but represent inflammatory products which have undergone the cheesy metamorphosis.

With this brief notice of the subject of tubercle, let us next look at our author's views with regard to leukæmia and leucocytosis.

LEUCOCYTOSIS is the name given by Virchow to the many transitory conditions in which the number of the white corpuscles of the blood is considerably increased. A similar increase in their number, of a more permanent character, is designated Leukæmia. Leucocytosis is very frequently associated with inflammation, and above all with those diffuse phlegmonous or erysipelatous inflammations in which the lymphatic glands are involved. In fact, it is "very rarely that a considerable increase of fibrin takes place without a simultaneous increase in the colourless blood-corpuscles." (p. 166.) But, on the other hand, a number of conditions may occur, and especially the typhoid processes give us examples, in which leucocytosis coexists with more or less deficiency of fibrin. These latter cases invariably present greater

or less enlargement of the lymphatic glands (*e. g.*, the mesenteric glands in typhoid fever), or of the spleen; conditions which appear to stand in a genetic relation to the increased quantity of white corpuscles. These conditions are more or less transitory, the leucocytosis disappearing on convalescence from the condition in which it took its origin. Pregnancy, long known to be accompanied by a physiological increase in the quantity of the fibrin of the blood, is also accompanied by a physiological leucocytosis. At full term, the colourless corpuscles are so abundant that, in the defibrinated blood, they form a pus-like sediment. And in certain cases of puerperal fever, this sediment has actually been supposed to demonstrate the existence of pyæmia.

LEUKÆMIA, while it agrees with leucocytosis in being characterized by an increased number of white corpuscles, is a much more serious condition. More chronic in its course, it appears to tend almost invariably to a fatal termination; of all the known cases, but one left the hospital improved; "in all the other cases, the result was death." (p. 169.)

"These cases possess, in addition, the remarkable peculiarity that, usually towards the close of life, a genuine *hemorrhagic diathesis* is developed, and hemorrhages ensue, which occur with especial frequency in the nasal cavity (under the form of exhausting epistaxis), but may also, under certain circumstances, take place in other parts of the body; as, for example, on a very large scale, in the form of apoplectic clots in the brain, or of melæna in the intestinal canal." (p. 169.)

Leukæmia is not associated, as leucocytosis often is, with increase of the blood fibrin.

Two diverse forms may be discriminated: an ordinary splenic form, associated with chronic enlargement of the spleen; and a lymphatic form, associated with enlargements of the lymphatic glands. The two forms may often coexist.

Leukæmia, it will thus be perceived, is a "permanent progressive leucocytosis"—a serious, if not fatal, chronic disease; while leucocytosis is a transitory affection, the import of which will vary with the cause on which it happens to be dependent in any individual case.

The last subject to which we shall invite attention, is Virchow's doctrine of *thrombi* and *emboli*; a doctrine by which a new light is thrown upon the subjects of phlebitis, pyæmia, and metastatic abscesses.

The term *thrombus* is applied by Virchow to a coagulum of blood formed during life within a vessel, and most frequently within a vein. The whole process which results in such formations is designated thrombosis. Such coagula were first accurately described by Cruveilhier, and were made by him the basis of the somewhat famous, but now extinct, doctrine of capillary phlebitis as the primal cause of all inflammations. The clot thus formed does not long remain unchanged. Sooner or later, a central degeneration is set up, the fibrin breaks down and becomes granular, the red corpuscles lose their colouring matter, become granular on the edges, and finally almost entirely disappear; the white corpuscles, more or less altered, are set free in the whitish or yellowish-white puriform liquid which results.

This process has been erroneously described as suppurative phlebitis; but it is evidently not suppurative, as no pus is formed, but only a fluid having some of the external characters of pus; and careful examination shows that it has no necessary connection with phlebitis, inasmuch as it occurs with great frequency in vessels, the walls of which present no trace of inflammation. In fact, Virchow utterly denies the ordinary notions of arte-

ritis or phlebitis, in accordance with which the inflammatory products were regarded as being poured out from the walls of the inflamed vessel into its lumen; on the contrary, he strenuously asserts, and in this we believe he is correct, that in phlebitis the exudations, as they are called—that is, the inflammatory products—are formed in the very thickness of the inflamed vascular wall itself, and not in the calibre of the vessel. Thrombi, such as have been above described, may coexist with phlebitis; but they may exist without it; and, on the contrary, inflammation of the vascular walls “may unquestionably exist when the current of blood within the vessels of the affected part is perfectly free and unobstructed.” (p. 199.)

The thrombus frequently is limited to the vessel in which it originates, extending as far as the next larger trunk into which the involved vessel empties; but very often layer after layer of coagulum is deposited upon its extremity, encroaching upon the larger trunk, so that “the thrombus is prolonged beyond the mouth of the branch into the trunk, in the direction of the current of the blood.” (p. 205.) Portions of this prolonged thrombus, detached by the action of the blood-stream of the vessel into which it projects, and hurried away into the torrent of the circulation, constitute *emboli*.

An embolus, or floating coagulum, thus set free will, if formed in one of the veins of the extremities, be readily carried along from vein to vein till it reaches the heart, and, thence proceeding to the lungs, will be arrested, in accordance with its size, in the larger or smaller branches of the pulmonary artery. Similar emboli produced in the branches of mesenteric and other abdominal veins, carried readily along to the portal vein, will be arrested in the liver. An embolus thus arrested in a small arterial twig, cuts off the nutritive supply from a certain portion of tissue; various changes may supervene, varying with the size of the vessel involved, the original condition of the thrombus from which the embolus, in any given case, was detached, &c. It is sufficient, however, for our present purposes to state that we have here one mode of the origin of those metastatic abscesses which have been supposed to denote purulent infection of the blood (*pyæmia*). Other metastases take their origin in endocarditis. “Ulceration takes place in one of the valves of the heart, not by means of the formation of pus, but in consequence of an acute or chronic softening; crumbling fragments of the surface of the valve are borne away by the stream of blood, and reach with it far distant points” (p. 208); as, for example, the spleen or the kidneys.

The idea that such metastases may originate in the introduction of *pus*, as such, into the blood, and its transportation from one point to another, is stoutly denied by Virchow; who, after showing the difficulties in the way of an absorption of pus, as such, asserts that the arguments in favour of the existence of a morphological *pyæmia* will not withstand even *gentle* criticism. Nevertheless, however, he admits another form of metastasis besides that depending on embolism; that, to wit, in which, from the primary seat of the disease, “corrupted ichorous juices,” unrecognizable by our microscopes or by our chemical tests, enter the stream of the circulation and seize upon the parts for which, like certain medicines and poisons, they have a special affinity.

“To this class belongs that metastatic pleurisy which develops itself without any metastatic abscesses in the lungs; that seemingly rheumatic articular affection in which no distinct deposit is found in the joints; that diffuse gangrenous inflammation of the subcutaneous connective tissue which cannot well be accounted for, unless we suppose a more chemical mode of infection.” (p. 215.)

In plain language, besides the cases of metastases which can be accounted for by Virchow's doctrine of emboli—which is so beautiful and ingenious, because it is founded upon observation—we have another category of cases which cannot be accounted for satisfactorily by any *facts* at present in our possession, and in which if, for the present, with Virchow, we resort to *suppositions*, it must be with the distinct understanding that they are only temporary makeshifts, to be flung unceremoniously away so soon as our stock of facts becomes competent to supply us with better explanations.

In the foregoing account of some of the leading views of “the cellular pathology, we have purposely abstained from comment; our intention being rather to present these views to the notice of our readers than to enter into a discussion of their merits. Into such a discussion, we are far from being inclined to enter at the present time, although we have no hesitation in stating that the majority of the statements of facts accord perfectly with our own pathological observations; that certain experimental researches undertaken by us to test the accuracy of some of the statements, with which we were not familiar, have borne out these statements in the fullest manner; and that we regard most of the doctrines presented in these lectures as affording the best solutions of the several questions discussed which are possible in the actual state of our knowledge upon these subjects.

It is with these feelings that we especially commend these lectures to the study of the medical profession in America. Criticize them, if you will, in accordance with the facts in your possession; criticize them as to either the statements presented, or the logic of the doctrines deduced; but, at least, read them, and think of them. Like the medical doctrines which have preceded them, many of them will no doubt be modified hereafter by the observation of new facts hitherto unnoticed or overlooked.

But such a possibility, even if it could be speedily realized, would not impair the merits of this work. In a developing science, in which new observations are multiplying every day, and which is still in its infancy, to hope that a *theory may last, is to hope that progress may cease*. It was well said by the acute Raspail, to whom reference was made at the beginning of this article, that the best theory is not that which lasts longest, but that which leads most quickly to its substitution by a better; “that which engenders most quickly, and for it, as for certain insects, to engender is to perish.”

Tried by this standard, Virchow's book will not be found wanting; already it is bringing forth fruit. In Germany, it has become the stand-point of a host of investigations, and of the most various, friendly, and adverse criticism; and in France and England, a similar effect is beginning to be observed. Everywhere it is producing warm friends or eager enemies, and the discussions of these cannot remain barren.

In conclusion, we may especially commend the English translation, of which the quotations in this article were made. Comparing it critically with the original, we see at once that it is what might be called a free translation, but the author's meaning appears everywhere faithfully rendered, as, in fact, is insured by the circumstance that the translation has been revised by him. The numerous wood-cuts are fac-similes of the originals, and quite equal to them.

J. J. W.

ART. XV.—*Un mot sur la Fièvre jaune de Lisbonne en 1857.* Par M. le Docteur GUYON. Paris, 1858.

Report on the Pathology, Therapeutics, and General Aitiology of the Epidemic of Yellow Fever which prevailed at Lisbon during the latter half of the year 1857. (Parliamentary Report.) By Dr. ROBERT D. LYONS. London, 1859. Folio.

THE history of all extensive and fatal epidemics is a subject of interest to medical inquirers. Among the diseases which have furnished examples of the most devastating visitations, in this and other countries, the yellow fever deservedly occupies a pre-eminent position. In respect, indeed, to the number attacked, and to the mortality occurring among these, it has been justly doubted whether any disease, the black plague of the fifteenth century and the Asiatic cholera in our days perhaps excepted, can be compared with it. The oriental pestilence itself, as remarked by a late writer, though it occasioned, a century or two ago, a frightful loss of life throughout many parts of Europe, and though it has given rise, up to this day, at each epidemic return, to a mortality equally large, in proportion to the number of individuals attacked, did not, at the time of its widest diffusion, or, if it did, has long ceased to, produce as great a loss as has resulted from the disease in question. It is and has ever been more circumscribed in the sphere of its prevalence, and has appeared less frequently, even in its more legitimate localities.

To the physicians of the United States the yellow fever constitutes an object of special interest, inasmuch as in some of the cities and towns of the middle section of this country it has often prevailed, and to this day continues occasionally to prevail, with wide-spread mortality; while in many more parts of our Southern and Southwestern States it almost annually shows itself in its most awful aspects.

Influenced by these considerations, we have long thought that we might, without fear of trespassing on the patience of our readers, offer a few facts gathered from the materials within our reach relative to the epidemic which prevailed at Lisbon during the autumn of 1857. Until very lately these materials were very scanty—much more so, at least, than could be satisfactory to any one desirous to form correct notions relative to the origin, mode of propagation, and nature of the disease; for little is to be gleaned from the French and English medical journals we have perused, and we have not as yet been fortunate enough to procure any publication issued by the physicians or public authorities of the ill-fated city. Meagre as they were, however, they appeared to us, in the absence of more ample and authentic documents, to possess sufficient value, in view of the importance of the subject, to justify their being embodied in the form of a short essay, and laid before our readers. To our great satisfaction, Dr. Guyon, in the small volume, and Dr. Lyons, in the very able report, the titles of which we have placed at the head of this article, have in most respects filled up the deficiency here referred to, and we gladly avail ourselves of their labours to enlarge and complete what we had originally intended to say on the subject.

The first of these writers, who holds a high position in the medical staff of the French army, resided a long while in Martinique, where he acquired considerable experience in the yellow fever. The series of experiments he instituted to decide the question of the contagiousness or non-contagious-

ness of that disease, as recorded by Dr. Lefort, is well known. In 1826 he published an essay, in answer to one issued by the last-named writer, on the employment of bleeding and bark in the treatment of the fever; and a few years later he produced another, on the medical topography of Gibraltar, and the origin of the epidemic of that city in 1828.

Dr. Lyons was appointed pathologist-in-chief of the British army in the Crimea, and has given, in his able report on the diseases which prevailed among the troops, the most satisfactory evidence of his qualifications to investigate all matters connected with epidemic occurrences such as those he encountered in Lisbon. Like Dr. Guyon, Dr. Lyons undertook a mission to that city to examine into the pathology and origin of the fever prevailing there, and embodied the result of his observations in the report before us, which is addressed, under date of June 1, 1858, to the President of the General Board of Health of London, and published by order of Parliament.

The city of Lisbon is built in the form of an amphitheatre along the margin of the river Tagus. It occupies an irregular group of hills, which extend to the north for a distance of from two to six miles. These hills attain, throughout various parts of the city and suburbs, a height of 330 to 400 feet above the level of the sea.

"On their inland aspect they fall with a more or less rapid inclination towards a depression which forms in part the valley of Alcantara, and which surrounds the city in the greater part of its extent. Beyond this depression the land rises with different elevations, and extending in the directions of northeast and northwest, is subdivided by the valley which passes from Carnide to Loures into two portions of unequal form and extent. One of these divisions lies to the east and northeast, and the other to the west and northwest of the city of Lisbon, being united near Carnide by a neck of high land upon which are divided the waters that flow to the streams of Alcantara and Odivellas respectively."—*Lyons*, p. 64.

Dr. Lyons states that the main hills upon which the city is built may, he believes, be regarded as not exceeding seven in number, though as many as ten or eleven have been enumerated; and if minor subdivisions of the greater and more salient elevations were to be counted, the number might be still further increased. They have a pretty uniform direction from south to north, occasionally deviating to northwest or north-northwest. They all rise with a more or less rapid gradient from a narrow strip of level bordering the Tagus with a serpentine curve, and probably gained from the river bed at no very remote epoch. The hills and intervening valleys have generally, from the above-named disposition, an exposure to the south, south-southeast, and, in some instances, east.

Dr. Lyons enumerates in detail, under the following heads, the causes which appear to operate most prejudicially against the sanitary state of Lisbon: 1. Defective water supply; 2. Defective sewage; 3. State of shore of Tagus; 4. State of uncleanness of streets in certain quarters; 5. Defective construction of houses, as regards sewage, ventilation, &c.; 6. Existence of general slaughter-houses, tanneries, &c.

1. A large part of the city, as we are told, depends for water supply on the aqueduct of Don John V., a very noble work, completed in the year 1738, which withstood the earthquake of 1755, and is well preserved and kept with great attention to cleanliness. The water it delivers is of fair quality; no record of its temperature exists, and Dr. Lyons is not aware of its having been submitted to accurate chemical analysis. Through about a mile and a half of the extent of the aqueduct, which he examined, the water is perfectly free from organic particles, and not the smallest trace of vegetation is to be found in either of the stone channels through which it

flows. It precipitates in these channels an abundant deposit, which assumes a very clear structure in some places, with brilliant crystalline fracture. The water of this aqueduct is conveyed to numerous public fountains throughout various parts of the city, where it is allowed to run day and night; other fountains exist in two or three parts of the city, which derive their supply from the contiguous hills. In the flat of the city numerous wells exist, which give a supply of less pure water, chiefly used for domestic cleansing purposes.

From data furnished to Dr. Lyons, he is of opinion that the water delivered by the aqueduct may be estimated to be under 500,000 gallons per diem. Of the supplies obtained from the fountains fed by independent springs, from wells, and all other sources throughout the city, it is exceedingly difficult to obtain any accurate estimate. But supposing the water supply from all other sources to equal that from the aqueduct, we shall have 1,000,000 gallons of water per diem to meet all the wants personal, domestic, and public, of certainly little less than 250,000 inhabitants.

"The water supply here allowed is, I am persuaded, far above that actually available, yet for the population assumed, 250,000, and this number is probably below the mark, the allowance of water per head per diem is but four gallons." (The usual calculation allows twenty gallons per head per diem in cities but moderately well supplied with water.) "If, allowing for but a very small annual increase, we estimate the population of Lisbon at 300,000, and calculate the required water supply at the moderate average of twenty gallons per head per diem, we shall have 6,000,000 gallons per diem as the quantity necessary for daily consumption for all purposes, personal, domestic and public. This, compared with the actual supply from the aqueduct and all fountains and wells, taken at a maximum of 1,000,000 gallons per diem, shows that the daily supply of water to Lisbon is equal to but one-sixth of the quantity which would be required to place the population in conditions of salubrity as far as this important hygienic element is considered." (67.)

2. The sewers with which Lisbon is supplied may be considered under four heads: *a.* A system of great sewers. They are five in number, and were intended to drain certain quarters of the city. They open upon the shore of the Tagus. *b.* Besides these, three sewers of minor dimensions, not uniform in size or direction, draining smaller areas, and likewise opening on the Tagus. *c.* Lateral branching sewers, serving limited areas, and having communications with one or other of the systems of discharge into the Tagus first specified. *d.* House drains.

The following conclusions are stated as the result of the examinations of the sewers of the three first classes, as executed by the commission appointed for that purpose, as also by Dr. Lyons. The sewers of the third class were in numerous instances found choked with a foul black semi-fluid, and very noisome matter, giving out very fetid gases, which escaped through the ill-arranged wooden valves which these sewers present at several parts of their course. In certain states of the wind the gases were driven back through the sewers and through the streets to great distances. These gases likewise in some instances escaped through apertures in the foot-paths, and other parts of the streets, diffusing a very foul and pestiferous smell, perceptible at considerable distances.

In many instances, the embouchures of the second and even of the first class of sewers are now below the natural level of the part of the shore of the Tagus at which they disembogue, and in some cases the tide has carried quantities of mud and sand a considerable distance into the sewers.

In the great majority of instances, the embouchures of the sewers, when examined at low water, are found to discharge either no fluid at all, or but

an extremely small quantity of it. In some cases, the fluid so discharged had the appearance of pure water, was not discoloured, and was not in any case more than one or two inches in depth, and did not occupy more than half the bottom of the sewer as it flowed out. Dr. Lyons states that in but one instance was a sewer of large dimensions found to sensibly colour the tidal water at its embouchure, when examined during the rise of the tide and at and after high water. At no time did he observe any fluid, except a small quantity of blackish fluid, to flow from the great sewer which leads from the abattoir or slaughter-house and discharges near the custom-house.

"It is estimated that this sewer receives from the Abattoir above 220 gallons nearly of blood per diem, and at least three cwt. nearly, of boyanderie, animal offal, &c. It also receives, or ought to receive, the dejections and refuse of a quarter of the city, comprising at least 60,000 inhabitants—in all, probably not less than 50 tons per diem! What becomes of this enormous quantity of offal and dejecta? Not one-tenth part of it, by the most liberal calculation, finds its way to the embouchure of this sewer."

Deposits of foul black mud, emitting noisome gases, exist at points of some of the great sewers at various distances from their embouchures. A deposit of putrefying fish was found in one of the great sewers, by the "sewer-rat," the foul emanations of which struck his companion down senseless. The sewers of the third class in all probability form the permanent receptacles of the greater part of the dejections, refuse and offal of a population of not less than 250,000.

"Much of the fluid contents of this system of sewers is probably in a state of constant percolation into and through the adjacent soil, while the gaseous elements escape through the ill-arranged wooden valves and through numerous other apertures, and become diffused through the air."

Dr. Lyons states that the entire system of house drainage is defective, whether reference is made to the dwellings of the rich or of the poor. The water-closet system is all but unknown. It is, however, to the dwellings of the poorer classes that the present observations are meant to apply. In several houses which he examined, in quarters the most opposite to each other, throughout the city generally, the following system of house-drain was the sole and exclusive means Dr. Lyons found for removing the dejections and offals of the inhabitants.

"In the main front wall of the house a vertical shaft was carried, in the substance of the wall, from the highest story to the ground. In communication with this tube or shaft, which was usually rectangular and about six inches in mean section, a number of apertures were constructed on the internal face of the wall, which by a short oblique tube led into the main vertical shaft. One of the apertures, quite devoid of any grating, valve or other protection against the entry of refuse of any kind, however large or solid, was to be found on each landing of the staircase which corresponded to a floor. Into this aperture were thrown by the inhabitants of each floor the dejections, fluid or solid, and the refuse and offal of all kinds accumulating daily in a family.

"In some instances the vertical shaft communicated below with a drain or small sewer leading into the nearest street sewer. It constantly happened, however, that there was no such small drain or sewer, and even where present the drain had, in the majority of instances, become choked and obstructed. Either from the absence of a drain and from the drain being choked, as just described, it was a common condition of things to find accumulations of human dejections and refuse of the foulest kind, in greater or less abundance, poured into the open street; a greater or less heap of such filth, corresponding to the external aperture of the vertical shaft, on a level with the ground, and still more of it carried

by a partial flush of rain or other water towards a shallow, gutter-like depression existing in the centre of some streets.

"Supposing this state of things repeated for a number of houses on both sides of a street in thickly inhabited quarters, and with the super-addition of the effects of the *agoa vay* system, by which still further quantities of ordure thrown from the windows become accumulated in the street, and still assuming a continuance of dry and hot weather for a period of weeks together, narrow streets thus become converted into a kind of open gutter, with the decomposing ordure of a dense population exposed to the rays of the sun, and no attempt whatever made to remove such abominations, and some faint idea may be formed of what is a permanent condition of many crowded streets in some parts of Lisbon."

Well may Dr. Lyons conclude, from what precedes, that the whole system of sewage is defective and ineffectual, and that by far the greater part of the solid and liquid dejections, refuse, and offal, of a population of not less than 250,000 inhabitants, is thus allowed to remain in and about their dwellings, vitiating and infecting the atmosphere they breathe. This accumulation of foul abominations, he remarks, is perpetually increasing, and becoming, as it were, year by year, interstratified with the soil of the city. Gaseous exhalations from the whole system of sewers, but more especially those of the third order, are being perpetually given off, and mingle with the atmosphere. (70.)

3. The Tagus forms a bay, or lake-like expanse, some eight or ten miles in its longest diameter, opposite to Lisbon. Its waters are highly charged with a yellowish-brown silt. The stream is but moderately rapid—five to six miles per hour. The spring tides rise to a height of twelve or thirteen feet; the neap tides rise ten or eleven feet. The ebb leaves exposed to low water a considerable extent of shore in the neighbourhood of Lisbon, very foul, muddy, and exhaling mephitic gases, in various parts. From Belem to Apolonia, the extreme limits of the city, from west and east respectively, the distance is nearly five miles. Dr. Lyons says there can be but one opinion as to the neglected state in which the margin of the city is allowed to remain, and the highly prejudicial influence upon the public health exercised by its existing condition. The portion facing the most densely inhabited parts of the city is only partially supplied with well-built quays; in a large extent it is not so protected.

"Gas-works, cotton, and other factories, several markets and waste spaces, serving for the deposit of the city refuse, collected for sale and deportation by water, are placed immediately upon the shore, and here discharge their aggregated debris directly into the Tagus. Innumerable small streams work their way from the adjacent hills, through an open soil, to the river border; there is also, during low water, a continuous outflow of return sea-water, while the various sewers of the 1st and 2d classes, already described, open upon the same line. These combined causes have produced upon a large extent of this shore a deposit of soft, slimy, foul, and blackish mud, in part fecal, in great part composed of decaying vegetable matter, including minute wood fragments, in large abundance; to these elements are to be added—and it constitutes no inconsiderable item—quantities of decaying fish and fish offal. Heaps of fish entrails, and great numbers of the larger and smaller fish, for which there is no demand for sale or for consumption by the fishing population themselves, are daily cast upon the shore; these masses lie for hours, sweltering in the sun, and are but partially removed by each successive tide." (71.)

In one part of the shore the area left uncovered at low water is estimated at about 400 feet from shore to low water mark. The mud is stated to be extremely fetid, and constantly giving off sulphuretted hydrogen and other mephitic gases. It is a blackish, semi-fluid mud, highly charged with

decomposing animal and vegetable matters. Its depth is variously estimated at from about one foot to four and five yards.

The extent and rapidity of the river deposit is such, that it is found necessary to keep a dredging machine constantly in operation, to maintain the basin in the above condition of even moderate freedom from deposit. Without this, it would become in a short time completely filled up.

"The conditions of the shore of the Tagus here described constitute, undoubtedly," as Dr. Lyons adds very justly, "a source of permanent insalubrity, and must be highly prejudicial to the health of the population of the portions of the city in the vicinity of the river. The exhalations given off from the area of foul, putrescent mud, which borders a considerable extent of the city, and which lies exposed for hours to the rays of the sun, are carried by the wind, whenever it has any southing, into the upper parts of the town. No morass can be supposed, which, for purposes of insalubrity, could be made to occupy a more favourable position with regard to any city, for the effectual development and diffusion through a great mass of the population, of its pestiferous emanations, than this foul mud area which borders a large extent of the river margin of Lisbon."

4. It is stated that throughout a large portion of its superficies, Lisbon presents the appearance of a well-paved, well-flagged, and well-macadamized city; that the permanent way is well paved and well kept; and that the condition of the streets, public places, and even of the lanes and alleys in remote and backward quarters, is, in many respects, highly creditable to the authorities. But, it is added, the absence or obstructed state of sewers, the want of privies and of proper house drains, and the system of vertical shafts, before described, all combined with the incorrigible habits of the population, which seems to have never known, or to have completely forgotten, the privacy, delicacy, and retirement, almost universally observed elsewhere, in satisfying the calls of nature, have led to the conversion of the streets, lanes, and alleys, in whole quarters of the city, into the common receptacles for the human dejections, vegetable and animal garbage, and offal of all kinds, of large masses of the population.

"These foul abominations lie exposed to the air and sun till decomposed and trodden into the soil, or partially flushed away by heavy rain falls. Many of the streets of the quarters alluded to are thus converted into permanent lakes. It may be remarked here that the Koprological studies thus forced on the eye of the observer, in so many and such opposite quarters of the city, lead to the conclusion that a confined habit of body has become a constitutional state with a large proportion of the population, both male and female, young and old. Medical testimony unanimously bears out the observation." "The *agoua vay* (water goes) system, so-called from the caution to passers by, shouted from the windows in the nightly process of throwing into the streets the contents of certain domestic utensils, is now confined to the back streets and quarters."

5. Dr. Lyons is justly of opinion that the defects of house construction at Lisbon are important causes of insalubrity, and that the chief of these defects which bear upon our present purpose may be briefly classed as follows: A. The total absence or small and confined state of rear premises in all houses. B. The total absence or imperfect state of privies, sinks, and house drains. C. The want of thorough ventilation and front and rear aeration and lighting. This is especially obvious when, as in the great rectangular blocks of houses which form the superb-looking main streets of the flat of the city, the houses in parallel lines are applied nearly back to back, so that a very narrow interval between the rear of these very lofty five to six storied houses exists, totally inadequate for the purposes of ventilation. The main staircase of many of the finest and most massively built houses

in this and other quarters is pitch-dark at noonday at the first and second landings. A close, confined, and in some instances very foul smell prevails in many of these houses. These conditions are to be understood as occurring in the better class houses as well as in those of the poorer classes; they are, however, still further exaggerated in the case of the latter.

Dr. Lyons, from whom the foregoing statements are borrowed, remarks that Lisbon may be said to be throughout, even in the remoter quarters inhabited by the lower classes, a substantially and even massively built city. If the vices of construction above described were removed, and a good system of sewage and house drainage adopted, it would not be easy to suppose a population of equal extent so well housed, as far as regards the physical elements of their dwellings.

6. The last important cause of insalubrity mentioned by Dr. Lyons is to be found in the existence within the city of the public slaughter-house, tanneries, factories of various kinds, &c. &c. In the general slaughter-house or abattoir—

“The number of animals dispatched annually amounts to from 25,000 to 30,000 head of cattle. This number does not include the pigs annually consumed in Lisbon, which are killed at various places throughout the city and on the opposite shore of the Tagus. The following passage from a report of one of the commissions shows the state of the abattoir in a striking manner: ‘The exhalations of the soil (saturated with animal products) were so fetid that they could not be supported by the veterinary officer in charge. I,’ says the member of the commission especially charged with the inquiry, ‘no sooner received the first breath of them, than I was attacked with vomiting and headache. The vomiting ceased when I withdrew to a distance, but the headache continued until dinner-time.’”

To all this the author adds, that although many other causes of insalubrity could doubtless be enumerated, the foregoing are, in his opinion, the most effectively operative. It is, however, to be observed, that some causes of insalubrity met with so constantly in other cities are wanting in Lisbon; as, for instance, intra-mural interments. Three well-organized public cemeteries exist on the outskirts of the city.

Dr. Lyons enters fully upon the general and special climatology of Lisbon. On this subject we cannot, and indeed need not, offer more than a few remarks. He states at the outset that, according to Colonel Franzini’s excellent meteorological observations made since 1816, it would appear that at Lisbon the year should be divided as follows: The months of December, January, February, and March form the winter; the spring only lasts during the months of April and May; the true summer continues during June, July, August, and September; and autumn only includes the months of October and November.

The following general conclusions as to mean results, drawn from the observations of sixteen years of the meteorology of Lisbon between the years 1816 and 1840, are recorded by the above cited accomplished academician:—

Barometric Pressure of the Atmosphere.—The mean height of the barometer is stated to be 30.24 inches. In general, the barometer reaches its greatest elevation in the clear days of the winter season, when the prevailing winds are N. and N. E.; its least elevation is during the prevalence of the S. and S. W. winds.

Temperature.—The mean temperature deduced from Col. Franzini’s observations respecting the climate of Lisbon is 61° Fahrenheit. This, with slight variations, is the temperature of the month of October. Range from 26° to 105°.

Mean Winter Temperature.—The mean winter temperature, as deduced from the same observations, is 52°.1 (26°—85°).

Mean Spring Temperature.—The mean spring temperature is $60^{\circ}.5$ (37° — 89°).

Mean Summer Temperature.—The mean summer temperature is $70^{\circ}.4$ (48° — 105°).

Mean Autumn Temperature.—The mean autumn temperature is $59^{\circ}.5$ (34° — 83°).

Rain-fall.—The mean annual rain-fall, as given in Franzini's table, is stated at $24\frac{1}{2}$ (nearly) inches. It is elsewhere given as 23 (nearly) inches, distributed over 98 rainy days. Two remarkable extremes were observed— $10\frac{1}{3}$ inches in the driest year, and $36\frac{3}{4}$ inches in the most rainy year.

Rainy Days.—The rainy days, 98 in number, are found to be very unequally distributed. In general, the two months of autumn, October and November, and the first two months of winter, December and January, were the most rainy. June, July, August, and half of September were usually pretty dry.

Winds.—The N. wind prevailed on 144 days; the S. W. on 138 days; the N. W. on 124 days; and all the rest in due proportion. There were 129 days of wind more than usually strong, and 12 days of actual storm.

The foregoing observations may be summed up as follows:—

Barometer (mean) 30.24 inches.

Thermometer (mean) 61° Fahr. Winter (mean) $52^{\circ}.1$; range 26° — 85° . Spring (mean) $60^{\circ}.5$; range 37° — 89° . Summer (mean) $70^{\circ}.4$; range 48° — 105° . Autumn (mean) $59^{\circ}.5$; range 34° — 83° .

Rain-fall (mean) 23 inches (English); range $10\frac{1}{3}$ — $36\frac{3}{4}$ inches.

Rainy days (mean) 98.

Prevailing winds N. N. W. to S. W.

Cold days (mean) 57.

Hot days (mean) 58.

Windy days (mean) 83.

When we compare these results and those obtained in 1855 and 1856 with corresponding ones noticed in 1857, we find slight modifications in the latter year, not unlike those that have occasionally occurred elsewhere during epidemic seasons. The barometrical maxima of 1857 are below those of 1855 in every month except October, November, and December. The minima of 1857 are likewise below those of 1855, except in February, March, May, and December.

The maxima of 1857 are likewise less than those of 1856, except in January, April, June, and July; while, except in May, June, August, October, and December, the minima of 1857 are greater than those of 1856.

In regard to the monthly extremes, we find that the highest maxima were in December, 1856; December, 1857; January, 1855; May, 1855; January, 1857. The lowest minima were in January, 1856; in November, 1857; March, 1856; and February, 1856. In the table of barometric means the highest will be found to occur in December, 1857; June, 1855; January, 1855; July, 1855; and December, 1856. The lowest means were in January, 1856; November, 1857; May, 1857; August, 1857; and October, 1857. The means for March, May, June and August, 1857, are lower than the corresponding ones for both 1855 and 1856. The means of 1857 are greater than those of 1855 only in the months of February, October, November and December, and less in other months; they are greater than those of 1856, in January, April, July, September and December, less in the other months.

The greatest monthly variations were in January, 1856; February, 1855; November, 1857; December, 1856; March, 1856. The least

variations were in July, 1857; July, 1856; August, 1855; July, 1855; and September, 1855. The monthly variations in 1857 were less than those of 1855 and 1856, in February, March, April, July, September, October, and December.

In respect to the thermometric data of 1857, we find from a summary furnished by Dr. Lyons, that in February, March, April, May, July, and October, the maxima of 1857 exceed those of 1855 and 1856. In every instance except one, August, the maxima of 1857 are higher than those of 1855. From February to July, 1857, a steady increase in the maxima is observed; and with the exception of the maximum for June, 1857, which is below that of June, 1856, these maxima are in excess of those of the two antecedent years. It is to be remarked, however, that the maxima of 1857 are, with the exception of those of June, September, November and December, *below* those of 1855 and 1856. The highest maximum for the three years 1855, 1856, 1857, is found to have occurred in July, 1857, when the thermometer marked $99^{\circ}.50$ F.

The variations for February, March, April, May, July, October, and November, 1857, were remarkable; and with the exception of the latter month, in which the variations were considerably less than in 1857, the monthly range of the thermometer was much greater in 1857 than in 1855 and 1856.

In comparing a summary given of the monthly means of 1855, 1856, and 1857, we find an excess in those of April, July, September, October, and November, 1857, over the corresponding ones of 1855 and 1856. This excess, however, is very slight, except in the months of July and November, in which it is $1^{\circ}.61$ and $1^{\circ}.91$ respectively. The means of January, February, March, May, June, August, and December, 1857, are below those of the corresponding months of 1856; while in February, August, and December, 1857, they are below those of 1855. The lowest mean is that of January, 1855; the highest that of July, 1857. The means of the aggregate means for each of the three years 1855, 1856, and 1857, were $58^{\circ}.362$ F.; $60^{\circ}.792$ F.; and $60^{\circ}.180$ F., respectively.

It may be here remarked that the means of 1851 are said to have fallen below those of an antecedent period of 16 years, recorded by Franzini, in all the months except July, September, and November. Dr. Lyons, however, entertains great doubts as to the propriety of comparing the means of 1857, as deduced from the records of the royal observatory, with those of Franzini's 16 years period of observation. His reasons are founded on the higher position of the locality where the former were taken, its exposure to the northwest and east winds, its distance from the Tagus, and other influential circumstances, which have probably the effect of keeping the extreme and mean temperature there below the degree it attains in the proximity of the river, where the denseness of the population, the houses, fires, and above all the reflected heat of the sun thrown back from the amphitheatrically arranged group of hills which border that stream, must unquestionably raise the mean heat of both night and day. The objection would seem to be sustained by the fact that a table exhibiting the mean and mean maxima and mean minima for nine years, deduced from observation conducted by Dr. Martin, in a locality contiguous to the Tagus, to the centres of business and population, and to the main force of the epidemic of 1857—a locality similar to that in which Franzini is supposed by Dr. Lyons to have made his observations—shows that in every instance except one, July, the means of 1857, as recorded at the observatory, are below those of the nine years period in question. They exceed those of Franzini.

In conclusion, Dr. Lyons states that he regards Dr. Martin's observations as entirely reliable, and that they show an excess of temperature in the epidemic months of 1857.

The total fall of rain was less in 1857 than in 1856, and far less than in 1855. The quantity in the last mentioned year was 41.074 inches, the average quantity annually, being 23. In 1856 it was 34.706 inches, and in 1857 only 32.048. In 1855, during the seven last months of the year, the quantity that fell amounted to 497.8 millimetres, about 19 inches. In 1856, it did not exceed, during the same period, 188.1, $7\frac{1}{4}$ inches; while in 1857, during the epidemic months, it rose to 446.0, $17\frac{1}{8}$ inches. No great difference, therefore, presents itself, in that respect, between the yellow fever season of 1857, and the non-yellow fever of 1855; while in 1856, when the disease existed to a limited extent, the quantity of rain that fell was comparatively small. In 1857, the fall prior to June amounted to 367.8 millimetres. In 1856 to 690.6, and in 1855 to 545.5. From this comparison, we may perceive that, so far as the quantity of rain that fell is concerned, nothing can be made out respecting the cause of the severe epidemic of 1857, and of the lesser one of 1856. The same remark is applicable to the degree of the dew-point, elastic force of vapour, and weight of vapour, observed in the months of July, August, September, October, November, and December, of the above mentioned three years; for, on comparing the means of 1854, 1855, and 1856, with those for 1857, we shall obtain the following results for the latter year:—

“July.”—There was an elevation of the dew-point in July, when it reached a maximum, 60.25. The elastic force of vapour was likewise at a maximum in the same month. The weight of vapour to the cubic foot of air was below the maximum in this month, as 5.83 (1857) is to 6.005 (1855).

“August.”—The dew-point in this month was above the minimum (59), but was below the maximum, as 59.2 (1857) is to 60.75 (1854 and 1855). The elastic force of vapour was considerably above the minimum, .373 (1856), but below the recorded results .533 (1854), and .568 (1855), respectively. The weight of vapour to the cubic foot of air was above the minimum, 5.63 (1856), but below the recorded results 6.05 (1854), and 6.075 (1855).

“September.”—The dew-point was higher than in the two preceding years, having reached 60.5 (but not so high as in 1854, when it was 60.75). The elastic force of vapour, .534 was above a minimum, .328 (1856), but below the maximum, .557 (1854). The weight of vapour to the cubic foot of air was below the maximum, 6.08 (1854), but above that of the other years.

“October.”—The dew-point was at a minimum, 54.1. The elastic force of vapour .457, was above the minimum, .306 (1856), but below the results of the other years. The weight of vapour to the cubic foot of air was at a minimum, .508.

“November.”—The dew-point was at a very high maximum, 54.5. The elastic force of vapour was likewise at a maximum, .456. The weight of vapour to the cubic foot of air was also in excess.

“December.”—The dew-point 50, elastic force of vapour .411, and the weight of vapour to the cubic foot of air, 4.62, were all in excess over the similar elements of the corresponding months in the previous years.”

Dr. Lyons observes that, in 1857, the dew-point fell but 10.25 degrees from July to December, and that the minimum depression for the same period, in the three preceding years was 12.85.

From all that precedes, he properly concludes that the Lisbon epidemic of 1857 had no such necessary connection with a high dew-point, as would be made probable from the results obtained by inquiries into the history of other epidemics.

We may mention that the epidemic of 1857 was not the first from which
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the city of Lisbon suffered. We have satisfactory proofs that the disease appeared there and prevailed extensively, in the autumn of 1723, during the reign of King John the 5th. The epidemic is described in a Special Essay, by an eye-witness, Dr. S. F. Da Cunha,¹ and is mentioned by several other native writers—José Rodriguez de Avreu, physician of the king; Manuel de Silva Leitão;² Joa Mendes Sachetti, and Duarto Rebello Saldanha. Both de Avreu and Leitão were, like Da Cunha, contemporaries of the epidemic. The latter, after having prevailed during three months, ended on or about the 15th of October, and caused a mortality of six thousand. By Leitão and de Avreu it was designated by the name of *vomito preto*; while Da Cunha applies that denomination to one of the worst and most characteristic symptoms of the fever—the black vomit. The disease is also mentioned, in some detail, by Dr. Gilbert Kennedy, physician to the British Factory at Lisbon, in a letter addressed to Mr. Cayley (H. B. M. Consul), in answer to a communication requesting information on the subject.³ The fever is also alluded to in the Eulogium by Vicq-D'Azyr, of Dr. Rebeiro Sanchez, another eye-witness of its ravages.⁴

Of the yellow fever nature of the disease they encountered, there cannot be the least possibility of doubt. Independently of the fact that most of the authorities cited referred to the black vomit as one of its characteristic symptoms, the description of the complaint left by Dr. Kennedy, and his statement of some of the circumstances attending its development and progress, cannot permit us to admit the possibility of these being applicable to any other form of febrile disease. Let the reader judge for himself. Dr. K. remarks:—

“The heat last summer began late, but continued very violent, and much longer than usual, so that the grapes, which were more plentiful this year than many years past, were burnt up, when pretty ripe. All the summer was sickly, but about the middle of August there appeared the fever which now reigns, accompanied with a pain of the head and loins, a great sickness at the heart and stomach, with retchings to vomit, very contagious in the lower parts of the city, going generally through a family, and very few families escaping, especially in the close, narrow streets. The high parts are much freer than the lower parts, and the villages and country houses about town are entirely free from this distemper, notwithstanding the great communication. The recovery is generally accompanied with the yellow jaundice. The only mortal symptom in this epidemic is the vomiting of black choler.”

The stomach, on dissection, was found to contain a large quantity of this fluid. Leitão states, as a fact well known, that the number of persons who died that year, from the 15th of September to the 15th of December, was over 6000, “most deaths where most dirt,” where vapours arose by reason of heat, and the winds which prevailed were from the west; those who were most free were those who lived on the higher parts, and in the larger houses. (p. 43.) Of the same epidemic Da Cunha remarks that the summer of that year was very hot and dry, the autumn partaking of the same character, while the antecedent years likewise had been dry. As the result of this, the city of Lisbon became very sickly, in both its occidental and oriental divisions.

¹ Discurso e Observações Appolineas, Sobre as doenças que houve na cidade de Lisboa Occidental e Oriental O Outono de 1723. Lisboa, 1726.

² Arte com vida ou vida Comarte, per le Dr. Manuel de Silva Leitão. Lisboa, 1738.

³ Lyons, p. 110. See also Bancroft on the Yellow Fever, 436. The letter is preserved in the British Museum.

⁴ Hist. de la Soc. Roy. de Méd., iv. 215; note. See also the collected works of Vicq-D'Azyr, iii. 225; note.

"The diseases were accompanied with continued fever, pains in the head, and laxity of body, some presenting horripilations, others not, most of them having nausea, without vomiting anything. Some presented anxieties, and in others the superior region of the stomach was painful, so that they would not allow it to be touched. The disease was very acute. Most of the patients vomited black matter, and had dejections of the same kind, and they died on the third, fourth, and fifth days."

It would appear from the statements of de Avreu, that the yellow fever prevailed in 1721, in Ericeira, a village situated at a distance of seven leagues from Lisbon; and in 1728, at Penihe, another village one or two leagues nearer that city. The disease again prevailed, though less extensively, at Lisbon, in the months of August and September, 1724; and finally, from de Avreu we learn that it broke out, and assumed the epidemical form at Funchal, in the island of Madeira, in the year 1738. Some modern writers speak of an invasion and wide diffusion of the disease in Lisbon in 1736; but there is reason to believe, as Dr. Guyon—who has investigated the question—informs us, that the epidemic referred to that year is, in reality, the one described by Da Cunha, and others writers cited, as having occurred in 1723.

More certain is it that the fever prevailed in 1856 in the last-mentioned city, as also at Belem a populous town situated a few miles southeast of the former place. The visitation on this occasion was of somewhat over three months' duration—commencing on the 5th of September, and ending on the 28th of December. The disease presented the same characteristic features as it did the year after; but the yellow fever being unknown to the physicians of the place, the true nature of the cases they observed was not satisfactorily made out by them till the succeeding epidemic, when the phenomena of the prevailing complaint were recognized to appertain to the fever in question; and their similarity to those of the preceding season served to establish the true nature of these. To the end of October there were 100 cases at Belem, and 30 deaths; till the same date there were 63 cases and 16 deaths, and in Rua de la Bica (in the same period) 62 cases and 4 deaths.

In the month of November there were 86 cases and 21 deaths in the Rua St. Roque and Rua de la Bica; a hemorrhagic tendency was generally observed throughout the city. The disease commenced at Belem, in the court of the old custom-house, "and afterwards passed into the Rua de Embaixador. It is stated that the first case of yellow fever which occurred in St. Roque was in the person of a chocolatier (chocolate-maker), who died September 23d." The patient is said to have had no intercourse with persons coming from Oporto (in which place the fever raged), nor with the lazaretto, nor with the custom-house. On the day he sickened he had walked through the market. This patient resided in the Rua St. Roque, No. 60. The fever subsequently appeared in the house No. 57 of the same street, in which it attacked 16 persons.

The number of cases reported at Belem, St. Roque, and Bica, in 1856, amounted to 311, with 71 deaths, or 1 in 4.380.

A case of yellow fever, in a soldier, is reported as having occurred at Oporto, in 1856, as early as the 25th of July. From this date to the 10th of September 70 cases occurred in that city, of which 42 died. It was chiefly confined to the quarter Miragaia, remarkable for its filth and insalubrity.

The epidemic of 1857, like that of 1723, made its appearance during the first fortnight of August. Some cases, indeed, were noticed earlier. But,

as usually occurs under circumstances of the kind, the cases were at first few in number, and their nature was not fully ascertained. One case is recorded as having been verified so early as May; another was seen in July; while there appears to have been at least five in August. The existence of the epidemic was not officially announced till the 9th of September, when three persons were admitted into the hospital, and two deaths were reported. No return of cases occurring in domiciles is found till the 16th and 17th of September, on each of which days one case is recorded. There is then an interval of ten days, during which no return is made of cases in domicile. On the 27th of September 13 cases of the kind were reported, against 44 in hospital. On the 9th of October this proportion had become reversed, the number of those reported being—in hospital 106, in domicile 156. From the 9th of September the disease gradually spread, the cases daily increasing in number. Nevertheless, on the 2d of October the number received in the hospitals since the outbreak of the disease did not exceed 712 (609 males and 103 females). Of these 712 cases 194 died (167 males and 27 females), while 321 remained under treatment (266 males and 55 females).

The epidemic attained its culminating point of intensity, both as regards the number of cases and the mortality, during the second half of October—between the 19th and 28th. The largest number of cases reported, and the greatest loss of life, occurred on the 24th, when the former, both in hospital and private houses, reached the number of 298, and the deaths amounted to 173. On the 19th the mortality was 141, and on the 23d 152; while on the 26th it amounted to 122, on the 27th to 152, and on the 28th to 121.

From the period mentioned (24th) the fever lost its force, and closed its epidemic career about the end of December; though straggling cases continued to present themselves at a subsequent period, and some twenty occurred as late as the early days of March. But though the number of cases diminished in the way mentioned, the disease did not lose its malignancy in a corresponding degree; for the mortality in proportion to the attacks remained to the last much the same as it was at the outset of the epidemic. On the 17th of November the whole number of cases reported from the commencement amounted to 10,556. This number, though large, probably falls short of the real one, inasmuch as many cases may, as has occurred everywhere, have failed to be reported, either designedly or through ignorance, on the part of the medical attendants, of the true nature of the disease. The mortality during the same period is stated, with a greater likelihood to correctness, to have reached 3,550, or about 1 in 3. Dr. Lyons states that, according to the official returns, the totals of attacks, cures, and deaths, to the 25th of December, amounted to 13,481—5,652 in hospitals, and 7,830 in domiciles. Of these, 4,753 died, or 1 in 2.836. In September the cases were 662, and the deaths 190, or 1 in 3.484. In October, cases 6,531, deaths 2,077, or 1 in 3.149. In November, cases 5,327, deaths 2,046, or 1 in 2.603. And in December (to 25th), cases 961, deaths 440, or 1 in 2.184. According to M. Guyon, the whole number of cases, from the outset to the close of the epidemic, exclusive of those occurring among the military, amounted to 19,510. Of these, 5,834 were treated in the hospitals, the rest in private houses. Of the hospital cases, 4,718 were males, and 1,116 females. The whole mortality out of the 19,510 cases reached 6,859. That in the hospitals alone amounted to 2,063—1,679 males, and 384 females. The number of cases among the military at the Marianos Hospital, from the 22d of September to the 31st of December, is stated to have been 585, with a loss of 116. To this must be added 37 cases at the

hospital of Belem. Of these, 2 died; making a total of 622 cases and 118 deaths; being in the proportion of 1 death in 5.271 cases. The garrison of Lisbon is about 4,300, and of Belem 500, which gives us 1 case in 7.4, and 1 death in 40.67. Of British and Irish residents—the total number of whom is not known accurately—28 died, according to an official return to the consulate. By a private return made to Dr. Lyons, through the kindness of the heads of the British and Irish firms, he finds that 47 were attacked (including servants), of whom 20 died. Of the extent of the disease and mortality among the sailors no satisfactory information has as yet reached us. All we know is, that in the marine hospital 47 cases were treated from the 8th of September to the 30th of November, inclusive. Of these, 16 died, 27 recovered, and 4 remained under treatment at the date of the report. Independently of the cases and deaths here mentioned, many—not a few not registered, or, if registered, not officially reported—occurred among foreigners. What the exact number was we are not told. All we can gather on the subject is that of the French alone not less than 35 or 40 died; which, at the usual rates of the mortality, would give from 105 to 120 cases.

Now if, assuming as correct the returns of cases and deaths as given by Dr. Guyon, we sum up the whole of these various items, we shall find that the proportion of deaths to cases amounted to 1 in 2.85. The mortality varied at the several periods of the epidemic from 1 in 2 to 1 in 3. In the hospitals it was 1 in 2.88, and in private practice 1 in 2.65. The population of Lisbon being about 250,000, it follows that the mortality amounted to 1 in 36.42. If, however, we take into consideration that the number of persons who emigrated did not fall short of from 30,000 to 50,000, we discover that the loss in reality reached a much higher proportion.

It may not be improper to remark, in connection with the subject of the mortality occasioned by the yellow fever during this truly memorable epidemic, that many of the cases and deaths here ascribed to the yellow fever ought perhaps to be referred to intercurrent complaints. Dr. Guyon, from whose essay the foregoing statements are principally borrowed, would lead us to conclude that the mortality from the prevailing fever was less considerable than we have stated it to have been—the number mentioned having reference to the general loss of life in the city during the progress of the epidemic, and not to that occasioned by that fever alone. In a foot-note at page 20 he informs us that the entire mortality during the whole of the year 1857 was 11,752; and adds, that inasmuch as the mean loss of life during each of the fourteen years anterior to 1856 (1842–1855) was 6,985, it follows that the excess of mortality in 1857—4,767—must be regarded as representing the abnormal loss occasioned by the yellow fever. When, however, we take into consideration that whenever, in this and other countries, the fever assumes an aggravated epidemic form, and spreads widely, it takes the place of most other complaints which otherwise would figure in the mortuary list, or, complicating them, imparts to them a fatal tendency, we may reasonably infer that the same results must have obtained at Lisbon; and that while a certain portion of the deaths included in the amount mentioned above—6,857—may not have been the offspring of the prevailing fever, a large number of those supposed by Dr. Guyon to have been produced by intercurrent complaints were in reality due to the other, directly or indirectly.

The conclusion to which Dr. Lyons has arrived relative to the total mortality are somewhat different from those just mentioned. He states that the total recorded number of deaths from the epidemic amounted to 4,753; but remarks that the numbers here given can be accepted only as

an approximation to the true figures ; inasmuch as it is very doubtful whether in any epidemic the method of direct enumeration from hospital and other returns can be supposed capable of giving results free from large error, arising from the difficulty of determining an absolute diagnosis of all cases and the omission of many well-defined instances of the prevalent disease. Deducting from the 13,481 cases reported to the 25th December, 134 as the number of cases wrongfully reported as yellow fever, and adding 1,000 as the probable number of true cases omitted, he arrives at the conclusion that the probable amount of cases reached 14,347. After recalling the fact already mentioned, that the mortality for the year amounted to 11,752, and that the annual mortality being 6,985, the loss of life from the epidemic has been estimated at 4,767—this being the amount above the average—he remarks that this cannot be assumed as the total mortality of the epidemic, and quotes the following conclusion, drawn by Franzini :—

“The average mortality for the four months, September to December inclusive, is only 2,528 ; the mortality in those months of 1857 amounts to 7,874 ; the difference is 5,346, which may be fairly ascribed to the epidemic, and which if we include the burials from yellow fever cases in the British, German, and Jewish cemeteries, may be safely stated at 5,500 for the whole population. This may be taken as the safest approximation to the mortality from the epidemic.”

Dr. Lyons adds, that a very constant proportion of deaths to attacks, as 1 to 3, is observable at variable periods of the epidemic, from which Franzini deduces that the total of attacked must have reached from 16,000 to 17,000.

But whatever be the truth in this matter, the mortality which occurred on this occasion was certainly a very large one, and cannot fail to be a subject of sad remembrance to the inhabitants of the Portuguese metropolis. Considerable, however, as it must be admitted to be, Lisbon may yet console herself at having suffered less than other cities on this and the other side of the Atlantic. When we inquire, for example, into the sum of the loss of life in proportion to the entire population and to the number of the sick consequent on some of the epidemics of yellow fever which have visited this city, we shall find a confirmation of this statement. The three sickly seasons of 1793, 1797, and 1798, give us an average of one death in 14.24 of the entire population, and of one in 10.03 of those who remained. The mortality among those attacked, estimating it from the results recorded in each epidemic year, from 1793 downwards, varied from one in 1.2 (1819) to one in 3.86 (1805)—giving an average for all those epidemics of one in 2.12. In other parts of this country the extremes of the ratios has ranged from one in 1.5 to one in 13.1. Excluding the latter epidemics as being exceptional, from the unusual mildness of the disease—the average ratio of all the statements, including those relative to the mortality in public institutions, is one in 2.53—the extremes being one in 1.2 and one in 6. In Spain the disease has occasioned a still greater loss of life. The year 1804 one of great calamity there. On that occasion, twenty-five cities or towns were severely visited by the fever. The population in these amounted to 427,228, of which not less than 52,559, or one in 8.12, perished. In fourteen of those places, at different periods, the mortality in proportion to the population was one in 6.42, the extremes being one in 2.25 and one in 13.3. In seven places, the proportion of persons affected was one in 3.087, the extremes being one in 1.3 and one in 6.42 ; while the hospitals gave a mortality of one in 2.15 of the number admitted, with extremes of one in 1.1 and one in 3.82. The whole number of items noted in the table published by Dr. La Roche, from whose work on the yellow fever the foregoing

statements are borrowed, gives an average of one in 3.55, the extremes being one in 1.1 and one in 9.6.

If we now turn to the results obtained in tropical climates, we shall perceive that, laying aside a few instances when the disease would seem to have assumed an unusually mild character, or to have mostly or exclusively prevailed, as at Boa Vista, among individuals who, from constitutional peculiarities, or the effects of acclimatization, are little liable to the severe forms of fever; or when it has presented itself in a garb calculated to justify doubts as to its genuineness, the mortality there, though sometimes falling short of that experienced at Lisbon, more frequently has exceeded it. Omitting the instances of an exceptional kind referred to, the extremes will be found to range from one in 1.08 to one in 10—the average of all the ratios being one in 2.32. Take, in further illustration, the occurrences at Gorée and St. Louis, Senegal, in 1830. In the former place, 53 Europeans out of the 150 whites constituting the whole of the civil population, died. The cases amounted to no less than 144, being within only six of the entire number of white persons exposed. The deaths, therefore, were in the proportion of one in 2.83 of the population, and one in 2.7 of the sick. In St. Louis, 308 out of 650 died. Scarcely a single individual of the susceptible population escaped an attack. The deaths were in the proportion of one in 2.63 cases, and one in about the same number of inhabitants.

Of the nature of the disease of Lisbon in 1857, little need be said. That it was the true yellow fever there can be no doubt.

Dr. Lyons gives a full, and, as it would seem, a very accurate description of the disease. He remarks that the cases resolved themselves naturally into certain groups, more or less distinguished from each other by the presence or absence of well-marked and characteristic clinical features. This, we scarcely need say, occurs everywhere, and at all epidemic times. Among the groups thus distinguishable from each other, the following were, according to his observations, perhaps those best marked, and most readily recognizable: 1st. The Algid Form. 2d. The Sthenic Form. 3d. The Hemorrhagic Form. 4th. The Purpuric Form. 5th. The Typhous Form.

It will be unnecessary to follow Dr. Lyons in a detailed account of the disease, under these several heads, which embrace all the cases of it which present themselves, in greater or less number, in times of wide extended epidemics. We have only room for a few general remarks. Dr. Lyons states that, regarding the epidemic in question in a general way, with a view to the determination of its leading nosological features, it presented all the characters of a well-marked fever. The nervous system was oppressed, the circulation highly excited, while the secretions were diminished in quantity, altered, or suppressed. The temperature was elevated, the skin hot and dry to the touch, and the thermometer in the axillæ indicated an increase above the natural standard, frequently to the extent of two, four, and sometimes six degrees Fahrenheit.

The sensorial functions were but comparatively little affected on the whole, and it was common for patients to retain possession of their faculties till shortly before death. Exceptions, however, occurred to this rule, and cerebral excitement, with delirium, and other head symptoms, were observed in a certain number of cases. It was difficult, Dr. L. continues, to reduce the disease to any distinct type of febrile action; strictly speaking, it did not correspond to any of the regular types of continued, intermittent, or remittent fever; the phenomena were, undoubtedly, perfectly continuous in numerous cases, for days together. Intervals also occurred in which the

patient was devoid of pyrexial excitement of any kind, but these intervals were irregular in their periods of occurrence, and not by any means constant, and they were certainly wanting in any definite character of periodicity. In his judgment, the disease is best described as a fever composed of distinct stages or periods, the characters and the order of occurrence of which, though by no means constant and uniform, observed much regularity in the majority of cases.

At Lisbon, the most commonly prevalent form was the algid. Next came, in point of frequency, the hemorrhagic; next again, the sthenic, while the purpuric occurred in a limited number of cases, and the typhous only occasionally. The algid form was, as its equivalent is elsewhere, generally speaking, that which presented the most rapid course, the earliest and greatest amount of prostration of the vital powers, and which, likewise, offered in most frequent combination the most appalling characters of the epidemic re-united in individual cases. Dr. Lyons well observes there can be no doubt—

“That a great deal of the discrepancy of statement and conflict of opinion in the accounts we possess of the several epidemics of yellow fever, occurring in various localities, has arisen from the different forms which the disease presents being confounded in one common description, embracing the phenomena of all. It will, I think, be pretty evident that the description of an epidemic, on which the form I shall describe as the algid form predominated, must differ essentially from that of an epidemic in which the sthenic, the hemorrhagic, or the purpuric varieties were most generally presented. The same holds with respect to the typhoid form, which, though not a prevailing type in the Lisbon epidemic, must, when presented largely in any visitation of yellow fever, give a special character to it.”

Dr. Guyon, though not offering an elaborate description of the disease, calls attention to some points relative to which it varied more or less from the cases noticed somewhat frequently in this and other countries, not only during different periods of the same epidemic, but during different sickly seasons. Generally speaking, there was an absence, or less intenseness, during the first stage, of headache, and pain in the loins and limbs. The febrile symptoms were moderate. Indeed, many of the patients felt so slightly indisposed, as to be able to walk without assistance to an hospital. But whatever may have been the aspect of the disease, during the first stage, its characteristic features did not fail to manifest themselves at a subsequent period; while the diagnosis was confirmed by the post-mortem examination of those who perished. In relation to this point Dr. Lyons makes the following statement:—

“I am not aware of the occurrence during any portion of the Lisbon epidemic of 1857, of that class of rapidly fatal cases, vernacularly known elsewhere as ‘walking cases,’ and though rachialgia was a common symptom, I did not observe nor hear of its occurrence in that intense form accompanied with sudden prostration of the vital powers, and rapidly fatal issue, known so constantly in the older epidemics, as ‘coup de barre,’ or stroke of the bar, from the suddenness and violence of the attack of pain in the back or loins, and the ‘knock-down’ influence of the disease upon the patients from the first moment of invasion.”

We must not neglect to mention that similarly to what has not unfrequently occurred elsewhere, and perhaps more particularly during the last few years, the black vomit at Lisbon did not prove as invariably fatal in 1857, as it appears to have done in 1723, and, indeed, as it has been wont to do in many places, and during the greater number of epidemics. So far from this being the case, a number of recoveries occurred after a free

discharge of this dreaded matter from the stomach of both male and female patients.

Nothing satisfactory relative to the pathological anatomy of the disease can be gathered from the work of Dr. Guyon. We learn from it, however, that this important subject is far from having been neglected by the Lisbon physicians, and we may expect to receive, before long, the results of their researches. In the meanwhile, we may state as a fact worthy of the special notice of all American physicians, and for a knowledge of which we are indebted to a recent French writer,¹ that the dissections performed at Lisbon, revealed the existence of the fatty degeneration of the liver, a pathological condition first pointed out, so far as the yellow fever is concerned, by our distinguished countryman, Professor Clark, of New York.

But while Dr. Guyon has remained comparatively silent on the morbid changes left in the tissues and organs by the disease under consideration; and we know as yet little or nothing of the researches of the native physicians; Dr. Lyons's excellent report presents us with a most valuable record of his ample investigations in the matter. Nowhere, indeed, shall we find a more minute and comprehensive account of the pathological anatomy of the disease. It embraces a detail of the lesions occurring in the several tissues and organs implicated, primarily or secondarily, in the complaint—in their solids as well as in their fluids. We have not the space required to enable us to follow Dr. Lyons in these details, and to do full justice to his statements on the subjects involved in the inquiry. Nor, indeed, do we deem it necessary to enlarge on the subject, even were it in our power to do so without inconvenience, inasmuch as the lesions recorded in the work before us are, for the most part, similar to those noticed elsewhere in the same disease, and rather confirm what has been noticed by other investigators, than indicate the existence of pathological characters before unknown. On one point only shall we dwell somewhat at large, *i. e.*, the condition of the liver.

Dr. Lyons states that the most remarkable, the most constant, and, to his mind, the most inexplicable condition presented in the *post-mortem* examination of fatal yellow fever cases, was the state of that organ. He believes it may be affirmed that *some* departure from its normal state was an absolutely constant condition in all the cases which proved fatal. In relation to colour, firmness, consistence, dryness, the organ presented the characters recognized by all morbid anatomists, especially since the researches of Louis in the Gibraltar epidemic of 1828; while the microscopic examination of the hepatic tissue, which Dr. Lyons instituted, fully confirmed the results obtained by Dr. Clark, and subsequent pathological anatomists in this country.

"The fawn-yellow coloration," Dr. L. remarks, "usually well indicated the change that had taken place in the hepatic tissue; but it was not only in those cases in which the liver presented this coloration that abnormal states of the hepatic structure existed. The yellowish-brown colour, '*chocolat au lait*,' was attended with similar, and fully as well-marked changes. The same may be said of the nutmeg condition of the organ, and even to some extent of several cases in which the ordinary liver-brown colour was unchanged.

"Minute fine sections (by Valentin's double knife), or matter scraped from the hepatic texture, exhibited the hepatic cells filled with globular oily and fatty matter. The natural appearance of the cell was completely altered, its outlines obscured, and its nucleus rendered invisible. It was surcharged with molecular and globular oily matter, while the whole field, and the interspaces

¹ Dutraulau, *Topographie Médicale des Climats Intertropicaux*, p. 98, foot-note.

between the cells, were filled with similar and equally abundant oily and fatty elements. When carefully treated with ether, its oily and fatty elements were in part dissipated, and the contour of the cells brought more clearly into view, but it was only rarely that the nucleus could, even by long treatment in ether, be made visible.

"It was sufficiently obvious from these investigations, that the marked character of the hepatic lesion was that of fatty degeneration, with accumulations of fatty elements in the otherwise normal hepatic cells."

Feeling convinced that researches with the microscope, however carefully made, gave but imperfect and unsatisfactory determinations of the positive and comparative amount of fatty deposit in the hepatic tissue, Dr. Lyons determined to estimate the specific gravity of the organ in a good many cases, with a view to a more accurate appreciation of the amount of change thus induced; it being of course fairly presumable, as he thought, that in proportion of the amount of fatty accumulation in the hepatic structure the specific gravity of the organ would be found diminished accordingly.

"The general results were uniformly found to bear out this presumption. It was ascertained that in general the fawn-yellow colour of the liver corresponded with abundant granular and molecular fatty matter in the hepatic cells, as shown by microscopic examination, and with diminished specific gravity of the liver substance, as shown" "by the areometer of Beaumé. The results thus obtained were further confirmed by the determination of the absolute quantity of ethereous extract in given weights of hepatic substance in different cases."

"The mode of procedure was as follows: A very strong solution of common salt¹ was made in a glass vessel of suitable height. The areometer of Beaumé was allowed to float freely in this vessel. Small cubes of hepatic substance were cut from the central parts of the liver, and immersed in the fluid; if they sank an additional quantity of salt was carefully added; if they floated high upon the surface, water was gently poured into the solution. In either case, the precise moment was carefully observed when the cube of hepatic substance, after having assumed a position between floating and sinking, and having oscillated slightly up and down, seemed for an instant or two to be in equilibrium; the degree marked upon the areometer was then recorded as accurately as possible. But as the instrument marked only whole degrees, the readings of fractional parts of a degree could not be taken with all the accuracy desirable. In recording the observations, care was taken to allow the error of sight to be on the side of the greater rather than the less specific gravity."

Dr. Lyons next proceeded to determine the absolute quantity of fatty matter in the liver by maceration in sulphuric ether and subsequent evaporation. The results were generally conformable to what was already stated. They prove that with the yellow or buff-coloured, or even the chocolate-coloured state of the hepatic substance in the cases examined, there was found a considerable quantity of that matter, and that, in some instances, a very marked increase of the fatty element was determined. It has already been shown that at the microscope and the areometer a corresponding result was obtained.

Some examinations, both qualitative and quantitative, were made to determine the state of the saccharine function of the liver. The following general results were obtained:—

"In cases fatal within the first period of the disease, the hepatic tissue gave the characteristic saccharine reaction with the Barreswil fluid, cupro-potassic solution. In cases of longer standing, which had passed into the second or third period, the reaction with the Barreswil solution was extremely faint, and in some instances was altogether absent."

¹ This, though not the most suitable agent for estimating the specific gravity of animal tissues, was the only one readily available.

In some instances Dr. Lyons obtained slight but appreciable reaction with the liquor potassæ test, with the production of a faint but characteristic odour of caramel.

Similarly also to what has usually been observed in most places and in the greater number of epidemics, the fever, though not sparing any classes of individuals, manifested a special preference for persons of a strong and plethoric constitution and enjoying robust health. It occurred more generally and was more fatal among males than females, as well as among individuals in the prime of life; while it spared, comparatively at least, young children, old people, and valetudinarians. All this is conformable to what was observed in the same city in 1723, and has almost invariably occurred elsewhere. The state of marriage exercised an influence on the mortality. Of 3,486 unmarried persons 1,123 died, or 32.21 per cent. Amongst 1,099 married persons 498, or 45.31 per cent. died; and of 503 widowed (276 females), 263, or 52.43 per cent. died.

Of 1,192 vaccinated patients 351 died, or 29.44 per cent.; and of 2,308 non-vaccinated 894 died, or 38.73 per cent. Of 2,087 who had had variola 635, or 30.42 per cent. died; and of 1,279 who had not had variola 556, or 43.54 per cent. Unlike what appears to have happened during the last-mentioned epidemic when, as we are informed on the authority of Sanchez, the black race was completely spared, a goodly number of persons of that colour were attacked with the disease in 1857. This liability of negroes to the fever, though contrasting with the results observed in former times at Lisbon and in most other places, from the earliest records of the disease, cannot be regarded, at present particularly, as a subject of surprise, inasmuch as, independently of the circumstance that in extra-tropical climates, where natives or long residents are never secure unless through the effect of an attack, individuals of the black race, though suffering in a less proportion and with less fatality than whites, have seldom been found to pass unscathed through an epidemic of considerable violence. It has not unfrequently happened, indeed, that they have furnished no inconsiderable quota to the list of cases and deaths, even in places where they had heretofore been regarded as perfectly secure.

The disease spread widely and fatally among the upper classes of society, contrasting in this respect with what had taken place the year before in the same city in regard to the Asiatic cholera, which caused a mortality of more than 4,500 persons, principally, if not wholly, among the lower classes. Hence, while the latter disease received from the inhabitants the epithet of democratic, the yellow fever was held up as being aristocratic in its tendencies.

In the worst quarters of the city the mortality was 42 and 43 per cent., the mean mortality being about 33 per cent. The mean duration of the sojourn in hospital was 6 days; for those cured it was 8 days; a little longer for women than for men. In fatal cases the mean duration was 4 days; for men a little more.

The number of physicians and surgeons practising at Lisbon and its dependencies at the time of the epidemic amounted to near 240. Of these, according to one report, 14 died, or 1 in 17.14; and according to another 18, or 1 in 13.34. This loss, though heavy, is small in comparison with that often experienced elsewhere. In this city, where the physicians were not, any more than they could be at Lisbon, acclimatized to the disease, the mortality among them during some of our epidemics was much more considerable.

In 1793 the number of physicians, regular and irregular, enumerated in the

Directory as residing not only in the city, but in Southwark and the Northern Liberties, was 61. In 1797 it amounted to 68, and the next year to 75. In the first of these years, exclusive of medical students, no less than 10 physicians were swept off by the fever in little more than a month. We are further told that hardly one of those that survived or remained in the city escaped an attack. This gives us a proportion of about 1 in 6 of the whole number. In 1797, 9 fell victims to the disease, while of the survivors 8 were affected more or less severely. This gives a proportion of 1 death in 7.55, and 1 case in 4. New York, in 1798, lost 11 of her physicians out of probably some 30 or 40, of which the medical corps was then composed. The same year Philadelphia, more fortunate than she had been on preceding occasions, lost but 3, besides 2 medical students, or 1 in 25 of the whole number.

It must be remembered that while the number of physicians in the city and districts was such as mentioned, the actual number exposed to the disease was much more limited. We are told by Dr. Rush that at one period of the epidemic of 1793 there was a great deficiency of physicians, from the desertion of some and the sickness and death of others, and that at one time there were but three physicians who were able to do business out of their houses. To this may be added that many lived and practised at a distance from the infected districts, and probably never approached the sick. In 1797, as we again learn from Dr. Rush, the disease bore, not on 68 physicians, the number registered in the Directory, but on only three or four and twenty, this being the full amount of those who attended patients in the disease. The others had probably deserted; or kept aloof from the scene of infection.

In 1798 this city could boast, at the time of the epidemic, of only 28 physicians doing duty among the sick. The loss, therefore, comparatively limited as it was, gives a larger percentage than was noticed at Lisbon. At Martinique, in 1821, of 15 unacclimatized and unsecured physicians 10 perished. Four years after the loss was still greater; for 3 out of 4 doing duty among the troops fell victims to the disease. In Senegal, in 1830, all the navy surgeons suffered to a greater or less extent, and 6 out of 12 died.

The apothecaries suffered more extensively at Lisbon than the physicians. They numbered only 135, and yet sustained a loss of 13, or 1 in about 10.4. Their proportional loss was therefore nearly double that of the physicians; for had their number equalled that of the latter, the deaths among them would, in the same ratio, have reached 23.11.

The fever, as is usual in extra-tropical climates, commenced and was most prevalent in the lower parts of the city, and produced, as we have seen, its greatest ravages in those localities specified, and the streets particularly remarkable, for the abominations referred to. Spreading at first along the borders of the river at each side of the spot where it broke out, it subsequently extended to the upper parts of the city. Though passing, as it were, gradually from house to house, it often left, in its onward course, one or more intermediary buildings free from its presence. The same may be said of its mode of progression from street to street, as also from district to district. In other words, it respected one or more streets and districts situated between those attacked. In none of these instances was it possible to explain the cause of this exemption; the houses, streets, and districts which remained free from the infection presenting the same hygienic conditions and being placed under the same atmospheric influences as those in the vicinity. Several public institutions—the poor-house, the orphan and insane asylums,

the gas-works, &c.—though located in infected quarters, remained free from the epidemic visitation.

The disease, when it once broke out in a house, seldom failed to attack several of the inmates. As many as 14 and 18 persons were seized by it in a single domicile. When once a house became the seat of infection, it was rendered by the fact a focus of reproduction of the disease. It was dangerous to visit, and especially to lodge in it, whether or not it contained persons affected with the disease. The danger was especially great at night. In several instances it was observed that infected houses could be visited or resided in during the day with perfect impunity, while individuals who remained there at night were almost sure to suffer. The same danger from night exposure was exemplified in the city at large, after the disease had assumed a firm footing in it and become general. Persons who, residing in the country, came to town during the daytime, and returned home in the evening, seldom were affected. In this respect the yellow fever of Lisbon bore the closest analogy to the disease as it shows itself in this and other countries, in relation to which the danger of night exposure has invariably been signalized. Well, indeed, may it be said that on no subject connected with the etiology of the yellow fever are writers more unanimous than in attributing to night-air in an infected district a more baneful influence than to that of day. Of the large number of such writers, scarcely one can be found unprepared to admit that exposure to such districts by night is almost sure to be followed by an attack.

In another respect the disease presented a phenomenon similar to that observed, if not always, at least frequently, in this country and elsewhere—its inability to spread beyond the circumscribed sphere of the infection. Developed within the limits of the city, it was not conveyed by emigrants who had been exposed to the influence of the poison, or by the sick, to the environs or to the interior parts of the kingdom, although many of the former sickened and some died after their removal, or left the city already labouring under the premonitory symptoms of the fever. Such occurrences, common enough, as we all know, everywhere, had already been observed in Lisbon; for we learn from a statement by Kennedy and Leitão, relative to the epidemic of 1723, that the high parts of the city were much freer than the lower parts, and that the villages and country houses of the vicinity were entirely exempt from the distemper, notwithstanding the utmost communication was kept up between them all the time. Da Cunha makes similar statements as regards the localities most affected, adding that “it was only in the unclean places that there was this multitude of diseases.” Of the epidemic of 1724 kindred remarks are made by José Pinheiro de Freitas Soares.¹ “It was thought to have its origin in the corruption of the air by reason of the dirtiness of the streets, because the epidemic attacked chiefly the families that lived in the lowest parts of the city, where the streets were very narrow and dirty, and in the higher places it was only a few families that were attacked.”

Dr. Lyons notes the immunity enjoyed by the shipping in the Tagus, notwithstanding the great and most constant intercourse with the town. He knows of but one instance of a British seaman (master of a brig) having become a victim to the disease. The individual in question had been drinking on shore.

As is usually the case in extra-tropical regions, and as was observed in 1723, the epidemic of 1857 broke out after, and continued to spread at first during, the prevalence of very hot weather. A hot, humid atmosphere

¹ Policia Medeca, 1818, p. 349.

tended to increase its extension and virulence. The pressure of the atmosphere exercised an equal influence upon it in both respects. It increased with the depression of the barometer, while a change for the better was observed as the pressure diminished. But although the fever appeared under the influence of high atmospheric heat, it continued to prevail, more or less extensively, after the temperature had greatly lowered. On the 1st of December, some seven or eight weeks prior to the final cessation of the epidemic, the thermometer marked $13^{\circ}.6$ of Cent. ($56^{\circ}.4$ of Fahr.), and on the 31st 7° ($44^{\circ}.6$ Fahr.),¹ and yet cases occurred, though in diminished number, for some weeks after. Kindred results had already been noticed at Lisbon in 1723, and are referred to by Dr. Kennedy, who, in the letter several times quoted, mentions that the weather had changed several times to rain and cold during the course of the epidemic, without any abatement in the distemper. It may, however, be presumed that these changes, though sufficiently marked to be noticed, were not so great as to bring the thermometer down to the freezing point, an event which at the close of October never occurs at Lisbon. To those of our readers who have devoted some attention to the history of the disease, the fact just mentioned will not be a matter of astonishment, for this continued occurrence of cases, after a change of temperature has taken place, is frequently noticed. Thus, to speak only of the events in this country, it is a well-known fact that when our cities are visited epidemically with the yellow fever, the disease is not always particularly rife during the hottest months of the season, but some time after, when the average temperature has lowered in a notable degree; and that, so far from its diminishing as the weather becomes cooler, cases are often multiplied daily till the temperature reaches the freezing point, when the occurrence of new cases ceases at once. Take the epidemic of 1793 in this city as an example. The deaths in August, when the mean temperature at mid-day was $82^{\circ}.55$ ($69^{\circ}.03$ in the morning), amounted to 325; in September, with a mean heat of $76^{\circ}.48$ ($60^{\circ}.93$ in the morning), the mortality reached 1,442; and in October, with a temperature of only $62^{\circ}.57$ ($44^{\circ}.06$ in the morning), the deaths fell but little short of 2,000.

Cool weather, therefore, when the disease is fairly established, so far from diminishing its extension, sometimes increases it. These facts have been noticed not only here, but in our Southern States also, as well as at Barcelona in 1821, and in tropical climates.

While the yellow fever thus prevailed in Lisbon, and was exercising there its usual ravages, the country around was sorely afflicted with malarial fevers of the remittent and intermittent types. They extended their sway to the very gates of the city, and prevailed in all the localities situated along the Tagus, as far as Santarem. These fevers are of annual occurrence, in the same season, in these localities, but were uncommonly rife in 1857, in consequence of the large number of men collected there for the completion of the railroad connecting Lisbon with Santarem.

We do not find that anything of importance was done in relation to the treatment of the disease. At an early period of the epidemic many of the physicians resorted to the lancet, but the results were not such as to encourage them in persevering in the practice. It was, therefore, finally abandoned. More success appears to have been obtained from the use of stimulants, counter-irritants, purgatives, and the employment of special remedies, such as bark, iron, and more especially quinia. The majority of physicians, though differing relative to the precise time at which quinia

¹ If the $13^{\circ}.7$ and 7° indicate degrees in the scale of Reaumur, the equivalents in that of Fahrenheit are $62^{\circ}.6$ and $47^{\circ}.75$.

should be administered, thought highly of its effects. Dr. Lyons, in speaking of the remedy, says it was used both by the mouth and anus. Given by the mouth, it was prescribed in large doses; and in small doses it was used at the outset of the disease, and in all stages to the last. In the form of enema he saw it administered to the extent of seventy-two grains, divided into four clysters, one of which was given every sixth hour. From his experience of quinia, as administered in the epidemic, he is far from being disposed to regard it as a drug upon which any reliance can be placed when exhibited during the course of the disease. He is of opinion that the indications for its use as an antiperiodic are not very clearly manifested, and that its tonic action is too slow in a class of cases in which prompt support to the system and immediate stimulation are often so urgently called for. It would appear, however, from what Dr. Guyon observed while in Lisbon, that the remedy was found more useful when employed at the period of remission usually occurring in the morning during the first stage of the attack, than during the metaptosis, or that moment of calm constituting the link between the first and second stages of the disease, and which some pathologists wrongfully regard in the light of a true remission. It may be presumed, from what is here said of morning remissions during the first stage, and the benefit derived from this antiperiodic when administered at these periods, that the disease was generally modified through the influence of the paludal poison, which, as we have seen, was rife at the time, and must have penetrated, to a greater or less extent, to the very heart of the city.

Dr. Lyons informs us that the perchloride of iron was employed as an internal remedy, in doses of from three to six grains, two, three, four, or more times in the day, and, in some cases at least, with apparently good results, in arresting the hemorrhagic tendency. Chromic acid, in similar doses, was also used internally with the same view, but, as he thinks, with less advantage than the iron.

Dr. Guyon furnishes us no satisfactory information upon which to base an opinion respecting the probable origin of the fever and its mode of propagation. He himself hazards no conclusion, and postpones to some future time the publication of the facts he has collected on the subject. It would be unsafe, therefore, could we appeal to no other authority than his own, to arrive at a decision on the important points in question. Much to our gratification, Dr. Lyons has been less disposed to withhold the opinion he has formed on the subject. More particularly pleased are we to find that the occurrences in Lisbon go far to confirm us in the views we have long entertained respecting the local origin and non-contagious character of the yellow fever.

Dr. Lyons remarks that, from the first invasion of the epidemic, a belief became wide-spread and general, that the disease had been imported. This opinion was shared alike by persons in all ranks of society, by many amongst the educated, as well as the illiterate, and by many highly respectable members of the medical faculty, as well as by the public at large. It was not, however, universally accepted, for both in the faculty of medicine, and from several non-professional persons of great intelligence, the most distinct avowals of a belief in the entirely local origin of the epidemic were made, while certain existing conditions were pointed out, manifestly sufficient to account for its causation. As an impartial investigator, Dr. Lyons considered it to be his duty to inquire very fully into such evidence as was adduced in support of both sides of this important question. On this subject he remarks:—

"After a most careful inquiry amongst various official persons, and in quarters in which reliable evidence could be expected on such matters, I am obliged to state that in no one instance did I obtain such a consistent assemblage of facts, or such an array of well-supported allegations, as would, in my mind, warrant the conclusion that the importation theory was even moderately well founded. On the contrary, so vague, and in some essential particulars, so conflicting were the allegations as to the time, place, and other circumstances attending the reputed importation of the disease, that I do not believe that the popular opinion so generally held as to the importation of the epidemic admits of being reduced to anything like a uniform or consistent statement in writing.

"Of the reports in circulation as to the alleged importation, the two most generally received were, firstly, that the disease was communicated to persons in the custom-house, engaged in the examination of the baggage and personal effects of passengers arriving from the Brazils; secondly, that the manner of importation was by certain cargoes of hides from the Brazils. In connection with the first report, it is to be observed that there was no uniformity in the accounts given by different persons of the circumstances of time and place under which the importation was alleged to have occurred. There was no concurrence of testimony as to the particular ship, the date of her arrival, the port she sailed from, or the health of the passengers she brought. By one set of persons the ship specified was the Brazilian mail steamer Tamar; by others, a different ship was particularized with equal confidence.

"In one account of the importation of the disease, in connection with hides from the Brazils, it was stated that the hides were landed at the custom-house; in another, that the hides were landed at a certain wharf higher up the river. After the most careful inquiry in all quarters, I am unable to produce any more circumstantial account of the alleged importation of the malady than is contained in the foregoing statement.

"The considerable number of attacks and deaths which took place in the custom-house, and in the adjacent parts of the city, was constantly adduced in proof of the origin of the epidemic in the custom-house, and its propagation from that establishment as a centre." No such argument, however, "is supported by the facts. The undoubted frequency of the attacks, and the mortality in the quarter in question, were equalled and surpassed in districts considerably removed from proximity to the river."

But besides all this, many things—nearly all of which are adduced by Dr. Lyons—point to the conclusion that the epidemic in question was of local origin, and devoid of contagious properties. In the first place, we may remark that the epidemic which broke out at Lisbon in 1723, and carried off, as we have seen, 6000 persons, was never shown to have been introduced into that city from abroad. Nor was it held, at the time, by those competent to decide, as being propagated by contagion. We have seen that, according to Leitão, there were "most deaths where most dirt," when vapours arose by reason of heat, and the winds which prevailed were from the west. We have seen, too, that Soares says of the fever of 1724, that it was thought to have its origin in the corruption of the air, by reason of the dirtiness of the streets. Writing a few years later of what he had noticed on the memorable occasion of the epidemic of 1723, Da Cunha says: "What I always said, and which I persist in, is that diseases were not contagious, and they were all choleric diseases, caused by the heating and exalting of the cholera (bile), all occasioned by the inclemency of the weather." He admits that others were not of his opinion, and that various objections were raised to his views. But he adhered to these, and upheld the doctrine of local origin and non-contagion, by reasonings and a display of facts that would do no discredit to a modern infectionist, while his antagonists contented themselves with stringing together unsupported assertions, and could never point out the source of the alleged importation,

or a proof of transmission from the sick to the well. In this connection, the following passage is interesting.

"I remember that, being physician in Alfizerão and St. Martin, there was a lake between the two districts, which, when the summer came on, dried up, and part of which remained putrefied, and the winds coming, carried the vapours to both districts; and there were so many diseases, that scarcely one person escaped without falling sick, having myself 125 persons sick, and almost all with malignant diseases; many had parotitis and pinta. The winter coming on, the lake was inundated again, and all the diseases vanished; and the following year the lake opening itself, and what remained being burned, there were no more similar diseases."

In another place, after stating that it was only in the unclean localities that this multitude of cases occurred, he says: "The sun causing the more subtile particles to rise in vapours, which entering into the houses were introduced into the body, which was prepared to receive them by the rarefaction of its pores."

We are not unmindful of the fact that the epidemic in question was said by Sir Gilbert Blane to have been the result of importation from Brazil. But the mere assertion of that writer cannot be admitted as sufficient to establish the fact, inasmuch as he does not state where and from whom he obtained the information; and writing as he did, a full century after the event, he could not know much about it himself. A little reflection might perhaps have led him to refrain from making the statement, for the fever did not prevail in Brazil at the time, and could not very well be carried thence across the ocean. But importationists and contagionists of pure water, as Blane was, and as many others we could name are, have an easy way of getting over difficulties of the sort; and when we recollect that the epidemic of Cadiz, in 1819, was by some, otherwise clever men, traced in a way satisfactory to them, to a vessel which some months previously had arrived from Calcutta, where, as is well known, the yellow fever never shows itself; when we bear in mind that, to meet an objection raised against that theory, and founded on the invariable absence of the disease, at its supposed source of origin, it was contended that the Asiatic cholera, which *was* prevailing there at the time, might well be supposed to have been transformed by change of climate into the yellow fever; when, besides, we are gravely told that the epidemic of Agamont, in Spain, owed its origin to a piece of cheese which some fishermen had procured at sea from an English vessel of war on her passage from Gibraltar to England, which vessel they did not board, contained no sick, and did not communicate the fever at the port where she arrived; when we recollect these things, we say, we must be prepared for any statements by which physicians of the category mentioned may uphold their favourite theories. Let it be remarked, that nothing has been said of the foreign origin of the fever of Ericeira, in 1721; of Lisbon, in 1724; of Peniche, in 1728, and of Funchal, in 1738. Again, let it be remarked, that if the epidemic of 1857 is to be attributed to importation from abroad, the same explanation must be given relative to the fever which broke out, not only at Lisbon, but at Belem also, in 1856. Now, how did matters stand, in relation to that epidemic?

Of 311 cases recorded to have occurred in Lisbon in 1856, somewhat more than two-thirds were observed in districts far removed from the river margin. The three districts attacked in 1856, Belem, St. Roque, and Los Anjos (Bica) are widely separated from each other, and present no recognizable connection, commercial or otherwise, among themselves, nor with

the exterior by way of sea traffic. These districts are likewise on different elevations.

Let it be remarked, too, that the yellow fever of Oporto, in 1856, is attributed by Dr. Gonveia of that city to the filth of the quarter Miragaia, in which the disease prevailed, and not to importation; and no one has yet been able to disprove that opinion.

Nothing in all this looks as in any way favouring the opinion of the disease being of foreign origin. Add to it that, so far, no fact has been adduced likely to justify, in the most remote degree, a belief in this origin. No vessel or individual has been pointed out with any show of probability as being the agent of introduction of the disease. Are we not at liberty to say that until those several visitations are shown to have been the result of an imported infectious poison, the more violent and disastrous epidemic of 1857 may well be considered as being produced by local causes? Dr. Lyons calls attention to the fact that while there is no uniform testimony as to the time, place, and other circumstances of the alleged importation of the epidemic in 1856 and 1857, it is certain that no case of the disease was received into the lazaretto in either of those years, or in any recent year. (The Inspector of this place, who has resided there forty-two years, affirmed in the most positive manner that there has never been one single person of those undergoing quarantine who was attacked with an epidemic disease.) The fact of the fever not being communicated out of the city by emigrants in health, or by those who afterwards sickened and died in the country, certainly does not indicate the existence of contagion. On this subject the statements of Dr. Lyons are positive.

"The freest communication was kept up, even during the height of the epidemic, between Lisbon and Cintra, and many other favourite places of resort, to which the citizens retired in great numbers; yet no cases can be adduced to show that the disease spread or was carried by contagion or otherwise from Lisbon to such localities. Cintra was perhaps the place most frequented, and with which much free daily communication was consequently kept up; but I could obtain no reliable evidence that one single case of genuine yellow fever occurred in that town. I have heard statements," Dr. L. continues, "to the effect that the disease was propagated by contagion to some minor towns, but I have seen no evidence, documentary or otherwise, to prove the truth of such allegations, or render them even probable. On the contrary, the uniform testimony of popular report (so far as it is at all reliable or worth noticing) went to show that the disease was not propagated beyond the walls of Lisbon."

As already mentioned, no restriction seems to have been imposed to the intercourse between the shipping on the Tagus and the inhabitants of various parts of the city, or between the sick and well, wherever the former might be; and again, as we have seen, the inmates of many houses, though in close proximity to others containing persons labouring under the disease, and doubtless visiting these, remained free from its attacks throughout the whole duration of the calamity. The inhabitants of certain healthy streets or quarters of the city, although visited daily by individuals who had been exposed, by residence or otherwise, to the poison in infected localities, and whose clothes may well be supposed to have been impregnated with such poison—although themselves mixing, for traffic or otherwise, with the population of the sickly parts, remained free from the disease, or if they took it there, did not communicate it to their families, attendants, or visitors.

It is not customary for apothecaries to mix much with the sick. They, as a general rule, remain in their shops. While they do this, physicians, as a necessary consequence of the nature of the duties they are called upon to perform, approach and touch the sick, and are in every possible way exposed

to the baneful influence of contagious poisons, when such poisons exist. Nevertheless, as we have seen, apothecaries suffered in a far greater proportion than physicians. It may be remarked, in corroboration, that a class of females who, from their frequent and close personal intercourse with strangers and individuals exposed to and carrying about them the contagious poison—we allude to prostitutes—were scarcely touched by the disease. They numbered 600 at the time of the epidemic. Of these only 5 or 6 died, or 1 in 60. In addition, attention may be called to a fact already adverted to, that individuals from uninfected spots, or from the country, who visited less favoured localities, escaped the disease, provided they retired to their places of abode before night. They might enter and perambulate the city, for business or otherwise; they might step from their healthy streets and districts to those in which the fever raged with violence, and enter and remain for hours in houses actually occupied by the sick, or where deaths in greater or less number had but recently occurred; they might, indeed, visit the very chamber of the sick and dying; but they must do so before nightfall. At a later hour such exposures became dangerous, and those who slept all night in such places were almost sure to be seized with the disease.

It may not be improper to add, to all that has already been said, that instances of immunity from the so-called contagion were long ago observed at Lisbon. Kennedy states that at the time of his writing—31st of October, 1723—two months and a half after the outbreak of the fever, the latter had not yet entered any convent except the Irish, although the inmates of all those establishments never intermitted their attendance on the sick. We must also remind the reader that the same physician stated that during this epidemic of 1723 the villages and country-houses about town were entirely free from the disease, “notwithstanding the great communication.”

The reader must bear in mind that all the parts of the city largely attacked by the epidemic present in common certain conditions of insalubrity, which have been described in detail in an early part of this article; nor must we omit to mention that the state of the shore of the Tagus, for fully one-half of the extent of the city along the river-side, is such as to be a highly effective cause of insalubrity, not only to the adjacent districts, but to the whole capital. The vast area of extremely fetid decomposing mud left exposed at low water exhales noxious gases very prejudicial to health.

While in the epidemic of 1856 two out of the three localities chiefly attacked—St. Roque and Bica—were at some distance from the river, in 1857 the largest number of cases occurred in the first instance in the proximity of the custom-house, and in that establishment itself.

“No case,” says Dr. Lyons, “is recorded in this locality prior to September, 1857; but before this date there seems good ground for believing that cases of undoubted yellow fever had been observed. One case is recorded as having been verified so early as May, 1857. Another case is stated to have been seen in July, while there appear to have been at least five in August. Some of these cases occurred in the quarter St. Roque, an elevated district, 100 to 150 feet above the Tagus level.” “It has been attempted to give a special significance to certain of these cases, by saying that the parties attacked, though residents in the district of St. Roque, were employed in the custom-house. I can neither affirm nor positively deny the truth of the statement, as I could procure no reliable evidence respecting it, and I am not aware that any such exists. My own conviction, based on the results of inquiries made amongst medical men of all shades of opinion, is, that prior to the declared and fully recognized existence of the epidemic, isolated cases presented themselves in various parts of the city, of which no accurate record was made, but of the existence of which no moral doubt can be entertained.”

In conclusion, it may be remarked that although public opinion was strongly in favour of the importation theory, little if any apprehension of personal contagion was entertained by those in attendance upon and in daily contact with the sick. This indifference to exposure to the supposed contagion of the fever was observable in all classes of society, amongst the lowest as well as the highest. A large number of the inhabitants, it is true, sought safety by flight from the focus of the infection; but amongst the population which remained, "humanity was spared those humiliating and appalling scenes which the medical historian tells us were so constantly presented in the epidemics of the middle ages, of the sick and dying abandoned by their nearest relatives and friends."

R. La R..

ART. XVI.—*Chemistry, in its Relations to Physiology and Medicine.*

By GEORGE E. DAY, M. A., Cantab., M. D., F. R. S., Professor of Medicine in the University of St. Andrew's. London: H. Baillière, 1860. 8vo. pp. 518.

THE department of science to which the treatise before us relates has, through the labours of its numerous cultivators, become so intimately connected with rational practical medicine as to exercise a very material influence upon the views of those physicians who act in accordance with facts rather than fancies, and, as a consequence, has modified to a considerable extent the practical application of medical principles.

We need not stop now to discuss the opinion above enunciated, as it will, we think, be sufficiently established by what we have to say in detail of Dr. Day's volume; and yet we can scarcely refrain from calling to mind how the students of physiological chemistry have been reviled; how they have had to contend with many of those who, from their eminence and ability, should have opened their minds to the light of truth, but who, alas! have wilfully or ignorantly closed them against the mild persuasions of science. We cannot forget how many prejudices have been called into action against those who brought to the investigation of nature's laws the rigid tests of experimental analysis, and how even the sacred influence of religion was sought to be hurled against those who presumed to employ the principles of chemistry in their inquiries concerning the functional actions of the organism. We do not forget these things, and, remembering them, we find the greater cause for congratulation that, in spite of those obstacles which since the inauguration of the freedom of thought have always been opposed to the progress of truth, the science of physiological chemistry has steadily, though slowly, advanced till it has attained a recognized position among the sciences developed by the reason and the labour of man. True, there are still some who refuse to receive its teachings; there are still some who prefer the vague theories of the past to the facts of the present; but their name is *not* legion, and their voice is *not* mighty, and they grope confusedly in the darkness they love so well, vestiges of an age which, we trust, has passed away forever.

The treatise which we propose to bring before our readers in this notice is divided into three books. Of these the first treats of "*The Organic Substrata of the Animal Body; the Proximate Principles entering into the Composition of the Solids and Fluids of the Organism.*" Although it is essential, for the full understanding of the science of physiological

chemistry, that a full acquaintance should be had with the subject-matter which Dr. Day has incorporated into this division of his volume, we do not propose to dwell upon it now. We desire, however, to call attention to a few of the more notable substances which are treated of under the above head; and after having done so, will proceed to consider the more interesting, if not more important, subjects which follow.

In regard to the protein bodies, Dr. Day includes under this head albumen, fibrin, syntonin, casein, globulin, and hæmatocrystallin, as being all that are met with in the animal organism. These substances occur under two chief forms—in solution and coagulated, but are only met with in the animal body in the first-named condition. Most of them may be converted into the insoluble form by heat, by the mineral acids, or by saline solutions; and fibrin assumes this form on being removed with the blood in which it exists from the body.

It is frequently important that the physician should be able to determine the presence or absence of protein bodies; and in order to do this, Millon's test is more generally to be relied on, both on account of its delicacy and certainty. It consists in the employment of a solution of mercury made by dissolving one part of mercury in two of nitric acid of specific gravity 1.41. On the addition of this solution, and raising the temperature of the mixture to from 140° to 212° , an intense and persistent red colour is produced. The only source of error is that the same reaction is given with gelatin and its derivatives.

Dr. Day also refers to the iodine test so much used by histologists in microscopical examinations, but gives no particulars in regard to its preparation and employment. One essential point is that the solution of iodine should not be too strong, or the reaction may be so marked as to interfere with the obtaining of definite results. We have found a good preparation to consist of from two to four grains of iodine and about half the quantity of iodide of potassium to the ounce of water. If the tissue under microscopical examination is subjected to the influence of this fluid, by introducing the latter under the edge of the thin glass cover, a deep brown colour is produced, if the former be of a proteinaceous constitution.

Undoubtedly albumen is to be regarded as the type of the proteinaceous bodies; but, nevertheless, it is not identically the same substance, as it is met with in the white of egg, in the blood, &c. The cause of the modifications which exist would appear to be due to varying proportions of alkaline salts present; for, as it is found in animals, albumen is always in combination with these substances, and consequently never exists in a perfectly pure state.

In regard to the position of *fibrin*, Dr. Day expresses no decided opinion, but appears to regard it as belonging to the class of substances undergoing progressive metamorphosis.

Casein is not only found in milk, but also in the yolk of the egg. Its admixture with albumen was until quite recently regarded as a distinct proteinaceous substance, designated as *vitellin*.

Globulin or *crystallin* resembles albumen in many respects, but differs with it in not coagulating at a temperature lower than 163° , and in one or two other particulars. Globulin is only found in the crystalline lens. The coagulable matter of the red blood-corpuscles is not, as was maintained by Berzelius, identical with it.

Hæmato-crystallin, the proteinaceous substance of the blood above referred to, differs from all the other members of the group, in being crystallizable, and in not being precipitated from its solutions by nitrate of silver,

chloride of mercury, chloride of tin, or basic acetate of lead. Its formula has not been established.

The blood-crystals so generally met with in the blood of fish, and some of the mammals, birds, and reptiles, and which, with a little management, can be obtained from most red-blooded animals, consist of this substance or its modifications. The forms of these crystals are exceedingly various, but are constant for the same species of animals.

Lehmann has made the discovery that hæmato-crystallin may be broken up into a nitrogenous substance, and a sugar resembling glucose. This fact has recently been brought forward by several German pathologists in explanation of the pathology of chlorosis, and as an argument to the effect that sugar possesses an important curative influence over this affection. Nothing is known of the origin of hæmato-crystallin.

Passing over a good deal of other interesting matter contained in this division of the work, we come to the second part, which treats of the "*Chemistry of the Animal Juices and Tissues.*"

Dr. Day divides the fluids of the body into the five following groups:—

"I. The *digestive fluids*, including the saliva, gastric juice, bile, pancreatic fluid, and intestinal juice; and to these we have added the consideration of the intestinal contents.

"II. The *blood and its allies*, including the chyle, the lymph, the blood itself, and the various transudations.

"III. The *fluids connected with generation and development*, including the seminal fluid, the milk, and the fluids of the egg.

"IV. The *excretions of the mucous membrane and the skin*, including mucus, the various sebaceous matters, and the sweat.

"V. The *urine.*"

Under the first group the saliva is first considered; and after the chemical properties of the secretion from each gland separately, and of the mixed saliva, have been pointed out, the author takes up the question of their physiology. Dr. Day does not, we think, give sufficient prominence to the experiments of Bernard relative to the uses of the parotid, submaxillary, and sublingual saliva separately. Most of our readers are doubtless acquainted with the results which this physiologist obtained, showing that the secretion of the parotid gland is mainly of use in facilitating mastication, that of the submaxillary in assisting taste, and that of the sublingual in promoting deglutition. The influence of the saliva in causing the conversion of starch into sugar is affirmed by Dr. Day; and though there is no doubt that it does effect this change out of the body, and to some extent in the mouth, we doubt whether Dr. Day does not go too far when he says that it is almost universally admitted that the principal use of the salivary secretion is to promote the conversion of the amylaceous portion of the food into dextrine, sugar, and lactic acid, and thus to promote its absorption. This view is objected to by several of the most eminent physiologists of the world; and though there are some facts which seem to support it, there are others, again, which go to show that the power to effect the change in question is incidental, and of no practical importance whatever. The matter is not settled, and physiologists have still an ample field for research before the true action of the saliva in this respect can be ascertained.

The second section of this part is devoted to the consideration of the *gastric juice*. The chemistry of this secretion is first considered. In regard to the free acid, Dr. Day appears to regard it as being hydrochloric, frequently, but not always, associated with lactic. The view of Blondlot, that the acidity of the gastric juice is due to the presence of acid phosphate

of lime, is very properly regarded as untenable. In regard to this point, we think the mass of evidence sustains the opinion of the author, and that the essential acid of the gastric juice is the hydrochloric. We do not deny the general presence of lactic acid, but we think it is derived from the decomposition of the amylaceous and saccharine articles of food, and is not altogether a normal product in this situation.

Dr. Day's section on the *bile* contains all that is positively known on the subject. In regard to the uses of this secretion, he thinks it is mainly employed in the digestion of the fat. This, however, is not proved, and we are still at a loss to designate the exact purposes for which this fluid is required in the economy. It is certainly essential to life, but it is just as certainly unessential to perfect digestion.

In considering the *pancreatic juice*, no mention is made of Corvisart's investigations relative to the action of this fluid on nitrogenous substances. It would appear, also, that Dr. Day sides with the German physiologists in their views regarding its action on fatty matters, views which are in opposition to those held by Bernard.

Owing to the difficulty of obtaining the *intestinal juice* in a state of purity, it has not yet been satisfactorily analyzed. Enough, however, is known of it to warrant us in saying that it is invariably alkaline. The quantity secreted in a given time cannot be accurately ascertained. In its physiological action it resembles both the gastric juice and the pancreatic fluid—dissolving proteinaceous bodies, and converting starch into sugar. It does not possess the emulsifying power in so high a degree as the latter secretion.

Vomited matters, intestinal gases, and the *feces* are next brought under consideration, and the latest investigation relative to the composition, &c., of these substances is fully given. With reference to the latter, we find no mention made in the text of the very interesting observations of Dr. Marcet. In a note Dr. Day states that he has refrained from alluding to them because of their comparative incompleteness, and he merely gives some of Dr. Marcet's conclusions. If this is a valid objection, it is certainly applicable to many investigations, the details of which are not omitted in the volume under consideration.

In the next chapter the *blood and its allies* are dwelt upon at length, and the matter which Dr. Day has brought together under this head constitutes, in our opinion, one of the best portions of the book, though we cannot say that any information not already in possession of physiologists is communicated. The views expressed, however, are, in the main, such as are supported by the greatest body of evidence.

The physical characters of the blood are first considered, and then the morphotic constituents are brought under notice.

The existence of a cell-wall to the red corpuscles is affirmed by Dr. Day, and several arguments adduced in support of this view. It appears strange to us that some physiologists should deny so evident a fact. We do not find that any opinion is expressed relative to the origin of these bodies. We have for some time been of the opinion that they were a further stage in the development of the nuclei of the white corpuscles, a view first propounded by Wharton Jones. We have several times seen fully-formed red corpuscles contained in cells in the spleen with white corpuscles and free nuclei, and we believe that it is in this organ, and others of its class, that the change is mainly effected.

In regard to the colourless corpuscle, Dr. Day is of the opinion that it contains, besides its nucleus and granules, an albuminous solution. We

find that whenever there is an increase in the number of these bodies, there is an increase in the amount of fibrin in the blood. An intimate relation would therefore appear to exist between the two circumstances. We therefore are rather inclined to adopt the view which was held by Addison, that, instead of albumen, the white corpuscles contain fibrin, and that it is only found in this situation in the blood. There are several other facts which might be brought forward in support of this theory, not the least of which is that in the buffy coat of a clot the white corpuscles may be distinctly seen breaking up and giving exit to a fluid which immediately undergoes fibrillation.

Dr. Day does not in this chapter consider the question of the physiological position of the fibrin. A subject of so much importance could not but have received some elucidation from one of the known abilities and good judgment of our author. We regret, therefore, that he has not given his opinions in detail, and we do so the more from the fact that we are at present engaged in a series of investigations relative to this subject, and should be glad to have the benefit of his views. These investigations are as yet far from complete, but we think they authorize us in expressing the opinion that fibrin belongs to the class of substances produced by the retrograde metamorphosis of the tissues.

The blood is not the same fluid as it is met with in the arteries and veins, or even in the various special vessels of the body. The main difference between venous and arterial blood consists in the fact that the former contains less oxygen and more carbonic acid than the latter. It also, we think, contains more fibrin; at least our own researches lead us to this view, though it is one not held by all physiologists.

Dr. Day holds to the opinion that the menstrual blood contains no fibrin, a view certainly not correct. The reason why menstrual blood does not coagulate is simply owing to the fact that it is generally discharged slowly, and is so acted upon by the secretions of the uterus and vagina that coagulation is prevented. When, however, it is discharged rapidly, it invariably coagulates as does other blood.

The composition of the blood in disease is next noticed, and much matter interesting and important to the physician is given. If physicians would only act upon the facts which are here shown to exist, they would be enabled to treat many diseases much more intelligently than they are now managed. The diseases to which the blood is subject, and the modifications which it undergoes when other parts of the body are abnormally affected, seem scarcely to attract the attention of those who call themselves "practical men."

After considering the "allies" of the blood—the *chyle*, *lymph*, and *transudations*—Dr. Day passes to the *fluids connected with generation and development*. The other chapters of this section relate to *mucus*, the *urine*, *pus*, and the *solid tissues*. We are obliged to pass all these over with the simple remark that they are treated in a highly satisfactory manner, and that Dr. Day evinces how thoroughly he is acquainted with the subject considered.

The third book treats of the zoöchemical processes, those actions by which the organism is enabled to live and retain health; and these we propose to consider as briefly as is consistent with a fair detail of the principal facts which may be regarded as established, or supported by strong evidence.

Dr. Day, in the following classification, embraces those substances which are essential to the well-being of the organism, or we may say those substances which enter into the composition of the food of man:—

1. The protein bodies and their derivatives.

2. The fats (or hydrocarbons).
3. The carbo-hydrates—*e. g.* sugar, starch, &c.
4. The mineral constituents of the body.

In regard to this classification, we think it is as complete as any that can at present be made, and one that is far preferable to Liebig's, which, aside from its imperfect character, is altogether wrongly based. That above given is identical with one proposed by the author of this notice several years since. It commences the chapter devoted to the important subject of the *metamorphoses of the tissues*.

After considering the uses of albumen in the economy, and the transformations which it undergoes, Dr. Day passes to the second class—the fats; and as the views which are expressed in his remarks are those which are now generally held by physiologists, we quote a paragraph or two from the volume before us.

“The second great group includes the fats, whose various uses in the chemistry of the animal body have been already noticed. We have there shown that, in addition to their mechanical uses, they take an active part in the chemistry of digestion, and in all the processes by which the fluid nutrient substances are converted into tissues, and have likewise there and in other passages drawn attention to the fact that no animal cell or fibre is formed independently of the presence of fat. In connection with the importance of fat in the animal economy, we may adduce the well-known fact that the introduction of fat into the body (either as food or medicine) predisposes the organism to the formation of cells; a cell-formation of this kind, however, requires the concurrence of albuminous substances for the construction of the cell-walls. When the organism does not find in the food sufficient materials to form the investing membrane of the fat-cells, it borrows from the muscular fibre the necessary protein substance; and when this source can no longer be drawn from, the fat begins to accumulate in the blood and other fluids. From these facts, and from certain microscopic observations, a theory of cell-formation has been propounded, according to which each cell is primarily formed by the deposition of a thin layer around a minute vesicle of fat. We are not prepared fully to support this apparently simple explanation of the origin of a cell; but this at least is certain, that fat is always to be found in all highly cellular organs (as, for example, the brain and liver), and in all tissues, during the process of their development; pus and certain cancerous growths are rich in fat; the hair-bulbs present an active formation of new cells, and we find them imbedded in the sebaceous glands; the chyle, which always abounds in cells in various stages of formation, always contains much fat; the germ of the egg is surrounded by the fatty yolk-fluid; and numerous fat-globules are found in the muscular and other foetal tissues.” pp. 420–421.

We see, therefore, how much the experience of the present day is against the view of Liebig, that the fats serve only as heat-producing substances, and not as histogenetic materials. The fact is, that no such exclusive classification as that proposed by this physiologist can be based upon truth, as all substances taken as food contribute both to the formation of tissues and the maintenance of the heat of the organism.

The formation of fat in the system is next considered. The weight of evidence is decidedly in favour of the view that fat is not only formed from the carbo-hydrates (starch, sugar, &c.), but also, under certain circumstances, from albuminous bodies. Dr. Day next passes to the metamorphoses of the carbo-hydrates and the mineral substances (through the consideration of which we regret not being able to follow him), and then proceeds to the discussion of the function of *digestion*.

We find nothing new in this chapter, though it is an admirable *résumé* of what is known relative to the important function considered. Dr. Day doubts the correctness of Mialhe's view, that calomel is converted into cor-

rosive sublimate in the stomach through the action of the chlorides of the gastric juice. He bases his objections upon the experiments of Van Oettingen and Buckheim. The only way to definitely settle this question is by direct experiment upon animals with gastric fistulas. We have performed such experiments, and are enabled to say that corrosive sublimate is formed, if means are taken to obtain the secretion of a sufficiency of gastric juice. The availability of the various kinds of food is next considered, and then the function of *respiration* is brought under notice. The several facts which have been ascertained, and the various theories which have been propounded in reference to this process, are fully dwelt upon, and then the twentieth and last chapter, which is devoted to *nutrition*, is reached. In this chapter the nutritive value of the several articles of food is considered, and Dr. Day adduces several arguments showing the necessity which exists for a mixed diet. Another element, frequently overlooked, is the digestibility of the food. Thus, no matter how suitable an article may be, from its chemical composition, to subserve some of the purposes of food, if it is not capable of assimilation it is useless. For example, looking at the composition of gum, it might reasonably be concluded that it would form a valuable heat-producing material, and, from its ready solubility in water, that it would readily be absorbed from the gastro-intestinal mucous membrane; yet when experiments are instituted with reference to this point, it is at once perceived that the substance in question, from its indigestibility, is altogether incapable of acting in any sense as a food. Physicians, however, who disregard the teachings of physiology, and who look upon experiments as useless expenditures of time and labour, continue to injure their patients by giving them gum-water, under the idea that they are sustaining the failing powers of life, and lubricating the mucous coat of an inflamed gastro-intestinal surface. The chapter concludes with a statement of the influence of alcohol and tobacco upon the metamorphoses of tissue, in which the conclusions arrived at several years since by the author of this notice are given in full.

We must now bring our remarks on Dr. Day's treatise to a close. For the physician desirous of knowing what has been done by physiologists, during the last decade, to advance the science of medicine, we cannot recommend a better book than the one before us. Dr. Day is in a position to speak authoritatively upon all the subjects considered, for he has long devoted his attention to them, and few men have done more than he to extend a knowledge of the results which science has given to practical medicine. In perusing his writings, we never lose sight of his thorough honesty, and never fail to perceive how capable he is of giving a correct judgment. We would like to see his work widely distributed among the members of the profession, for we are confident that, were the facts which it contains brought home to the minds of physicians, we would have less empiricism, and might in time see the rise of that system of rational medicine, the advent of which is so much to be desired, but which is still, apparently, a great way off. That it will come, we do not doubt; and our confidence is increased when we reflect that those who labour in the cause are more than the equals, in ability, in eminence, and in numbers, to those who would force upon us the false and dangerous dogmas which spring from the effete systems of vitalism and solidism; and among those who have exerted themselves to break down the false and to erect the true, in the pure spirit of scientific zeal, we must assign a high place to the distinguished Professor of Medicine in the University of St. Andrew's.

W. A. H.

BIBLIOGRAPHICAL NOTICES.

ART. XVII.—*A Treatise on Human Physiology; designed for the Use of Students and Practitioners of Medicine.* By JOHN C. DALTON, Jr., M. D., Professor of Physiology and Microscopic Anatomy in the College of Physicians and Surgeons, &c. &c. Second edition, revised and enlarged. With two hundred and seventy-one illustrations. Philadelphia: Blanchard & Lea, 1861. 8vo. pp. 690.

UPON the appearance of the first edition of Dr. Dalton's treatise, two years ago, we reviewed it at length in our columns, pointing out what we considered to be its deficiencies, but dwelling at greater length, and certainly with far greater pleasure, on the many excellent features which characterized his work. We commended it for its originality, for its clearness, for the simple but elegant language in which it was written, for its beauty and fulness of illustration, and, with the exceptions which were mentioned, for its great adaptability to the wants of the student and practitioner.

Now, after the short period of two years, it comes to us again, "revised and enlarged;" and, with no intention of going over the ground which we formerly traversed, we enter with pleasure upon the congenial task of bringing before our readers the main points of Dr. Dalton's additions, and of again expressing our opinion of the volume which his learning as a physiologist and his ability as a teacher have enabled him to produce.

We cannot better indicate the character and extent of the additions which the author has made to his treatise, than by quoting the following extract from the preface to the second edition:—

"First, the introduction of an entire chapter devoted to the consideration of the *Special Senses*, which were only incidentally treated of in the former edition.

"Second, the rearrangement of the chapter on the *Cranial Nerves*, and the introduction of some new views and facts in regard to their physiology.

"Third, an account of some new experiments, original with the author, relating to the functions of the *Cerebellum*, and the conclusions to which they lead.

"Fourth, certain considerations respecting the general properties of *Sensation* and *Motion*, as resident in the nervous system, which are important as an introduction to the more detailed study of these functions.

"Fifth, the introduction of a chapter on *Imbibition* and *Exhalation*, and the functions of the *Lymphatic System*; including the study of endosmosis and exosmosis, and their mode of action in the animal frame; the experiments of Dutrochet, Chevreuil, Gosselin, Matteucci, and others on this subject; the constitution and circulation of the lymph and chyle; and, finally, a quantitative estimate of the entire processes of exudation and reabsorption, as taking place in the living body.

"Additions have also been made in various parts of the chapters on Secretion, Excretion, and Circulation, and the functions of the Digestive Apparatus. In every instance, these alterations have been incorporated with the text in such a manner as to avoid, as far as possible, increasing unnecessarily the size of the book.

"Twenty-two new and original illustrations have been introduced into the present volume; of which number, five replace others in the former edition which were regarded as imperfect, either in design or execution; the remaining seventeen are additional."

It will be seen, therefore, that Dr. Dalton's best efforts have been directed towards perfecting his work. The additions are marked by the same features which characterize the remainder of the volume and render it by far the most

desirable text-book on physiology to place in the hands of the student which, so far as we are aware, exists in the English language, or perhaps in any other. We use these strong expressions without, of course, intending to indorse all the opinions which Dr. Dalton expresses on physiological subjects. In fact, on the contrary, we find many where we are constrained to differ with him; but they are generally subjects which cannot be regarded as definitely settled, and where, therefore, a difference of opinion is allowable. We know no other treatise on physiology in regard to which we would not have to use similar language, and with more foundation for it than we have in the present instance.

We, therefore, have no hesitation in recommending Dr. Dalton's book to the classes for which it is intended, satisfied as we are that it is better adapted to their use than any other work of the kind to which they have access; and we trust that the speedy sale of the present edition will enable the author to keep the profession *au courant* with the rapid progress which physiological science is making, by the issuing of repeated editions of his excellent and satisfactory treatise.

W. A. H.

ART. XVIII.—*Medico-Chirurgical Transactions*. Published by the Royal Medical and Chirurgical Society of London. Volume the forty-third. London, 1860. 8vo. pp. 397.

THIS volume of *Transactions* contains thirty-five original communications. Ten of these communications, or abstracts of them, were published in the numbers of this Journal for last year, in the quarterly summaries of improvements and discoveries in the medical sciences. The following is an analysis of its contents:—

I. *History of an Additional Case of Tubal Gestation*. By ROBERT LEE, M. D.—The patient in this case was a woman, whose age is not given, who died suddenly from internal hemorrhage, one month after menstruation had taken place as usual. At the autopsy, three pints of blood were found in the sac of the peritoneum, that had escaped from an opening in the left Fallopian tube, where an ovum, containing a perfectly formed embryo, was discovered. Inclosing the chorion everywhere, and adhering to the inner surface of the tube, Dr. Lee found what he calls the *membrana decidua*. An opening existed in this membrane, which corresponded with the opening met with in the Fallopian tube, through which the blood had poured into the peritoneal cavity.

On the occasion of a communication made by Dr. Lee to a former volume of *Transactions*, some reasons, which we deem sufficient, were given for differing with him in regard to the formation of the *membrana decidua*.¹ Those interested in the subject will also find some important information in the paper of Dr. Hicks, entitled "Remarks on two Cases of Extra-Uterine Fœtation," contained in the last volume of *Guy's Hospital Reports*, a notice of which will appear in the next number of this Journal.

This communication is accompanied by a plate representing the uterus and the right Fallopian tube, containing the ovum, which has been opened.

II. *On the Etiology and Treatment of Peritonitis*. By SAMUEL O. HABERSHON, M. D.—An abstract of this paper is published in this Journal for April, 1860.

It is stated simply in this abstract that, in 56 cases where the peritonitis was caused by perforation of the intestine, 15 were from continued or typhoid fever. It is interesting to know, moreover, that the fatal lesion took place at the lower part of the ilium, and generally about the twenty-first or twenty-second day of the fever. Several perforations were produced, or at least accelerated, by removal to the hospital. Five other cases are mentioned, in connection with fever as a cause of peritonitis, in two of which the perforation was not complete, the peritoneum being left; in a third case, the intestine had given way, but local

¹ See this Journal for July, 1859, p. 178.

abscess was the result rather than general acute peritonitis; in a fourth, chronic adhesions had formed; and in the fifth, described as a case of fever, vomicae were found in the lungs, and only very slight ulceration in the ilium.

III. *Notes of a Visit to the Leper Hospital at Granada.* By JOHN WEBSTER, M. D.—This communication contains a short but very interesting memoir of a visit to the ancient Leper Hospital at Granada, which hospital was founded by Queen Isabella, about three centuries and a half ago. The establishment, at the time of Dr. Webster's visit, contained 53 inmates, in whom the disease could be witnessed in all its stages; some individuals having but a slight eruption on the skin, while others formed an almost indescribable mass of bodily corruption.

As the result of his investigations, Dr. Webster deduces the following general inferences:—

1st. Leprosy chiefly affects the male sex, as it always has done heretofore.
2d. Every age is liable to its attacks, but mostly that after puberty and during manhood.

3d. The malady is not infectious, in the strict sense of that definition.

4th. It seems to be an endemic disease.

5th. Occurs only among the lower and badly fed ranks of society, at present.

6th. Residents on the sea-coast constitute its ordinary victims in Spain, as elsewhere.

7th. The principal apparent causes seem to be putrid, indigestible, and in-nutritious aliment; also indigence, filth, and occupying insalubrious dwellings in marshy, miasmatic soils, or where a humid, variable, marine atmosphere prevails.

8th. Leprosy may be communicated by inoculation, according to some authorities.

9th. It is incurable, when fully developed; hence, in the latter stages, all medical treatment proves unavailing.

10th and lastly. Although the disease seems nearly extinct in districts where leprosy formerly prevailed extensively, and has almost disappeared from every other European country, excepting Greece and Norway, it yet still exists to some extent throughout Spain, especially among poverty-stricken natives who live either upon or near the southern Mediterranean sea-shores of that peninsula.

IV. *Observations on Stertor, and on the varying Conditions upon which it is dependent; with the Treatment necessary to its Relief.* By ROBERT L. BOWLES.—An abstract of this communication is published in the number of this Journal for April, 1860. It is a communication of great interest, and at the same time of great practical importance. The influence of position is almost always strictly attended to in every surgical case; while, in medical cases, it is very generally overlooked. When there is paralysis of the muscles of the tongue and fauces, as in apoplexy, or from the action of chloroform, alcohol, etc., this neglect, as Mr. Bowles shows, shortens life, and even, at times, suddenly cuts it short.

A plate is attached to this communication containing two figures, representing the position of the tongue when the mouth is closed or open.

V. *On a Form of Secondary Syphilitic Inoculation.* By HENRY LEE.—In this communication, Mr. Lee gives a number of cases which, he believes, show that secondary syphilitic disease may be communicated, by contact, from one individual to another, and that one form in which it is so communicated bears a very strong resemblance to the primary indurated chancre. The reason why many secondary syphilitic affections are not capable of being communicated is owing, in his opinion, to the fact of their not yielding any fluid secretion.

A plate containing three figures accompanies this communication, showing the appearance of the inoculated sores and of the enlarged glands attending them.

VI. *On a New Method of Operating for the Radical Cure of Hernia.* By JOHN WOOD.—An abstract of this communication is published in the number of this Journal for July, 1860.

All methods of operating hitherto practised for the radical cure of hernia,

have been shown by experience to be more or less dangerous and more or less ineffectual. Of late years, the favourite method has been that of Wutzer—whose name, by the way, is invariably written *Wurser* in the present communication. This method, though rarely, if ever, followed by inflammation of the peritoneum—which is the great danger in all these operations—is generally conceded, at the present time, to be entirely unsatisfactory; the rupture re-descending either immediately on leaving off the instrument and assuming the erect posture, or after discontinuing the truss worn after the operation. The chief cause of the failure of Wutzer's operation is believed by Mr. Wood to be the inefficiency of the steps taken to cause adhesion of the surfaces of the posterior fold of the invaginated sac together and to the posterior wall of the hernial canal. By the method of operating which he recommends, as is seen in the above-named abstract, the abdominal opening and the inguinal canal are drawn together and compressed by ligatures introduced subcutaneously. In this way, the structures forming the opening through which the hernia protrudes, are agglomerated into a solid mass, not of new and adventitious tissue, but of the more permanent tendinous and fascial tissues.

Mr. Wood states that he has performed his operation in fifteen cases of hernia. In all, except two of them, the general symptoms were so slight as to excite little attention. In one case, there was copious suppuration and burrowing of matter between the muscles of the abdomen, with tympanitis, possibly associated, Mr. Wood admits, with some parietal peritonitis. In one of the cases, there was a return of the hernia three weeks or a month after the operation, from attempting to lift too great a weight *and without a truss*. This return is attributed to the imprudence of the patient, a sailor boy, in omitting to wear a truss. It is strange that this reason should be given by Mr. Wood, for on only the previous page we find it said that trusses with strong springs are most injurious after his operation, by favouring the absorption of the effusion within the canal; that the occasional and intermittent use of a weak instrument is, on the contrary, beneficial, by exciting contraction in the canal; and that the usual effect of a truss worn continuously after an operation for the radical cure, is to mask its failure. It would be generally understood from such statements, that a weak truss occasionally used could do good by its action on the structures about the canal, aiding them to contract; but that a strong, compressing instrument was not only unnecessary after his operation, but, on the contrary, even injurious. In attributing, therefore, the return of the rupture to the fact of no truss having been worn, Mr. Wood would seem to contradict his previous statements.

We confess that we can see nothing in the peculiarities of Mr. Wood's method of operating, or in the results he has attained therefrom, to induce us to change our opinion as to the dangers and the inefficiency of all operative procedures for the radical cure of hernia.

Two plates, containing nine figures, are attached to this communication, by which the understanding of Mr. Wood's peculiar mode of operating—which is certainly one attended with considerable difficulty—is greatly facilitated.

VII. *On the Nature of Ovarian Cysts which contain Teeth, Hair, and Fatty Matters.* By ROBERT LEE, M. D.—This communication, which contains the histories of several very curious cases never before published, and a reference to a number of others to be found scattered here and there in medical writings, is a valuable contribution to the obscure subject of which it treats.

A plate is attached for the purpose of illustrating one of the cases recorded in the paper.

VIII. *Case of Intra-Uterine Fracture, with Observations to show the Analogy between Fractures in Utero and Congenital Distortion.* By BERNARD E. BRODHURST.—A short abstract of this interesting paper is published in the number of this Journal for October, 1860.

The author has evidently been at very considerable pains in searching for cases recorded of intra-uterine fracture. It is, therefore, remarkable that he has entirely overlooked Malgaigne (*Traité des Frac.*, p. 30 *et seq.*), who has devoted several pages of his treatise, with his usual display of erudition and critical acumen, to their consideration. There are also several cases recorded in the

recent work of Dr. Hamilton, and the one related by him at page 38 would be a very valuable addition to Dr. Brodhurst's list. He has himself cited none where the connection between intra-uterine fracture and distortion is so well marked.

IX. *Case of Femoral Hernia which had descended beneath the Pectineal Portion of the Fascia Lata, and was partially covered by the Pectineus Muscle.* By JOHN ADAMS.—The patient in this case was a woman, 45 years of age, who, after labouring for several days under the ordinary symptoms of strangulated intestine without there being any symptom of hernia, but a fulness considerably to the inner side of the usual site of femoral hernia, was operated upon by Mr. Adams. Cutting down in the direction of this fulness, he found a hernial sac, which contained some fetid pus, a portion of dark omentum, and about an inch and a half of small omentum in a gangrenous condition. The woman died four days after the operation, and, at the autopsy, the hernia was seen to have descended through the femoral ring, not in the sheath of the vessels, but behind and somewhat internal to the femoral vein, making a deep excavation in the pectineus muscle.

X. *Case of Imperforate Arch of the Aorta in which the Root of the Aorta was Ruptured.* By THOMAS ALFRED BARKER, M. D.—This communication gives the history of a very curious case, where a young man, 24 years of age, after always having enjoyed excellent health, was suddenly attacked, while lacing his boot, with severe pain in the left side of his head, in the left arm, and in the lower half of the anterior part of the chest, accompanied with palpitation and a sense of suffocation. He died suddenly, nineteen days afterwards, and on examining the body the commencement of the aorta was found dilated and ruptured in two places, the ruptures opening into the cardiac cellular tissue subjacent to the pericardium. There was pericarditis, and extravasation of blood into the heart. Some blood was found in the cavity of the pericardium, that had escaped from a rent in its visceral layer, which had probably taken place immediately before death. About half an inch beyond the ductus arteriosus, there was complete congenital constriction of the aorta.

XI. *On the Treatment of Axillary Aneurism.* By JAMES SYME.—An abstract of this communication, together with a just and comprehensive criticism made upon it, by Mr. Erichsen, at the time of its reading before the Society, is published in the number of this Journal for July, 1860.

XII. *Report on the Condition of the Prostate in Old Age, founded on the Dissection of One Hundred Specimens in individuals over sixty years of age.* By JOHN COCKBURN MESSER, M. D.—A full abstract of this communication, together with the remarks made at the time of its reading before the Society by Mr. Henry Thompson, whose researches, already noticed in this Journal, are remarkably confirmed by those of Dr. Messer, is published in the number of this Journal for July, 1860.

XIII. *On the Condition of the Blood in Mania.* By W. CHARLES HOOD, M. D.—This communication, of which a short abstract was published in the July number of this Journal for the past year, is an important contribution to what is already known of the condition of the blood in various diseases.

XIV. *Cases of Cerebral Affection caused by Disease in the Region of the Nose and Eyes.* By HERMANN WEBER, M. D.—Cerebral affections caused by disease of the internal ear are not of rare occurrence, while they are very seldom seen to accompany diseases existing in other regions adjacent to the cavity of the cranium. The three cases recorded in this communication will, therefore, be found particularly interesting. In two of them, from the post-mortem examination, it was manifest that the disease within the cranium commenced with coagulation of the blood in the cavernous sinus, in consequence of the introduction of morbid substance from the original seat of the disease in the nose and the eye, through the ophthalmic vein.

XV. *On Congestion of the Heart and its Local Consequences.* By WILLIAM JENNER, M. D.—The object of this paper is to call attention to the occurrence

of congestion of the muscular tissue of the heart from mechanical impediment to the passage of the blood; to the induration, toughening, and thickening of the walls of the heart caused by this congestion; and to the influence which those changes of texture exercise, as predisposing causes, on the development of permanent dilatation of this organ.

XVI. *Case of Acute Caries of the Walls of the Tympanic Cavity, producing Ulceration of the Internal Carotid Artery.* By JOSEPH TOYNBEE.—The case related in this paper is interesting principally from its rarity; indeed, only one other similar case, so far as Mr. Toynbee's researches have enabled him to discover, has ever been recorded. Both cases terminated fatally, from hemorrhage.

A wood-cut is attached to this communication, representing the condition of the carious bone and the ulcerated internal carotid artery.

XVII. *On Diseases of the Kidney accompanied by Albuminuria, considered in relation to their Origin in change occurring in the Tubes or in the Inter-tubular Structure.* By W. H. DICKINSON, M. B.—The peculiar views of Mr. Dickinson, in regard to the capability of arranging into two great classes those morbid conditions of the kidney called after the name of Dr. Bright, may be learned from an *extract* published in this Journal in January, 1860, which extract is a good summing up of the views and arguments contained in the present communication.

Three plates, containing five figures, are added to this paper, to illustrate the different conditions of the kidney.

XVIII. *Glaucoma and its Treatment by Iridectomy.* By J. W. HULKE.—A full abstract of this communication is published in the number of this Journal for October, 1860.

XIX. *Contribution to the Study of Spirometry.* By T. GRAHAM BALFOUR, M. D.—This contribution to the study of spirometry is made up from the hospital books and records of the Grenadier Guards, where the height and "vital capacity" of each recruit are given, and where, also, his medical history is to be traced from the time of his joining the regiment. The number of recruits taken was 1126; the average time of service amounted to three years and five months.

The identity of the results of the well-known observations of Mr. Hutchison and those here examined by Dr. Balfour is very remarkable, and may fairly be deemed an evidence of their accuracy.

XX. *Inquiry into the Treatment of Congenital Imperforations of the Rectum by Operation, founded on an Analysis of One Hundred Cases, nine of which occurred in the Practice of the Author.* By T. B. CURLING.—This communication, of which an abstract is published in the number of this Journal for October, 1860, is the longest, and at the same time the most important, paper in the whole volume. Of the many congenital malformations to which the body is liable, there are few more fatal to the life of the infant, or more distressing to the parents, than those here considered. At the same time, until the appearance of the very recent publication of Dr. Bodenhamer, it was very difficult, without considerable research, to acquire information in regard to the operative measures to be undertaken for their relief. We venture to express the hope that the study of this paper of Mr. Curling, as also that of the work of Dr. Bodenhamer, may lead to a change in the doctrines taught by two of the most distinguished teachers of surgery in this country. Dr. Bigelow, of Harvard University, thinks it unnecessary to discuss the question of "eking out the life of a new-born baby by an artificial anus in the groin or back" (*Boston Medical and Surgical Journal*, vol. lvii. p. 512); and Dr. Gross declares that he is "struck with astonishment that any one possessed of the proper feelings of humanity, should seriously advocate a procedure so fraught with danger, and followed, if successful, by such disgusting consequences." (*A System of Surgery*, vol. ii. p. 766.) A further acquaintance with the subject, such as is now afforded to the profession by these recent publications, will show the propriety and safety of teaching doctrines more in consonance with the spirit of our profession and of our civilization.

XXI. *On the Relative Amounts of Sugar and Urea in the Urine in Diabetes Mellitus.* By SYDNEY RINGER.—An abstract of this communication is published in the number of this Journal for October, 1860.

XXII. *Tabular Statement of Seventy-two Cases of Hæmatemesis; with Remarks.* By C. HANDFIELD JONES, M. B.—Of these 72 cases of hæmatemesis collected by Mr. Jones, 53 were females, 19 males—a proportion of nearly 3 to 1.

The hemorrhage occurred before the age of 20 in 2 males and 3 females; from the age of 20 to 40 in 9 males and 36 females; after the age of 40 in 8 males and 14 females.

In 28 cases, indigestion is mentioned as occurring a month or longer prior to the hæmatemesis. In 12 cases, it is stated that indigestion was absent or very slight.

In 40 cases, the existence of gastric ulceration seems to be more or less certain. In 32 cases, its existence is more or less doubtful.

In 3 cases, the liver was in a state of cirrhosis, and in 1 in a state of acute yellow atrophy; in 1, the spleen was greatly enlarged and there was ascites; in 2, the presence of cancer in the stomach was highly probable; and in 5 cases, hæmatemesis had a distinct relation to the catamenial periods.

XXIII. *Account of a Fresh Dissection of a Preparation of Tubal Gestation, described by Dr. John Clarke, in the Transactions of a Society for the Improvement of Medical and Chirurgical Knowledge for 1793.* By T. HOLMES.—The chief point of interest in this communication is the determination, by a careful dissection, of the existence of a stout membrane, of a fibrous appearance, surrounding the chorion in the whole of its extent.

A plate is attached to this communication, representing the appearance of the uterus and Fallopian tube.

XXIV. *Cases of Obstruction of the Veins of the Lower Extremities causing Œdema of the Corresponding Limb, and occurring in Phthisical Patients.* By GEORGE CURSHAM, M. D.—The cases of venous obstruction related by Dr. Cursham are three in number, and he refers to several others that have been recorded by various authors. He believes the coagulation in the vein to be produced by the presence of pus in the blood. All the cases in which it was witnessed were in the last stages of phthisis, the lungs containing numerous suppurating surfaces, from which pus or some foreign matter might readily be conveyed into the circulation.

XXV. *Case of Recurrent Anæsthesia of almost the entire Surface of the Body, accompanied by partial Loss of Motor Power in all the Limbs, traceable to the effects upon the Spinal Nerves of Effused Products within the Spinal Canal.* By JOHN W. OGLE, M. D.—This case is one of peculiar interest on account of the close and intelligible dependence of the symptoms upon the lesion, as determined after death. The comprehensive title of the communication will serve to give a correct idea of the nature of the case. W. F. A.

ART. XIX.—*Transactions of the Epidemiological Society of London.* Vol. I., Part I. 8vo., pp. 136. London, 1860.

THE Epidemiological Society of London was organized in the Summer of 1850. Its chief object is, as its name imports, "the investigation of epidemical diseases;—the institution of a rigid examination into the causes and conditions which influence their origin, propagation, mitigation, prevention, and treatment;—the collection and promulgation, with relation to these subjects, of such facts as appear to be established on sound and sufficient evidence, and the indication of those methods of investigation by which the misleading influence of false or deficient facts may be avoided.

The society embraces among its members a large number of physicians and others interested in the leading object for which the society was instituted, and who, from their respective positions—their habits of careful observation, close investigation, and cautious induction, are well qualified to contribute valuable materials towards carrying out that object to a successful accomplishment.

Through the medical periodical press of Great Britain we have been made acquainted with the results of the first nine years of the Society's operations. Convinced that it is only by the publicity given to its transactions, that its efforts can be rendered generally useful—that the promotion through it of the public good can only be accomplished by imparting to those beyond the comparatively narrow circle of its members the facts which these have elicited, and the general conclusions to which they have been led by their observations—thus adding them promptly to the common stock of professional knowledge,—abstracts of papers read at its monthly meetings were furnished to the medical journals. It being found, however, that such abstracts furnished, at best, but a general idea of the materials elicited by the operation of the Society, and that the promotion of the good of the community and the enlightenment of the medical profession on subjects of the deepest interest as matters of scientific investigation, and of the utmost importance in their directly practical application, would be best and most certainly attained by the publication from time to time, as materials accumulated, of their transactions in a separate form, and under the immediate superintendence of a suitable committee; the Society accordingly determined upon such a course, and, as its first fruit, we have the volume which lies before us.

It opens with the annual address, delivered Nov. 7, 1859, of the President, Dr. Benjamin G. Babbington. The address presents a very neat but brief *resumé* of the transactions of the Society during its ninth session—with a general sketch of the principal epidemics which had occurred during the preceding twelve months in Great Britain, the British colonies, the United States, and other lands. The address is replete with interest—it is not one, however, adapted for analysis, while it presents no prominent points demanding especial comment.

The first of the scientific papers is on "the Theory of Zymosis," by Dr. B. W. Richardson. The author denies that the virus of diseases of the zymotic class has in the proper sense of the term any reproductive property or power, when conditions favourable to such reproduction are supplied. The view which he presents as explanatory of the propagation of all communicable diseases is, that "there is a step in the process, which consists in the development of an albuminous matter having the power of exciting chemical changes, or zymosis in the organism; and that there is a second process, consisting of a modification in the animal chemistry, in the formation, that is to say, of new and inorganic poisons, upon the presence of which the symptoms of the disease depend."

Dr. R. believes that in the production of communicable and some other diseases there really takes place such a process as zymosis, or fermentation, in the animal body. The views he offers in respect to such process are:—

"1. That the zymosis, whether occurring in the body or out of it, though manifested always by the agency of organic matter, is essentially a chemical process, leading to the resolution of complex organic compounds into simpler compounds. 2. That when the process occurs in the body, the new chemical products are the causes of the symptoms as well as of the pathology of the diseases excited. 3. That the virus of these diseases is purely incidental to a modified chemistry in the organism; is excrementitious; and is as necessarily an excrete under the modified conditions, as urea is necessarily an excrete in perfect health. That this excrete, as an albuminous product, possesses the power of exciting the zymosis in a person liable to such action, and is connected with symptoms, not by an immediate organic increase of its own parts, but by the establishment of a new series of chemical changes."

This view of zymosis Dr. R. believes will explain satisfactorily many points which, otherwise, are all confusion, and at the same time link certain diseases to the zymotic not now ranked on that list, while it will reduce the verbal list of the zymotic to a few units. It shows why diseases directly communicable

from person to person resemble other diseases which have a tendency to spread, but without being communicable. Thus, he remarks:—

“The poisons of phytogenic fever and of dissecting-room diarrhoea are not communicable poisons in the strict sense of the word. Yet they give origin to particular cases of disease which may be considered endemic, and by extension of cause, epidemic.

“A second explanation supplied by this theory relates to the quantity of poison required for the production of symptoms. It has often been a subject of wonder why so minute a portion of the virus of a disease, such as smallpox, excites such decided effects. Dr. Fordyce largely diluted the virus of the above named disease, hoping thereby to induce a milder form of the disorder by inoculation. He was mistaken in his hopes; for he found that the disease, if produced at all, was as severe, after extreme dilution of the virus as when the virus was directly used. The reason for this is sufficiently simple when we recall that, in zymosis out of the body, the external conditions and supply of matter susceptible of zymotic change being supplied, the quantity of ferment neither removes the fact of fermentation, nor modifies the process.”

The theory of zymosis advanced by him, according to Dr. R., suggests, also, an alliance between diseases arising from the absorption of an external poison and diseases which are produced by changes occurring in the body, independently of an exterior agency which could, with propriety, be designated as of a poisonous nature.

“In this sense,” he remarks, “acute rheumatism is a disease connected in a degree with what are called the zymotici. This disease in all its symptomatic phases, is due to the presence of a poison in the blood. We now, indeed, know the nature of this poison; we have an idea of the part of the system in which it is produced; and we are conversant of the mode by which it is eliminated. Now, if acute rheumatism could be excited by the inoculation into a healthy body of a virus which would start the formation in the blood of the acid poison on which the fibrous inflammation depends, then rheumatism would be palpably a zymotic disease. As the disease cannot be so produced directly, it has not been considered a zymotic, notwithstanding that its cause is a poison generated in the animal laboratory, and that it is itself one of the purest representatives of the zymotic class.”

Tetanus is another disease Dr. R. considers to belong to the zymotici.

“In this disease, the poison, in my opinion,” he writes, “is first developed in the wound, as a result of decomposition. Thence carried into the circulation, the new substance, without any necessary increase of its own parts, excites a zymosis, ending in the production of an alkaloid or alkaline body which has all the power of exciting the symptoms of spasm—as much the power as strychnine itself. Nay, in regard to this tetanic poison, we have, as I have pointed out in a previous paper, a clue to the nature of the poison. I showed then that all the specific tetanic symptoms could be induced by the introduction into the body of certain of that series of the ammonias into which carbon enters as a constituent element.

“In the next place, the theory propounded explains the reason for the fact why the majority of epidemic diseases are not fatal, if the patients in whom they occur are placed in favourable conditions for recovery. We have seen that, if the hypothesis of the constant reproduction of an organic poison in the body, and of the presence of this poison as the cause of the symptoms, were tenable, the process of reproduction of the poison should continue so long as the patient could supply the means for the continuance of the process, that is to say, until death itself. When, however, we take the chemical view of the question, when we consider that the organic virus primarily introduced is not immediately propagated, but induces chemical changes upon which the symptoms of the disease depend, we see a reason why the epidemics run a limited course; since chemical changes, having no independent continuance when their causes are removed, necessarily cease after a time, together with the symptoms which have been excited by them during the period they were in operation.

“If,” Dr. R. remarks, in conclusion, “there is any truth in the theory of zymosis in the body, its principles or laws are at hand, and are limited. All the

primary products of fermentation reduce themselves into two groups, the acid and the alkaline, or the alkaloidal. Experiment demonstrates an analogy, as existent between the action on the body, of certain poisons which would result from zymosis externally produced, and the symptoms of some zymotic diseases developed spontaneously, in the which these very same products are generated."

The views advanced by Dr. R., in explanation of the production of zymotic diseases are deserving of a careful study. They are, to say the least of them, extremely plausible.

The next paper is "the Report of the Diphtheria Committee." The entire document with its appendices is one of deep interest. The disease to which the facts it embraces refer, notwithstanding it has prevailed extensively in former years, and been accurately described by several prominent writers, cannot be said to have been studied with the closeness and care required to throw full light upon its etiology, nature, and treatment. It is to the opportunity afforded by the prevalence of the malady in Europe and this country, that we are to look for a more careful investigation of every particular having a direct bearing upon its pathology and therapeutics; nor has the opportunity been neglected. Among the many contributions to which the investigation of the present epidemic has given birth, the one before us will hold a very prominent rank, as well for the value of the materials it furnishes as for the caution which appears to have been observed in their collection and their record.

We can only command space for the general summary of the facts of chief importance embraced in the report, which are as follows:—

"1. The occurrence of sporadic diphtheria in the county of Kent, in the years 1851-52 and 54, and probably also in 1855-56, and in the county of Essex, in the year 1854.

"2. The probable occurrence of sporadic diphtheria in different parts of the kingdom, at intervals within the last twenty years.

"3. The unusual prevalence of throat affections of all kinds, and not unfrequently of a peculiar kind (apparently, if we may so phrase it, of a *quasi*-diphtheric character), prior to or contemporaneously with the present epidemic of diphtheria.

"4. The concurrent or intercurrent prevalence in the same locality of epidemic diphtheria with epidemic scarlet fever, and the occasional occurrence of a diphtheritic affection of the fauces in scarlet fever.

"The whole of these facts have an important bearing upon the history of the development of the present epidemic of diphtheria—(a.) In reference to the introduction of the disease into the kingdom from without: (b.) In reference to the causes which have led to the development of the epidemic: (c.) In reference to the relations of the disease to cognate affections and scarlet fever. Upon all these points the foregoing facts are valuable as suggestive of further inquiry, and as indicating the direction in which that inquiry should tend, as well in those districts in which the epidemic has prevailed, as in those in which it now prevails.

"5. The doubtful contagiousness of the disease in some districts, its *undoubted* contagiousness in others.

"6. The unsanitary state of the majority of the localities in which the disease occurred.

"7. The greater liability to the disease being in the first decennium, or, to narrow the question, the second quinquennium of life.

"8. The proportion of *males* in one hundred cases was 42.3; of *females*, 58.2.

"9. The nature of the treatment indicated: (a.) The application of a more or less stimulating or caustic substances to the diseased parts; (b.) The regulation of excretions; (c.) The sustentation of the powers of the system."

To the foregoing we would add that, in the report, the duration of the disease is specified in *thirty-three* fatal cases, namely—

In 2 it continued 3 days.			In 4 it continued 7 days.			In 1 it continued 13 days.		
" 2	"	4	" 1	"	9	" 3	"	14
" 5	"	5	" 4	"	10	" 1	"	17
" 3	"	6	" 3	"	11	" 1	"	18
			3	"	12			

The mortality varied in different localities from 0 to 100 per cent. The average of the cases reported to the Committee—514 cases, 96 deaths—being 18 per cent.

The mode of death is specified in 22 cases only. *Twelve* of these died from *exhaustion*; *one* from *hepatitis* on the third day; *three* from *laryngitis* (one on the second, and one on the first day); *five* from *suffocation*, and *one* from *asthenic pneumonia*.

No post-mortem examinations were made.

The paper which follows contains some pertinent suggestions for utilizing the statistics of disease, as it occurs among the poor; for so employing them as to enable us to derive from them valuable and accurate data for determining the most frequent disabling diseases among the labouring classes in the towns, villages, and rural districts; the prevalence and severity of these diseases in different districts and localities, and the influence of age, sex, condition, modes of life and occupation, in their production; particulars, a knowledge of which is essential to a correct appreciation and right estimate of the principal predisponent and exciting causes of those diseases, with a view to their abatement or prevention.

Believing it fair to presume, remarks Dr. Gavin Milroy, to whom we are indebted for the suggestions in question, that the plan adopted in one department of the public service with success, is equally applicable to another, he points to the salutary changes already effected in the sanitary condition of the army and navy of Great Britain, based upon reforms, the suggestion of which were derived from the statistical returns of the naval and military medical officers. Five and twenty years ago no use was made, at least to any extent, of the constantly-accumulating experience of the medical staff of either branch of the service, in reference to the average annual amount of sickness, mortality, and disablement among the soldiers and sailors. It had been long surmised that, at all times, and in all climates, this amount was high—higher a good deal than it was thought it should be. It was known, too, that every now and then the losses from epidemic disease were excessive in barracks, and on board ships of war. Still, no measures for the general improvement of the health of the men could be satisfactorily devised until all the facts bearing on the subject, and contained in the returns and reports of the medical officers were examined and sifted. This was at length done, and the work has since been systematically carried out; and with the best results in the way of making known the defective arrangements which told injuriously upon health, and of leading to various improvements, which have had the effect of materially reducing the proportion of sickness and mortality among the men.

Now, Dr. Milroy is persuaded that were something of the same sort done with the mass of instructive details contained in the returns and reports of the parochial medical officers, equally beneficial results, in reference to the sanitary condition of the poor, would ensue; more slowly and gradually, it is true, but in the long run not less decisively. The poor and labouring classes suffer everywhere a disproportionate amount of sickness and death, from circumstances not inevitable or inseparable from mere poverty, or from their respective locations and employments, but which are superadded to those from either ignorance or neglect, or from causes over which they have themselves no control, however capable the evils may be for correction or removal.

An annual report, founded upon an examination of these documents, and embodying their leading facts and general results, could not fail, it is believed, in conjunction with the admirable yearly reports of the Registrar-General, to prove of the highest value to the science of public hygiene and topographical medicine.

The remaining paper in the present volume of Transactions, presents a series of remarks on the topography and diseases of the Gold Coast—the west coast of Africa. It is by R. Clarke, Esq., late surgeon to the natives. It is full of curious and interesting details in respect to the condition, habits, customs, and occupations of the natives, topography and meteorology of the country, the prevailing diseases, and the general influence upon the health of the climate. The entire paper will repay a careful perusal.

We cannot command room for even a brief notice of either of the subjects embraced in it.

Dr. C. would attribute much of the reputed insalubrity of the Gold Coast to the neglect on the part of the Europeans who visit it, of the necessary precautions required for the preservation of health. They frequently fall into excesses which the best constituted could not bear with impunity in England. Were the requisite sanitary laws obeyed the climate would prove, he believes, quite as favourable to life and health, or even more so, than others in the same latitude. Among the most important of the means of preserving health to be observed by Europeans visiting the coast, are temperance, regular and industrious habits, appropriate clothing, and daily exercise in the open air. Many entertain an impression that the sun's rays are hurtful, whereas, in nine cases out of ten, we are assured, that the mischief is not done by the sun's rays, but by errors of "personal economy."

Dr. C. informs us that among the classes best adapted to resist the climatorial influences of West Africa, the negro race holds the first rank; still, he thinks it questionable whether they enjoy this privilege to a much greater extent than persons of pure European blood, provided the latter are sober, cleanly, and active in their habits. It is true, that upon their first arrival in the country Europeans run a greater risk of incurring fatal disease than the blacks. When once they have become acclimated, however, they seem, according to Dr. C., generally to withstand the influence of the climate better than the latter; provided, always, they are temperate in their habits.

D. F. C.

ART. XX.—*The Composition of the Urine in Health and Disease, and under the Action of Remedies.* By EDMUND A. PARKES, M. D., Fellow of the Royal College of Physicians. Professor of Hygiene in the Army Medical School, &c. &c. London: John Churchill, 1860. 8vo. pp. 404.

If there be any one subject in medical science which is pre-eminently a favourite with original observers and investigators, it certainly is the urine. In every journal, and in the proceedings of every medical society, we are sure to find some recent contribution to urology, whilst monographs upon the subject are being sent out with a rapid order of succession, arguing the most unbounded faith on the part of publishers, and which is only equalled by the perseverance of the authors; and yet, notwithstanding the number of treatises on the urine, there are few, if any, which do not contain something of interest, and which are not marked by some distinguishing peculiarity. The subject is so vast, and can be treated of from so many points of view, that each separate work is sure to exhibit some feature of importance not met with in any of its predecessors; add to this, the fact, that the urine contains a great part of the ashes of the effete tissues, and the additional fact that it admits of easy examination and analysis, and we have two of the main reasons which have attracted physiologists to the study of this important excretion.

The work before us is no exception to the statement that every treatise upon the subject to which it relates, is marked by some important peculiarity. In this case the feature is that of presenting a complete record of all the observations which have been made by physiologists upon the composition of the urine in health and disease, showing the various alterations which ensue under certain fixed conditions in the proportions of the several substances which go to make up the excretion in question. The following extract from the preface sufficiently indicates the scope of Dr. Parkes' treatise.

"I have attempted in this work to give an abstract (necessarily extremely condensed, but I hope accurate), of the very numerous observations on the 'Urine of Man' which have been made of late years. It seemed to me desirable to compress, if possible, into some narrow compass, the records of researches of great interest, but yet so elaborate in their details, and so scattered in different

publications as to be almost inaccessible to men engaged in active professional work. To anything like a full account, I cannot lay claim, but I hope I have succeeded in presenting a readable book for those who wish merely to know the latest observations on this subject, and in furnishing to those engaged in similar original inquiries, some assistance in the way of references. I have given the references carefully, not from any display of pedantry, but because I am quite convinced that, in the present state of this difficult inquiry, every statement should be easily traced to its author, so as, if necessary, to be properly verified.

"The title of this work expresses its exact nature; it is a mere enumeration of the alterations in the urinary constituents, under various circumstances. I have not entered into the chemical history of these constituents, nor into the mode of determining their amounts. Works on chemistry deal rather with the first subject, and the technical treatises of Neubauer and Thudichum have, for a time, exhausted the second."

We do not intend to give a full analysis of Dr. Parkes' volume, in fact, such a work would be almost impossible, neither would a critical review of his labours be necessary, after having expressed the opinion we have upon the subject. The work, however, affords us an opportunity of laying before our readers an abstract of some of the more important observations which Dr. Parkes has collected relative to the amount of urine excreted by the human organism, and the proportions of the several constituents of that fluid.

Quantity of Urine.—Twenty-three observations are referred to by Dr. Parkes, showing a range from 35 to 67 ounces in the twenty-four hours, about 52 ounces being the average. It is seen, therefore, that the quantity of urine is subject to very considerable variation within the limits of health, being affected by the amount of fluid imbibed, the temperature of the atmosphere, &c.

Urea.—This substance is also excreted in variable amount, ranging from 286 to 688 grains in the twenty-four hours, the mean being 512 grains. Several of the lower amounts were obtained by processes which were not capable of giving accurate results. Food, physical and mental exercise, are the main factors which influence the quantity of this substance excreted.

Creatin and Creatinine.—The amount of these substances has not been definitely determined. Thudichum is the only observer quoted. According to his investigations, on two individuals, each 28 years old, the first of these constituents amounted to 4.7 grains, and the second to 7 grains. They are generally estimated as *extractives*.

Urine Pigment.—Nothing is known relative to the amount of this substance excreted by the kidneys.

Extractives.—"Under this head are included various substances, viz., pigment, creatin, creatinine, xanthin, hypoxanthin, sarcine(?), xanthoglobulin, the resinous substances called omichmyloxyde by Scharling; the resin discovered by Harley, which adheres so strongly to the pigment (perhaps the xanthin of Strahl and Lieberkühn); the acids discovered by Marcet; the substances containing sulphur and phosphorus, examined by Ronalds; and perhaps a little sugar(?)."

Nothing very certain is known relative to the amount of extractives. About 154 grains in the twenty-four hours, is perhaps a fair average.

Mucus.—Böcker's observations show this to be about 7 grains per day.

Uric Acid.—This constituent ranges from 4.32 to 14.19 grains, in twenty-four hours, the mean amounting to 8.569 grains. It is subject to variation from the character of the food, exercise, &c.

Hippuric Acid.—This substance, from Weismann's researches, would appear to be excreted in much larger quantity than the uric acid, 34½ grains being eliminated when he lived upon a mixed diet, and 17 grains when on animal food.

Oxalic Acid, Formic Acid, and Carbonic Acid are found in the urine, the two former in small amount; the latter in considerable quantity.

Sulphuric Acid—This constituent varies, according to the observations cited, from 17.34 to 41.14 grains per day, the mean being 31.11 grains.

Phosphoric Acid.—This acid ranges in quantity from 24.70 to 79.07 grains; the average amount in twenty-four hours being 48.80 grains.

Chlorine.—This substance is excreted in amounts varying from 51.87 to

173.23 grains, in twenty-four hours, 126.76 grains being the average of the results obtained by fifteen observers.

Lactic Acid, if present, is so in very small quantity. *Ammonia* is met with, according to Neubauer, in quantity equal to 7 or 8 grains.

Potash, *Soda*, *Lime*, and *Magnesia*, are excreted to the amount of about 120 grains in the twenty-four hours.

The variations which are induced by age, sex, food, drinks, exercise, &c., are next fully considered, and much important information is collected under this head.

The effects of certain medicines are next brought under notice, and then the second book is reached, which treats of the *Urine in Disease*. More than half the treatise is embraced in this section, and little of any importance seems to have escaped the author.

In conclusion, we can recommend Dr. Parkes' work to practising physicians, as one calculated to be of much service to them, and to those commencing or engaged in the study of the physiology or pathology of the urine, it cannot fail to be of great value.

W. A. H.

ART. XXI. — *On Diphtheria*. By EDWARD HEADLAM GREENHOW, M. D. Fellow of the Royal College of Physicians, Physician to the Western General Dispensary, and Lecturer on Public Health at St. Thomas's Hospital. 8vo. pp. 160. New York, 1861: Baillière Brothers.

Diphtheria: its Nature and Treatment, with an Account of the History of its Prevalence in Various Countries. By DANIEL DENISON SLADE, M. D., of Boston, Mass. The Dissertation to which the Fiske Fund Prize was awarded, July 11, 1860. 8vo. pp. 60. Philadelphia, 1861.

Diphtheria: its History, Nature, and Treatment. By WM. HENRY THAYER, M. D., Professor of Theory and Practice of Medicine, in the Berkshire, Mass. Medical Institution. From the Berkshire Medical Journal. 8vo. pp. 38 Pittsfield, Mass. 1861.

THE three publications the titles of which are given above have been called forth in consequence of the somewhat extensive prevalence of diphtheria for some time past in different portions of Europe and this country. The first is based upon a careful study of the disease in public and private practice, in conjunction with information obtained in the course of an inquiry made by its author for the English Privy Council in the spring of 1859. It forms a tolerably complete monograph on diphtheria, presenting its history as it prevailed epidemically under different names, in various countries and localities, during the sixteenth, seventeenth, eighteenth, and nineteenth centuries; with copious references to the notices which the disease has received from medical writers who flourished near or during the periods of its prevalence.

The second essay obtained, as stated in its title page, the prize of the Fiske fund for 1860. It presents a very complete exposition of the literature of diphtheria, from the earliest periods to which published notices of the disease can be traced down to the present day, and exhibits a clear and faithful account of nearly all that is known in respect to its symptoms, nature, course, and treatment. The essay will be found entire in the last number, page 145 *et seq.* of this Journal.

The third essay, though less full in its reference to the writers who have treated of diphtheria, presents a very fair and instructive digest of the leading facts that have been developed in respect to its etiology, symptomatology and treatment.

Though comparatively rare as a sporadic affection, especially in its more severe and malignant forms, diphtheria has been repeatedly observed as an epidemic, extending either simultaneously or successively over extensive regions of country, or even distant lands, including portions of the old and new worlds.

More commonly, however, we find it, in certain directions and at particular periods, limited to a single district, neighbourhood, or even house, not a case of the disease even in its mildest form being met with beyond these infected spots.

The entire history of diphtheria, its wide extension at one time, and its limitation within a more or less circumscribed area at another—its tendency to linger in particular localities, or to return repeatedly to the same neighbourhood—its varying intensity in different places, showing itself in one as an affection of great mildness, and in another as one of extreme malignancy and but little under the control of any system of remedial treatment, would seem to indicate that, while the disease owes its origin to some wide-spread aërial influence, some zymotic cause existing in the atmosphere generally, which when received into the blood brings it more or less quickly into a morbid condition, this influence or cause, whatever may be its nature and origin, is modified in the direction and intensity of its action from certain local conditions connected with either population or locality.

Although new to the physicians of the present day in its epidemic form, diphtheria has nevertheless been accurately described under other names by many of the older medical writers. One of the most graphic accounts we have of it is that given by Dr. Samuel Bard, who in 1789 published an inquiry into the nature, cause and cure of the *angina suffocativa* or sore-throat distemper which prevailed in New York in 1771; an affection evidently identical in all respects with the prevailing epidemic.

From the fact that diphtheria and scarlet fever have been very generally observed to prevail simultaneously in the same place, neighbourhood, and house, often even in the same individual, the identity of the two diseases has been inferred by some writers. That diphtheria and scarlatina are closely related to each other there can be little doubt, but that they are not one and the same affection, nor yet, as others have supposed, merely different forms or varieties of the same disease, must, we think, be evident to any one who will carefully study their history. The distinctness of the two would seem to be very clearly shown by the infrequency of a second attack of scarlet fever, and when it does occur its usually mild character, while on the contrary nothing is more common than for the same individual to experience a second or even third attack of diphtheria, within a short period of each other, and often of increased severity. Their non-identity is further shown by the entire failure of any immunity from the attack of scarlatina, in the otherwise unprotected, being afforded by the occurrence of diphtheria, or from an attack of the latter by a previous attack of scarlet fever. Further evidence of their non-identity is not wanting. Thus Dr. Greenhow refers to the absence of any *scarlatinal* rash in cases of pure diphtheria or in connection with such cases during extensive epidemics of the last named disease.

"Albuminuria," Dr. G. remarks, "an occasional symptom of both diseases, occurs at very different stages in the two; in scarlet fever it rarely or never occurs until the primary and characteristic stage of the disease is past. In a series of cases of scarlet fever, in which the urine was carefully examined day by day by himself, with a special reference to this question, he found albumen during convalescence in nearly all, but as early as the tenth day from the appearance of the rash, in only a single instance; more frequently it was not met with until the twelfth or thirteenth day. In only one of the cases, that of an adult, was the albuminuria attended by anasarca. In diphtheria, on the contrary, albuminuria is of less frequent occurrence, if the above-mentioned cases are to be received as an index of the frequency of its existence after scarlet fever, and when it does occur, it is at an earlier period of the disease, sometimes within a few hours of the commencement of the illness. Dr. G. has notes of a case in which he found albumen in the urine thirty-six hours after the first symptoms of indisposition, and before the exudation had attained its maximum intensity."

In addition to the foregoing marks of distinction between scarlatina and diphtheria, Dr. G. notices the frequency of croupal symptoms in the latter compared with the infrequency of their occurrence in the former disease. Anasarca, a common sequela of scarlatina, is very rare in diphtheria, as is also the

peculiar kind of arthritis which sometimes follows scarlatina. The same remark may be made with respect to suppuration of the glands of the neck, pericarditis and chorea.

"Besides the absence after diphtheria of the well known sequelæ of scarlet fever, the former disease is succeeded by sequelæ of a character peculiar to itself, and such as have not been found to follow scarlet fever. These are, partial paralysis of the muscles of deglutition and voice, impairment or disorder of vision, paraplegia, hemiplegia, partial paralysis of the upper extremities, numbness of the hands or feet, tenderness, pricking or tingling of the extremities, and gastrodynia. Then, lastly, the occurrence of diphtheria on other parts of the body, as on abrasions of the skin, or wounds, or on the pudenda, has no parallel in scarlet fever. When to these differences we add that the anæmia which soon occurs, and for a long time succeeds to diphtheria, is more intense than in almost any other acute disease, there can be little hesitation in accepting the conclusion that diphtheria and scarlet fever are not the same disease."

A much closer relationship may be traced between diphtheria and epidemic erysipelas, than between it and scarlatina. The two have been known repeatedly to prevail in conjunction. Erysipelas of a severe and unmanageable character will sometimes commence, increase, and decline simultaneously with diphtheria. Dr. Greenhow reports an instance of this commingling of the two diseases as observed by Dr. Nicholson in Reddich, England.

In the monograph of Dr. Greenhow reference is made to the supposed intimate relationship between diphtheria and the lung distemper, or pleuropneumonia of cattle. The two have, in many districts of country, prevailed at one time, and it is a somewhat curious and significant fact that after an absence of many years, within a short period the two should have appeared again simultaneously.

"What renders these cattle epidemics peculiarly interesting in connection with the present subject, is," remarks Dr. G., "the fact that, although the time of their appearance, twenty years ago, they were quite new to the existing generation of dairymen, farmers, and cow doctors (there being no record of their existence in this country during at least the preceding half century), pulmonary murrain, preceded by an eruptive murrain, prevailed about the middle of the last century, just before the outbreak of diphtheria which then occurred. Towards the close of the first half of the century, when Fothergill, Cotton, Huxham, Starr, and other writers, were describing the diphtheria epidemic then prevalent, an anonymous member of the College of Physicians, and Drs. Brocklesly, Hurd, and Layard, wrote their accounts of the murrain then prevailing epidemically among horned cattle. This may, indeed, have been a mere coincidence, but that the appearance of the eruptive and pulmonary diseases among cattle, and of diphtheria in the human subject, are in some measure attributable to the operation of a common cause, seems more than probable, seeing that several of the older writers on *morbus strangulatorius* (diphtheria,) mention its coincidence with certain diseases among cattle. Thus Ghizi says, there was a great resemblance between the epidemic angina which prevailed at Cremona in the years 1747 and 1748, and a disease affecting the respiratory passages at that time prevalent among oxen. Dr. Wall, speaking of the epidemics in England about the middle of the eighteenth century, says, 'This disease has so great a resemblance to the epidemic sickness amongst cattle, that I am persuaded it is of the same nature.' Severinus, who wrote in the seventeenth century, also mentions that a great mortality among cattle preceded the appearance of malignant sore throat, and M. Malouin, in his account of the epidemic diseases observed at Paris in 1746, says, that the disease among cows had already appeared in France when children were attacked by epidemic sore throat. The same reporter, in his remarks upon the diseases of October, 1748, when the form of epidemic sore throat—*maux de gorge gangréneux*—was again prevalent, says it had been noticed that oysters disagreed with every one who ate them during the month, especially before the weather became cold."

Diphtheria has not made its appearance exclusively in any particular season, climate, or locality. The warmest and the coldest season of the year, the most inclement as well as the most pleasant climate; the highest and driest situations

equally with the low and marshy have been visited by the disease. It has appeared in the open country and in the most densely populated cities; in the badly located quarters, and the ill constructed and badly kept habitations of the poor, and in the comfortable well ventilated mansions of the wealthy. In short, the disease has not been found to spare places and neighbourhoods and residences whose sanitary condition is to all appearance unexceptionable, and yet it is evident from the history of the several epidemics of diphtheria on record, and the facts collected by contemporary observers, that it is in those districts, localities and houses which are generally recognized as unhealthy that the largest number of its victims are always found. Here too it presents itself in its worst forms, and is productive of the largest amount of mortality.

The major portion of those attacked by diphtheria have been children between two and twelve or thirteen years of age. The susceptibility to the disease diminishes rapidly after puberty, and is very slight in infancy. The tendency to a fatal result diminishes also with the progress of life. Persons, however, of fifty, sixty, and even seventy years of age have been known to suffer from very severe attacks of diphtheria.

The propagation of the disease by contagion is much insisted on by many writers, while others are as positive that it is neither contagious nor infectious. As is the case with all epidemic maladies, when diphtheria makes its appearance in one member of a household it is very common for most or all the other members to suffer from it either within a very short period of the first, or one after the other at irregular intervals; or, when it occurs in a neighbourhood, to spread as it were from house to house; not because of the contagious character of the disease, however, but from the fact that all who are attacked in the household or neighbourhood, have been exposed alike to one common atmospheric morbid influence. The successive occurrence of the disease in persons all equally exposed to the same epidemic cause can be readily explained without the necessity of supposing that the subsequent cases were produced by a contagious matter emanating from the bodies of those first attacked. It is known that, in very many instances only a single member of a family, and only one or two patients in the same neighbourhood, have suffered from the disease, notwithstanding not the slightest restraint had been interposed upon the free intercourse of the sick and the well. In the report of the committee on diphtheria of the Epidemiological Society of London, the disease is said to have been of doubtful contagiousness in some districts, but undoubtedly contagious in others. A statement which in our estimation casts a very great degree of doubt upon its communicability by a specific contagion in any district.

A digest of the leading facts bearing upon the question of the contagiousness of diphtheria, will be found very fully and candidly set forth in all three of the works before us.

According to Dr. Greenhow, death in diphtheria is in one class of cases preceded by all the symptoms of collapse occurring in cases of fatal gangrene, or by a profuse hemorrhage. Usually death is caused by entire exhaustion of the powers of life by the violence or very protracted course of the disease, or the fatal event is preceded by syncope. In other instances death results from the extension of the diphtheric deposit from the throat into the larynx, presenting thus a mechanical obstruction to respiration. The sudden deaths attributed to exhaustion of the powers of life, there is reason to believe result, in some instances at least, from the formation of heart clots. This is especially the case in those instances of sudden death that occur in patients who had presented no alarming symptoms up to the period of their demise, or at a period of the disease when the patient was, to all appearance, out of danger. The fatal result is in a few instances preceded by convulsions.

Dr. Greenhow enumerates as signs of great danger, the occurrence of croupy symptoms, or of pneumonia, a brown or blackish appearance of the diphtheritic deposit, hemorrhage from the nose, throat, lungs, or intestines; purpura; copious discharge of matter from the nostrils; intense albuminuria; extensive swelling of the glands of the neck; marked diminution of temperature; sickness and diarrhoea, especially at an advanced period of the attack. Either of

the symptoms denotes a severe case; but when, as sometimes happens, two or three are combined, the danger must be regarded as imminent.

Diphtheria is liable to be complicated or followed by bronchitis, less frequently by pneumonia. In the former, besides the stethoscopic signs of bronchial inflammation, there is expectoration of a frothy or glairy fluid, intermingled with which there are often seen casts of the bronchial tubes. The presence of pneumonia, by which diphtheria is occasionally complicated, is frequently unsuspected during life, its existence being first revealed only upon an examination of the body after death. In respect to the morbid anatomy of diphtheria, with the exception of what relates to the abnormal condition of the throat, we have few very accurate observations. In the greater part of the published results of examinations after death in cases of diphtheria, no attempt has been made to distinguish those lesions present which are to be esteemed proper to the disease, and those which had been the result of accidental or intercurrent maladies.

The fauces are generally found after death to be covered to a greater or less extent with an abnormal deposit, varying in consistency from that of a semifluid, pulaceous matter, to that of a firm, consistent, and more or less elastic membrane, having a thickness of from a quarter of a line to several lines. Its outer surface is sometimes uneven, at others flocculent or fissured, and less dense than the deeper portion. In many cases the membraniform deposit is dry and of a granular appearance. Low forms of cryptogamic plants are occasionally seen upon it. Examined by the microscope, the membraniform deposit is found, according to Dr. Greenhow, to consist of coagulated fibrin and epithelium, the latter being most abundant in the outer layer, the deeper portion being more purely fibrinous. Exudation cells are often intermixed with the fibrillated texture. In some cases the deposit is more or less stained with blood. From being simply opaque, it soon becomes white or ash coloured, or if thick and adherent, brownish or buff coloured; when stained with blood, blackish. It is sometimes very loosely, at others very firmly adherent to the subjacent surface; occasionally, especially when friable and granular, it is merely superimposed upon the natural surface.

The mucous membrane upon which the pseudo-membranous deposit occurs is often intact; generally it is much congested and swollen. Sometimes it is white, opaque, or unnaturally pale, or, it has a raw appearance, the epithelium having been shed with the false membrane. It often has an excoriated and roughened appearance, is occasionally ulcerated, and more rarely gangrenous. When the adherent deposit is raised up, it is often seen to be attached to the surface beneath by numerous small filaments, as though processes of the deposit passed into the mucous follicles. When removed, in such cases, the mucous membrane is more or less dotted with bloody points.

"The submucous tissue is often œdematous, infiltrated with blood, and sometimes the seat of interstitial exudation. The tonsils are usually swollen, and on being cut into, are often infiltrated with blood, so as to impart to them an ecchymosed appearance; sometimes their tissue is softened. In two instances Dr. G. found the centre of a tonsil in a state bordering on gangrene. There is generally more or less inflammatory effusion into the structure of the tonsils. In one instance, on the tonsil being laid open, there was an oozing from it of a creamy fluid resembling pus. In some instances, the œsophagus and the muscular and other tissues around the fauces are congested or infiltrated with blood. The parotid and submaxillary regions are much swollen, and the integuments studded with livid purpureous spots."

In cases where the pseudo-membranous deposit extends to the larynx and trachea, it generally diminishes in thickness and firmness as it descends into the air-passages, until it assumes finally the form of a very thin pellicle or of a creamy fluid. There is usually more or less congestion of the mucous membrane of the affected portions of the larynx and trachea. It is often thickened, also, so as to diminish the calibre of the passage, even after the false membrane has been removed or has separated. The same remarks are true in reference to the subjacent mucous membrane here as were made when speaking of that of the fauces. The epiglottis, besides being covered on its upper and lower surface with false

membrane, is often swollen so as to contract the entrance to the windpipe. The bronchial tubes are sometimes lined with the diphtheritic deposit to the third or fourth bifurcation, or even beyond. The lungs are sometimes emphysematous, or they may be affected with pneumonia, usually of the lobular form. The kidneys are sometimes entirely healthy, in other cases they have been found congested, and presenting under the microscope when sliced, transparent fibrinous casts of the tubes, which occasionally contain blood-corpuscles, or granules of hæmatine, or a few altered epithelial cells.

There is a very general agreement among those who have had an opportunity for studying diphtheria, as to the necessity of a tonic and stimulant treatment from the very onset of the disease. It has been recommended, and we think with propriety, to commence the treatment when the patient is seen immediately when attacked by the administration of a prompt emetic of sulphate of zinc, followed by a moderate purgative of castor oil and turpentine.

The most efficient internal remedies are Watson's chlorine mixture, or, a simple solution of chlorate of potass in syrup and water, with a minim of diluted hydrochloric acid to each grain of the salt, given with the tincture of sesquichloride of iron; the dose being in each case proportioned to the age and condition of the patient, and repeated every three, four, or six hours. This mixture Dr. Greenhow prefers in cases unattended with urgent symptoms, and where the deposit is firm, and there is no tendency to hemorrhage. When the mucous membrane surrounding the deposit is deeply injected and softened, abrading and bleeding on the slightest touch, or whenever there is albuminuria, he considers the tincture of the sesquichloride of iron alone, in full and frequent doses, to be the proper remedy. If there is a tendency to purpura, he recommends the addition of from five to ten or twelve minims of diluted hydrochloric acid. When the hemorrhagic tendency coexists with great fetor of breath, the tincture of sesquichloride of iron should be given in conjunction with the chlorine mixture.

When there is great depression from the onset of the attack, ammonia given in decoction of cinchona is sometimes very useful, but in most cases it will be found inferior as a stimulant to good wine, or to brandy combined with egg or milk. The quantity and repetition of these must be regulated by the state of the circulation, care being taken to so time their employment and regulate their amount as to prevent, both day and night, any rapid or considerable flagging of the pulse. In cases occurring in young children a resort to suitable nutriment, will render the use of any large amount of alcoholic stimulants unnecessary, and by proper management the patient may almost always be got to swallow nutritious substances. The best of these are beef-tea, properly prepared; chicken broth, new milk, cream, and fresh laid eggs soft boiled, or, in cases where alcoholic stimulants cannot be dispensed with, beaten up with wine or brandy.

In the croupal form of diphtheria, as soon as the embarrassment of respiration is observed, and the patient's strength will admit of it, an emetic of sulphate of zinc and ipecacuanha should be given and followed by small repeated doses of calomel or hydrargyri c. cretâ; nutriment and stimulants being at the same time given freely and regularly, so as effectually to sustain the patient's strength. In these croupal cases we have found a combination of calomel, muriate of ammonia, ipecacuanha, and extract of hyoscyamus often to afford very prompt and decided relief. All external applications to the throat, poultices, fomentations, rubefacients and blisters, do far more mischief than good. As convalescence becomes established, fresh fish, oysters, chicken, tender-loin steaks, mutton chops or other solid food, nutritious and easy of digestion, should be given as early as possible.

The patient throughout the attack of diphtheria should be confined to his bed, and until convalescence is confirmed, he should still retain the recumbent position. Sudden movements, and active exertions of every kind should be avoided until the strength has become fully re-established. Even for some time after recovery has been apparently confirmed, it will be prudent to give a little light nourishment, or a glass of wine and water, each time before active exercise is taken.

In respect to local treatment, whatever may be the benefit derived from the direct application of nitrate of silver, or of a slightly diluted tincture of the sesquichloride of iron, or from the use of a gargle of the latter in a still more diluted form and sweetened with honey, in the early stage of the attack, when there is but a small amount of deposit and that on the anterior portion of fauces, at a later period of the disease, they are, to say the least of them, in most cases of very doubtful propriety; in many instances they are absolutely and to some extent injurious. In all cases mild, soothing detergent applications will, as a general rule, be productive of the most good.

"Simple gargles of borax or alum dissolved in water, or of solution of chloride of soda, in the proportion of half a drachm of the solution to the ounce of water, in either case sweetened with honey," are said by Dr. Greenhow, to "bring away the foul secretions and loose flakes of exudation, and thus, both by cleansing the mouth, and lessening the obstruction about the fauces, very materially to facilitate the administration of remedies and nutriment. Such gargles may be used either cold or tepid, as is most agreeable to the patient, and in very young children, or whenever patients are unable to gargle, they may be injected into the throat with a syringe, the patient's face being held over a basin immediately afterwards, so as to facilitate the return of the liquid together with the secretions and debris of the exudation which it may have detached. Probably in many cases, a simple injection of tepid water might answer the purpose equally well." Dr. G. has seen little benefit derived from the use of the chlorinated gargle, except where the breath was fetid from the decomposition of the exudation within the fauces. When the excretion has cleared away, and the throat is free from ulceration or excoriation, it is well to discontinue all local treatment, time being usually the best remedy. Sometimes the throat remains slightly congested or relaxed, or very sensitive to changes of weather for a long time after an attack of diphtheria. In the former of these cases the gargle, with the tincture of sesquichloride of iron already mentioned, has been the most useful application in Dr. G.'s experience. In the latter, he is accustomed to trust to constitutional treatment, gargling the throat with cold water, and, especially when the advice can be followed, to commend change of air.

The several forms of nervous disease which follow diphtheria usually disappear with time, but the cure is often promoted by judicious change of air and scene, good nourishment, and the use of tonics. Diluted nitromuriatic acid with the infusion of gentian or calomba, will often be found to suit the patient; citrate of iron and quinine also are often very useful. When there has been albuminuria or purpura, or the patient is extremely anemic, the tincture of sesquichloride of iron, with or without a few minims of diluted hydrochloric acid, or a grain or two of quinine to each dose, according to circumstances, is often serviceable. In some cases sulphate of quinia combined with diluted sulphuric acid, in any proper vehicle, will be found the best tonic. In cases where paraplegia has followed diphtheria, minute doses of strychnia are said to have been sometimes useful. "On the whole," remarks Dr. Greenhow, "though convalescence is usually much protracted, patients for the most part recover perfectly from the anæmia and purely nervous sequelæ of diphtheria, provided they survive the sixteenth or seventeenth day of their illness; and though tonics should certainly be persevered in, if they appear useful, there can be no doubt that they ought to be discontinued, if they do not agree with the patient nor appear to accelerate his recovery, seeing that time and good nursing will, in all probability, restore him to his pristine health."

D. F. C.

ART. XXII.—*A Handbook of Hospital Practice; or, an Introduction to the practical study of Medicine at the Bedside.* By ROBERT D. LYONS, K. C. C., M. B. T. C. D., L. R. S. C. I., M. R. I. A., etc. Physician to Jervis Street Hospital, Dublin; Professor of the Practice of Medicine and Pathology in the Catholic University of Ireland; Late Pathologist-in-chief to the British Army in the Crimea, &c. &c. &c. New York, S. S. and W. Wood, 1861. 12mo. pp. 185.

A CONVENIENT manual for the guidance of the advanced student or hospital resident in those early clinical studies which are often the most important of a lifetime, must undoubtedly be a very useful book. We find, in the present volume, condensed into a small space, and expressed clearly as well as concisely, a great deal of that kind of information which the student has generally been expected to search for through immense volumes, or to receive in the perishable form of oral instruction. It may, therefore, serve somewhat the same purpose in "walking the hospital," or in studying cases of disease in private practice, which is met in the dissecting room by the "Practical Anatomy" or "Dissector's Guide."

The first section of the work is occupied with directions for the clinical examination of patients. For classification, the system of nosological nomenclature of Dr. Farre, employed in the official registration of deaths in England, is adopted. This is by no means a perfect system; but will no doubt answer as well as any other as yet in public use. Diseases are, therein, divided into four classes, each with several orders; as follows: Class I. Zymotic Diseases. Order 1. Miasmatic Diseases. Order 2. Enthetic Diseases. Order 3. Dietetic Diseases. Order 4. Parasitic Diseases. Class II. Constitutional Diseases. Order 1. Diathetic Diseases. Order 2. Tubercular Diseases. Class III. Local Diseases. Order 1. Brain Diseases. Order 2. Heart Diseases; the remaining orders comprising Lung, Bowel, Kidney, Genetic, Bone and Muscle, and Skin Diseases. Class IV. Developmental Diseases. Order 1. Of Children. Order 2. Of Women. Order 3. Of Old Age. Order 4. Of Nutrition.

A very good practical account is next given of the best method of scrutinizing the whole condition of a patient, as concluded upon from the knowledge of I. The history of the patient antecedently to the time of the examination. II. His state at that time; comprising all particulars concerning, 1. His general, moral, and physical condition. 2. State of cerebral functions and organs. 3. State of pulse and other general phenomena of the circulation. 4. State of respiration. 5. State of general cutaneous system. 6. State of primæ viæ, digestive system, and urinary system. 7. State of thoracic cavity and organs, including the special diagnosis of diseases of the heart and great vessels, lungs, and pleuræ. 8. State of abdominal cavity and organs. 9. State of the cutaneous system. 10. Malformations, and wounds or injuries of any kind, affecting the head, trunk, or extremities. III. Therapeutic appliances. IV. Daily progress of the case. V. Termination of the case.

It is well remarked by the author, in connection with this schedule, that the method of clinical examination and record laid down in the well known work issued by the London Medical Society of Observation, under the title "What to Observe at the Bedside," is too abstractedly scientific to work with at the bedside, although it is very useful to form the mind upon in the study. The present Handbook has been composed especially with the intent of avoiding this abstractedness and impracticability, and of making the student's knowledge immediately available. There is thus afforded a very brief though comprehensive summary of the principal facts in symptomatology, physical diagnosis, chemical and microscopical examination of the urine, &c. In short, it is an epitome of Medical Diagnosis, compressed into about 110 duodecimo pages. The subjects of pathology and treatment of disease are not touched upon. Morbid anatomy, however, receives full attention, in the second section of the volume; which, to the extent of 52 pages, is entirely occupied with directions

for the post-mortem examination of all parts and organs of the body. This section will be especially valuable, both to the student and practitioner; as it contains detailed as well as general information, given just in the order and manner in which they are wanted for use.

An Appendix is added, with brief directions for writing prescriptions, and a tabular view of the principal signs, abbreviations and Latin words employed in pharmaceutical language. A Glossary follows, chiefly of Latin and Greek words used in medicine; and the volume concludes with a few blank Forms for reporting cases, according to the system inculcated in the text.

The writer's experience has evidently made him acquainted with the wants of the clinical student, and has enabled him to prepare a book for which we anticipate a popularity in proportion to its merit.

H. H.

ART. XXIII. *Compendium of Human Histology*. By C. MOREL, Prof. agrégé à la Faculté de Médecine de Strasbourg. Illustrated by 28 plates. Translated and edited by W. H. VAN BUREN, M. D. New York: Baillière Brothers, 1861. 8vo. pp. 207.

THIS work, which is intended to serve as an introduction to the study of human histology for the medical student, is especially to be commended, as attempting to embody the more recent observations of German and other investigators. Thus, for example, in the section on cells, we see with pleasure Schwann's erroneous doctrine of free cell-development thrown aside, and we are correctly told that "every cell must derive its origin from another previously existing cell. In the present state of science, but two modes are known in which cell-generation is accomplished in human histology: *endogenous* generation, and multiplication by *cleavage*." (p. 14.)

So also we are presented with the more modern doctrine of the structure of connective tissue (called connecting tissue in the translation), in the account of the development of which certain of the errors of the earlier microscopists are, however, retained.

"The essential elements of connecting tissue are fibres and cells. Its fibres are of two kinds, viz: connective fibres properly so called, and elastic fibres. Its cells are diminutive in size, generally branched, but sometimes fusiform, and have received from Virchow the name of plasmatic cells." (p. 17.)

And again: "The cellular element of connecting tissue (the plasmatic cell) is a recent discovery; we are indebted to Virchow for the first thorough exposition of its nature, and especially of its important pathological relations. Plasmatic cells are minute corpuscles, sometimes fusiform, but more frequently star-shaped, with sharp outlines, and connected with each other by means of their branching prolongations, so as to constitute a network similar to that formed by the cells of bone." (p. 19.)

"Finally, the researches of Virchow tend to prove that all, or nearly all, of the morbid formations developed in the meshes of the connecting tissue throughout the body, are traceable to the perverted growth of plasmatic cells." (p. 21.)

The above quotations sufficiently show the views of M. Morel with regard to the minute structure of connective tissue, as well as the source from whence they are derived. We are decidedly of the opinion that it would have been better had he drawn from the same authoritative source his account of its development. According to Virchow, while some of the embryonic cells are assuming the stellate character of connective tissue corpuscles (plasmatic cells), and others becoming elastic fibres, &c., the homogeneous intercellular substance in which the elements are imbedded becomes fibrillated in appearance, and thus produces the connective tissue bundles. According to M. Morel, however, the connective tissue bundles are developed out of cells after the fashion first described by Schwann.

"The fibres of connecting tissue develop themselves from cells of the simplest

form, which commence the process by assuming an elongated shape, then join each other, end to end, and gradually break up into fibres within, so that each row of cells thus attached by their extremities is developed into a bundle of connective fibres." (p. 23.)

Besides this method, M. Morel thinks he has observed in pathological cases the transformation of a similar series of cells into a single fibre, and also the transformation into a single fibre of an elongated series of free nuclei; as to the first of these possibilities, we feel convinced that the observation on which it is based was concerned with the development of yellow elastic fibres, with which the drawing of the specimen well accords (Plate IV, fig. 4); as to the second, we are not able to gain a satisfactory idea of the basis of fact upon which it reposes, or to interpret the drawing of the case (Plate IV, fig. 5).

The erroneous views entertained by M. Morel of the genesis of the connective tissue bundles, necessitates a similar error in connection with fibro-cartilage. "In the formation of fibro-cartilage a portion only of the original formative cells take on the changes above described, whilst the remainder transform themselves into connective and elastic fibres." (p. 29.) This is a view of the development of fibro-cartilage which has no foundation in fact, and which is at once overturned by the practical study of the development of this tissue; the matrix of the fibro-cartilages being homogeneous at first, and subsequently fibrillating, so that these fibres, like those of the white fibrous bundles, represent, in fact, not cells, but transformed intercellular substance.

It is not in our power, nor do we feel inclined at present to follow M. Morel from subject to subject in detail. Did we deem it advisable to do so, we should in many other points be obliged to criticize him for not presenting to the student the best information attainable at the present moment. Indeed, any error in the selection of his views on disputed points becomes especially important, where, as in the present work, the compendious character of the treatise compels the author to admit no other account than that which he determines upon as his own.

Perhaps, however, the most serious fault of the book is its extreme brevity. An account of the minute anatomy of the organ of smell, begun and finished in about half a page, of the mammary gland in six lines, of the Meibomian glands in three, &c. &c., is not complete enough to give anything but the loosest general notions of the matter to a novice, and can be of no possible use to the more advanced student.

In concluding, it is only just to bestow the praise which is deserved upon the American translator, who has moreover added a number of valuable notes, and to commend the general character of the lithographic plates, twenty-eight in number, with which the book is illustrated.

J. J. W.

ART. XXIV.—*Recherches sur la Substitution Graisseuse du Rein.* Par M. le Docteur ERNEST GODARD, Ancien Interne des Hôpitaux de Paris, Membre de la Société de Biologie, etc. Paris, Victor Masson, 1859. 8vo. pp. 31.

M. GODARD occupies the first few pages of his monograph with citations from several authors, to the effect that fat may be deposited in the tissue of various organs, a fact which we believe is not disputed. He next impresses upon the reader the view that fatty infiltration or deposition, is quite a different thing from fatty degeneration, or transformation—a task which he might well have spared himself, as we are not aware that any opposition exists among pathologists to this very obvious and reasonable doctrine.

In the next place, M. Godard asserts that the kidney is, after the muscles, the organ most liable to undergo fatty degeneration, or substitution. We think this opinion admits of being questioned; nevertheless, no doubt can exist that fat is frequently substituted for the normal tissue of the kidney. Several cases are cited by M. Godard from the writings of Morgagni, Rayer, Cruveilhier, Petrequin, and others, and he concludes that

"The cases which exist in science, and which I have collected, appear to me to show that fatty substitution of the kidney is essentially characterized by the deposit of a variable quantity of fat, either in the parenchyma of the gland, or at its periphery, but always within the capsule or in the hilum of the organ. Then the fat penetrates between the mucous membrane and the pyramids, and leads little by little to the atrophy of the kidney."

After giving the details of an autopsy which came under his own notice, and quoting a case from Baader, and one reported by Rayer, M. Godard says:—

"The two preceding cases afford some information relative to the condition of the patients, in which, after death, fatty substitution of the kidneys was discovered.

"I shall not endeavour, whilst recalling the principal facts which they present, to trace the history of this disease; in order to do this, I am of the opinion that more complete, and more voluminous testimony is necessary. I will insist only on the difference existing between the infiltration of fatty granulations and globules in the epithelium of the kidney—an occurrence which is often met with in albuminous nephritis—and fatty substitution of the same organ, which results from, and coincides very frequently with, the presence of one or many calculi in the ureter, or pelvis of the kidney.

"I think that these two diseases are distinct, and that the one cannot be transformed into the other.

"Whilst the existence of the first is made known by the occurrence of easily recognized conditions (albumen in the urine, ascites, and anasarca), the other supervenes insidiously, and may lead to the more or less complete destruction of one of the kidneys, without exciting suspicion of its presence. The case observed by M. Brichteau shows that if both kidneys are affected, the secretion gradually diminishes, then stops for a certain number of days, and the patient dies without presenting any of the symptoms which are the ordinary consequences of the sudden suppression of the urinary excretion."

This passage closes M. Godard's memoir—a memoir in which we do not find an original idea, and which belongs to a class unhappily becoming very numerous, especially in France.

The essay is illustrated with one steel plate engraving, representing fat globules, adipose vesicles, and several tissues affected with fatty substitution, which, like the text, is made up of borrowed material—Bowman, Johnson, and Robin, being put under contribution for the purpose. There are also two lithographs, showing the appearance of the kidneys, in the case of which M. Godard had the opportunity of witnessing the autopsy.

W. A. H.

ART. XXV.—*Anatomy of the Arteries of the Human Body, descriptive and surgical, with the Descriptive Anatomy of the Heart.* By JOHN HATCH POWER, M. D., etc. With Illustrations, by B. Wills Richardson, F. R. C. S. I., etc. 12mo., pp. 374: Dublin, Fannin & Co., 1860.

ALTHOUGH specially designed for the use of students, and of practitioners who have not frequent opportunities for dissection, this excellent little volume may be consulted with advantage by any one who has occasion to investigate matters relating to its subject. The descriptions contained in it are extremely clear, concise, and accurate; equal perhaps to those of any of the more imposing treatises on anatomy. They have evidently been prepared by one who not only knew what to say, but how to say it, having both a natural facility and an acquired skill in teaching.

Dr. Power's production is a very much modified edition of a book bearing the same title, by the late Dr. Flood, of Dublin. The latter work, which enjoyed a good share of popularity in its day, is now out of print; it has been partly curtailed and partly amplified, and its arrangement slightly changed, in preparing it for reissue. We are somewhat inclined to regret the omission of the introductory sketch of the physiology and comparative anatomy of the circulatory

system, prefixed to the earlier editions, and to wish that Dr. Power, instead of striking it out, had reproduced it with such corrections and new facts as to make it accord with the science of the day. Nor do we think his transference of the anomalies to a concluding chapter an improvement upon the usual method of describing them in connection with the normal anatomy of each part; this is, however, a matter of individual taste. We are glad to notice that he has been careful to recognize the efforts which Godman, Gibson, Mott, Peace, and other American practitioners have made for the advancement of the anatomy and surgery of the arteries.

The typographical execution of this little work is handsome, and very generally correct. More than sixty wood-cuts, many of them original, have been introduced in preparing this edition; but these seem to our eye to have been drawn better than they were engraved. Those relating to the heart, taken from the work of M. Jamain, are by no means equal to the originals, and many of the others are at the same time small and coarse.

We should be glad to see a good reprint of this monograph placed in the hands of the profession in this country; it would doubtless meet with a cordial reception.

J. H. P.

XXVI.—*On Gout; its History, its Causes, and its Cure.* By WILLIAM GAIRDNER, M. D. Fourth Edition. 8vo. pp. 430. London, 1860.

THE very high estimate we gave of the treatise of Dr. Gairdner, upon the appearance of the first edition, has been, we are pleased to find, fully sustained by all who have subsequently given a public expression of their opinion of the work. The views of the author in respect to the pathology of gout, furnish, it must be admitted, a far more probable explanation of the phenomena and results of the disease, in different cases, from its onset onwards, throughout its entire course, than any of those previously advanced, and a more consistent rationale of the treatment, whether preventive or curative; whether directed to the eradication of the depraved diathesis upon which the attack of gout essentially depends; to the alleviation of certain urgent symptoms, or to the special management of individual cases, in the particular circumstances under which these usually present themselves to the notice of the practitioner. The treatise is throughout highly instructive; and, on many questions of deep interest, concerning the nature and causation of gout, and its therapeutics, is eminently suggestive. The fact that the demand for it has been such as to have exhausted already three editions, is a significant indication of the esteem in which the treatise is held by the physicians of Great Britain, while its translation, by professional men of eminence, into other European languages, evince its equal appreciation by the physicians of the Continent.

Having already, in our notice of the first and second editions of the treatise, given a tolerably full analysis of the views advanced by its author, and of the leading facts and arguments by which these are sustained, the same task need not be gone over on the present occasion.

To the edition before us the author has added such observations in illustration and enforcement of his teachings, as his subsequent more enlarged experience has suggested.

A chapter has been added, devoted to a consideration of the mode of application of the principal remedies adapted for the cure of gout to individual cases, having regard to the age of the patient, his constitution, and the period of the attack. The remarks which are made in respect to the treatment of the metastatic forms of gout are pertinent and instructive. We have seen quite enough of the disease to convince us that, in many of these cases, the practitioner will produce far more mischief by prompt interference, and a resort to heroic remedies, than were he to remain altogether inactive, or, at furthest, to merely attempt by the simplest and mildest measures, the alleviation of the immediate and more urgent sufferings of the patient. We can fully endorse the remark of

Dr. Gairdner that, in a large number of instances of metastatic gout, if time be given, and forbearance practised, it is wonderful to observe what painful and alarming symptoms will quietly depart with very little artificial aid. Time, as he justly remarks, is of great importance in the treatment of misplaced gout. However grave appear the circumstances, it is always unwise to prescribe in hurry and alarm, the seeming risk being often much greater than the actual danger, so that recovery is frequently retarded rather than prompted by the powerful remedies which are too readily and too commonly resorted to.

"At present it is a matter of very nice tact and judgment to distinguish the case in which the most heroic remedies must be instantly applied to sustain the fleeting powers of life, from that in which the advantage of the patient is best consulted by 'expectation' and forbearance; but of one thing Dr. G. feels certain, that medical practitioners will best exhibit those qualities which entitle them to confidence, when they do not suffer themselves to be hurried into decision by the clamor of patients and their friends. In general, it may be said that those cases attended with little change in the pulse, even though there be intense suffering, severe vomiting, and signs of much disturbance in the stomach and diaphragm, best bear delay; and that those in which the action of the heart is very depressed, and where there is little acute pain, are the cases which most imperatively call for prompt and effectual aid."

The edition before us contains the notes of the German translator of Dr. G.'s treatise, Dr. Braun, of Wiesbaden, whose large experience, and intimate acquaintance with German physiology and pathology, give to them no trifling interest.

D. F. C.

ART. XXVII.—*Further Researches on the Gray Substance of the Spinal Cord.* By J. LOCKHART CLARKE, Esq., F.R.S. (Reprint from the Philosophical Transactions, Part I, 1859.) London, 1859. 4to., pp. 31. With seven lithographic plates.

IN this paper Mr. Clarke has given us the elaborate results of his investigations into the minute structure of the posterior cornua of the medulla spinalis.

"Many of the older anatomists," he says, "from Bartholinus downwards, had observed that the gray substance of the spinal cord is softer, more delicate, and more vascular than the surrounding white columns; but Rolando was the first to point out a diversity in its structure. He observed, chiefly in quadrupeds, that on each side the posterior third of the gray crescent consists of a peculiar cineritious substance, which presents a different aspect from that which forms its two anterior thirds: it is different in colour, darker and less red. Rolando, however, assigned too large a space to this 'new substance,' which does not comprise so much as the posterior third of the gray crescent, but forms only a comparatively narrow and curved lamina or band around the extremity of each cornu, and, when viewed in a thin section by transmitted light, is found to be actually much paler and more transparent than the rest of the gray crescent."

Our author divides each posterior cornu into two parts, namely, the caput cornu and the cervix cornu. The caput cornu consists of two different portions, an outer and comparatively transparent portion, called the gelatinous substance, and an inner and more opaque portion or base. The gelatinous substance consists of nerve-fibres, nerve-cells, bloodvessels, and connective tissue with nuclei.

The nerve-fibres are transverse, longitudinal, and oblique. The primitive fibres composing the bundles of nerves are tubules of small, average size, the larger kind possessing double contours. In size the greater number vary from about the $\frac{1}{50000}$ th to the $\frac{1}{20000}$ th of an inch in breadth, but are intermixed with others of about the $\frac{1}{17000}$ th of an inch in diameter.

The cells of the gelatinous substance are described by Mr. Clarke as large, small, and intermediate. The large cells are almost exclusively found among the semicircular fibres which run within the external border of the substantia gelatinosa, and never extend beyond the middle of its depth. They are more or less oval, pyriform, fusiform, crescentic, triangular, or otherwise irregular, and

contain distinct nuclei. They all give off processes from two to five or six in number. The small cells abound in every part of the gelatinous substance. They are round or oval, somewhat pyriform, fusiform, or angular, and give off two processes or more. They vary considerably in size, and many are nearly as minute as blood-disks. The largest of them contain distinct nuclei. The cells of intermediate size are found chiefly near the verge of the posterior columns.

The inner or more opaque portion of the *caput cornu* also consists, besides bloodvessels and connective tissue, of longitudinal, transverse, and oblique fibres and cells.

Upon the microscopic anatomy of the *cervix cornu* Mr. Clarke dwells at considerable length. He devotes several pages also to the description of the gray substance of the *filum terminale* and the epithelium of the central canal.

For the benefit of those who may be engaged in similar investigations, we transcribe Mr. Clarke's method of preparing parts for microscopic examination.

In an appendix to the paper under consideration he says: "The structure or part intended for examination should be as fresh as possible, and cut into portions as small as is compatible with the end in view. These portions I formerly hardened by means of a mixture of one part of spirit of wine and three parts of water, which at the end of twenty-four hours was replaced by a fresh mixture of equal parts of spirit and water, and this again after the same interval was replaced by pure spirit, which ought to be renewed every five or six days. At the end of ten to fourteen days the medulla is sufficiently hard for making sections, which are then subjected to the following process for the purpose of inducing transparency. The sections are first placed in a mixture of one part of strong acetic acid, and four, five, or six of spirit, for a period varying from two or three to ten minutes, according to their thickness. They are then washed in pure spirit, after which, if thin, they are floated on the surface of spirit of turpentine, where they remain until they are quite, or nearly transparent, when they are removed to glass slides on which a little Canada balsam has been previously dropped. If now examined under the microscope, they frequently show but little or no traces of cells or fibres; but if the sections be set aside for some time, and treated occasionally with a little turpentine and Canada balsam, the cells and fibres reappear and present a beautiful appearance. Before they are finally covered with the glass, they should be examined at intervals by the microscope. If the sections be thick, I find it best to place them in a shallow vessel, the bottom of which is kept simply wet with turpentine, which can therefore ascend from below while the spirit evaporates from their upper surfaces; for the *principle* of the method is this: to replace the spirit by turpentine, and this by Canada balsam, without drying the sections. The method at first presents some difficulties, and practice is necessary to insure complete success. Experience, also, will suggest, according to circumstances, many little deviations from the exact rules here given, which, to a certain extent, must be considered as general. For the last three years I have used chromic acid instead of spirit in the process of hardening. The medulla of man and the higher mammalia is steeped in a solution of one part of crystallized chromic acid in two hundred parts of water, for two or three weeks, and then kept in a solution of about one part of bichromate of potash in one or two hundred parts of water. Spirit is used to wet the knife in making the sections, which are first placed in spirit for a few minutes, and then (with or without the previous use of acetic acid) transferred to the turpentine and Canada balsam, as before."

J. A. M.

ART. XXVIII.—*Etudes sur la Rage dans divers Etats de l'Europe, et particulièrement dans la haute Italie.* Par M. BOUDIN. *Annales d'Hygiène publique*, &c., Janv. 1861.

It is a popular notion that hydrophobia, or rabies canina, occurs almost exclusively during the hottest season of the year. This idea is both suggested and perpetuated by the laws which require dogs to be muzzled during that period

alone. On the other hand, the greater number of medical writers upon this subject have reiterated the statement that rabies is quite as apt to occur under one extreme of temperature as another. Among ancient authors who describe the disease, none make an allusion to its peculiar prevalence during the summer. Cælius Aurelianus, who has left so graphic and correct a picture of rabies, has not a word of its relation to temperature. Pliny, indeed, says *rabies canum sirio ardente homini pestifera*; which implies that rabies is peculiarly dangerous in summer, but is not so at that season exclusively. Paulus Ægineta, after stating that dogs for the most part become mad during violent heat, adds: but, also, as Lycus says, sometimes in extreme cold. Aëtius writes that this affection is most frequent where wide extremes of heat and cold prevail.

Among modern authors who have insisted upon the occurrence of rabies in very cold as well as in very hot weather, may be mentioned Boerhaave and his commentator Van Swieten, Mease, Villermé, and Troillet. All are agreed that mere temperature is not an adequate cause of the disease, because in high northern and in tropical climates it is of rare occurrence; and some, like Bardsley, infer from the latter fact that atmospheric conditions have no share in its production at all. It is evident, however, that one or both extremes of temperature may serve as an exciting cause of this or any other affection which depends upon a specific virus. And, again, its prevalence at a particular time may depend, as Hertwig remarks, upon the mere accident of a mad dog running at large, and inoculating a large number of persons and animals. Thus, in May, 1817, a single mad wolf, in the neighbourhood of Lyons, bit twenty-three persons, thirteen of whom died of hydrophobia.

In his valuable "Report on Hydrophobia," Dr. Blatchford makes the following remarks: "Every investigation, and anywhere made, only proves that a belief in the influence of the 'dog-star,' or climate, or season of the year, as inducing or favouring the production of rabies, is an utter fallacy, a mere astrological fancy, handed down, it may be, from remote ages, its very antiquity, perhaps, shielding it from the probe of investigation; but yet entirely unsupported by facts." Now, it will have been remarked in the citations above made, that the doctrine here advocated has always been held by medical writers upon rabies; they have uniformly maintained that no season enjoyed an immunity from the ravages of the disease, and have generally agreed that either extreme of temperature is favourable to its production. How, then, are we to account for the popular belief, or superstition, whichever it may be?

While in doubt upon this point, we met with the paper of M. Boudin, of which the title is prefixed to this article, and finding that his results, the largest which ever have been collected, fully sustain the current notion of the greater prevalence of rabies during warm weather, it seemed not uninteresting to extend the research into other periods and localities than those examined by him. Having divided the year into two halves—as nearly as the reports would allow—one of which included the warm and the other the cold months, two tables were constructed, showing the number of persons bitten, and also the number who became hydrophobic during the respective periods. The following are the results:—

Persons Bitten.

Authority.	March to Aug.	Sept. to Feb.
Blatchford ¹	52	49
Smith ²	57	50
Radcliffe ³	45	39
Boudin ⁴	526	280
Tardieu ⁵	76	41
	<hr/> 756	<hr/> 459

Or, 1.64 : 1

¹ Trans. Am. Med. Assoc., ix. 257.

² New York Journ. of Med., N. S., xv. 241.

⁴ Annales d'Hygiène, Jan. 1861, p. 183.

³ Lancet, Feb. 1855, p. 153.

⁵ Ibid., Jan. 1860, p. 199.

Authority.	Date of Attack.	
	March to Aug.	Sept. to Feb.
Blatchford	47	41
Smith	48	59
Radcliffe	40	30
Tardieu (1854) ¹	67	30
Tardieu (1860) ²	110	71
	<hr/> 312	<hr/> 231

Or, 1.35 : 1

It appears, then, that the number of persons bitten by mad dogs during the warm months is two-thirds greater than during the cold months of the year; and that the proportion attacked with rabies in the former period is one-third greater than during the latter. These facts are sufficient to explain the popular belief upon the subject, while they, at the same time, demonstrate how many rabid animals and cases of hydrophobia in man are to be met with at seasons when there is but little apprehension of the disease. It may, perhaps, be suspected that the excess of wounds from rabid animals during the warm months is due to the greater number of vagrant dogs at this season, rather than to the greater relative proportion of cases of rabies among these animals; but, on the other hand, it is chiefly at this period that precautions are taken, through police regulations, to diminish the canine population, and, by means of muzzles, to prevent dogs from biting.

A. S.

ART. XXIX.—*Cours théorique et pratique de Braidisme ou Hypnotisme, Nerveux considéré dans ses rapports avec la Psychologie, la Physiologie et la Pathologie et dans ses applications à la Médecine, à la Chirurgie, à la Physiologie Expérimentale, à la Médecine Légale et à l'Éducation.* Par le Docteur J. P. PHILIPS, suivi de la relation des Expériences faites par le Professeur devant ses élèves, et de Nombreuses Observations par le Docteurs Azam, Braid, Broca, Carpenter, Cloquet, Demarquay, Esdaile, Gigot-Suard, Giraud-Teulon, Guérineau, Ronzier-Joly, Rostan, etc. Lapis quem reprobaverant ædificantes. Paris : J. B. Baillière et Fils, 1860. 8vo. pp. 180.

It is a well-known fact that if we place in front of, and at a little distance from, the eyes of any impressible person, some brilliant object, as a piece of glass or polished metal, and cause the attention of such person to be concentrated upon the object, or if we make passes before his eyes with the hands in such a manner as strongly to rivet his gaze, he will presently fall into a cataleptic state, analogous to that produced by the inhalation of certain anæsthetic agents. While in this condition, his limbs are either rigid, or more or less completely relaxed: general sensibility is blunted, or even abolished, and certain of the special senses, such as hearing and touch, become remarkably acute. The faintest sounds are perceived, the lightest impressions are vividly felt; and dreams and hallucinations, as in certain cases of catalepsy, offer themselves in quick succession to the mind.

These singular phenomena—though within the past year they have attracted and are still attracting much attention in Paris, owing to, their practical applications in surgery—are by no means new. Under the name of *actinobolism*, they were long ago described by Kircher, in his *Ars Magna*. A writer named Schwenter, also dwells upon them in a work published in 1656, and entitled *De licie Physico-Mathematicæ*. They appear to have formed an important element in the religious ceremonies of antiquity. According to Bernier and Anquetil Duperron, the mystical Joguis of India were accustomed to hypnotize themselves by looking fixedly, for hours at a time, at the end of the nose, or at some imaginary point in space. We are also informed that the Christian monks

¹ Ibid., Jan. 1854, p. 230.

² Ibid., Jan. 1860, p. 205.

of Mount Athos, the Arabians of the sect of Aïssa, and the Egyptian magicians, threw themselves, at will, into this cataleptic or trance-like state, by somewhat similar procedures. The phenomena of hypnotism, or Braidism, constituted the basis of the medical system of the Brahmins, the Therapeutæ, the Asclepiades, and the mystical physicians of the school of Alexandria. They explain, in a measure, some, at least, of the prodigies performed by ancient and modern thaumaturgists, and they afford an experimental confirmation of those singular recitals which occupy so considerable a place in the annals of the middle ages concerning the works of enchanters, magicians, sorcerers, &c.

In the 16th century, the examination of these phenomena was undertaken in a critical spirit. Cardan, Maxwell, Wirdig, Paracelsus, Van Helmont, and others, endeavoured to bring them into the category of general physics. More recently, Mesmer gave to the world the startling results of his investigations. In 1843 appeared the remarkable work of Braid, entitled *Neurypnology, or the Rationale of Nervous Sleep, considered in relation with Animal Magnetism*. This was followed, in 1845, by his *Observations upon Trance*, and in 1852, by another publication upon *Witchcraft, Hypnotism, Electro-Biology, &c.* Since the time of Mesmer, various observers, of greater or less note in the medical and scientific world, have directed their attention to this subject. Among these we find Cloquet, Rostan, Huston, Lordat, Reichenbach, Frank, Jobard, Gregory, Esdaile, Lafontaine, J. K. Mitchell, J. H. Bennett, Simpson, Carpenter, Alison, Holland, Sir D. Brewster, Dugald Stewart, Charpignon, and others. Quite recently, Dr. Azam, of Bordeaux, having long and patiently experimented upon hypnotism, announced to the physicians of Paris that he was able to produce catalepsy and anæsthesia at will, during which surgical operations, ordinarily very painful, could be performed without suffering to the patient. Attracted by these experiments, Drs. Bazin, Broca, Demarquay, Velpeau, and others, have also been lately engaged in examining this subject. The results of their investigations are recorded in the *Gazette Hebdomadaire*, the *Archives de Médecine et de Chirurgie*, *Gazette des Hôpitaux*, *Bulletin Général de Thérapeutique*, *Journal de Genève*, *Gazette du Midi*, and other journals.

The work before us is one of the latest contributions to the science of hypnotism. It is enthusiastically written, and contains the details of many experiments and observations of a very singular and anomalous character. Its opening pages are occupied with a brief account of the origin and progress of Braidism. This is followed by a description of the process by which this condition is produced, of the phenomena by which its different stages or degrees are characterized, and of the dangers which sometimes accompany its induction. The physiology of hypnotism is next considered, and finally many observations and experiments are brought forward, illustrative of its medical, surgical, and medico-legal applications. These observations and experiments constitute by far the most satisfactory and reliable portion of Dr. Philips' work; and they are calculated to attract the attention of those of our readers who are at all interested in this curious field of inquiry.

J. A. M.

ART. XXX.—*Lives of Eminent American Physicians and Surgeons of the Nineteenth Century*. Edited by SAMUEL D. GROSS, M. D., Professor of Surgery in the Jefferson Medical College of Philadelphia. 8vo. pp. 836. Philadelphia: Lindsay & Blakiston, 1861.

THE series of biographies presented in this volume comprises those of some of the most distinguished of the physicians and surgeons who by their knowledge and skill, their success as teachers, cultivators and practitioners of the healing art, have illustrated their profession, and conferred honor upon their country.

Among the elder of our medical and surgical practitioners were many men of great learning—close observers and acute interpreters of the various morbid conditions of the animal organism—fruitful in resources for their control, and

skilful in the adaptation of these to the several phases of morbid action presented in individual cases. Of the professional career of these gentlemen, the story, taken in connection with the circumstances amid which their lives were passed—their respective characters, personal habits, and domestic and social surroundings, constitute a theme which, when fitly dealt with, is as instructive as it is interesting.

The lives of some of those we have just had reference to, have been ably and fully delineated; of others we have as yet but imperfect records, and of many none at all, while in the mean time the materials necessary for their preparation are fast disappearing.

Early in the present century an effort was made by Dr. James Thatcher, of Massachusetts, to collect the biographies of the more distinguished of our older physicians, and as far as the effort extended it was attended with marked success. Some years later the same thing was attempted, by Dr. Stephen W. Williams, likewise of Massachusetts. Neither of these works, nor the two together, cover the entire ground of American medical biography. The number of distinguished medical men of our country, whose lives are omitted, far exceed that embraced in them. Nor does the volume before us supply, to any great extent, the omissions of Thatcher and Williams. There are still materials sufficient for many more volumes of American Medical Biography, even though the life of no one should be admitted whose demise occurred later than the end of the first quarter of the current century.

The volume under notice is an exceedingly interesting one. The lives contained in it, with scarcely a single exception, are written with ability, and commendable impartiality, presenting a very vivid and spirited delineation of the character and services, the professional and social status of their respective subjects. But three of these memoirs—those of Drs. John Syng Dorsey, Ephraim McDowell, and Daniel Drake are from the pen of Dr. Gross, the others have been derived from other sources—either from the personal friends of those who are their subjects, or qualified, at least in a special manner, for the delicate and responsible office of their biographers from an intimate acquaintance with their career, their labours, and their character. The plan is an excellent one, and better calculated to insure a just delineation of character than were the entire series of biographies prepared by a single individual.

Several of the lives in the present volume—those of Hosack, McClellan, and Hartshorne—have been prepared by the sons of their respective subjects. The task, which is confessedly one of unusual delicacy, has been performed in the instances before us, with commendable taste and judgment.

The memoirs presented in the work of Dr. Gross are arranged in chronological order according to the period of decease of those whom they respectively commemorate. A somewhat awkward plan, which gives Drs. Dorsey, Godman and Eberle precedence of Dr. James Thatcher, a practitioner only a very few years the junior of Dr. Rush.

The work of Dr. Gross is in all respects deserving of a favourable reception, not only from American physicians generally, but from the community at large. It is one well adapted to popularize the character and office of the medical practitioner, by placing his educational requirements, his professional services, and his claims upon his fellow citizens in a proper light before the public.

D. F. C.

ART. XXXI.—*A Catalogue of the Pathological Cabinet of the New York Hospital.* Classified and arranged by ROBERT RAY, JR., M.D., Curator. With a Memoir of the author. 8vo., pp. 364. New York, S. S. and W. Wood, 1860.

EVERY large hospital where clinical instruction is given should have attached to it a pathological museum, in order that the demonstrations of disease in the living subject may be illustrated still further, and the ante-mortem and post-

mortem phenomena placed, as it were, side by side. With most men, and perhaps especially in dealing with such a science as medicine, the memory retains impressions made upon the eye better than those which fall upon the ear. Hence, where students are to be practically taught in the elements of their profession, and in many cases to receive their preparation for an immediate entrance upon its active duties, it is important that the lecturer should be provided with specimens exhibiting the causes, as well as the meaning, of symptoms. A striking instance of this is found in diseases of the chest; a teacher can explain abnormal heart or lung sounds to infinitely greater advantage by means of specimens, or even diagrams, showing the lesions met with, than by mere description.

The New York Hospital was, we believe, the first institution of the kind in this country to act upon this idea; its museum was first opened in 1841. The example thus set has been recently followed at the Pennsylvania Hospital, as well as at the Baltimore Infirmary. Our impression is that at nearly all the English hospitals, as well as at some of those in Germany and France, more or less extensive pathological collections have been made.

As a matter of course, a full descriptive catalogue greatly enhances the value of every such collection. Very few specimens in morbid anatomy are self-explaining, and often our conclusions are essentially modified by the knowledge of certain circumstances in the antecedent history of a case. A catalogue may, indeed, be so prepared as to be of great use even to those who have not access to the collection itself, and especially to such as are labouring in a similar field. The largest and best work of this kind is probably the catalogue of the splendid museum of the Royal College of Surgeons, in London, arranged by Messrs. Stanley and Paget. Equally well known, or perhaps more so, from the frequency with which they are quoted, are the two octavo volumes and the atlas of exquisite lithographic plates published by the Parisian Faculty of Medicine, describing and illustrating the bony preparations contained in the Musée Dupuytren.

The catalogue whose title heads the present notice, although a far more unpretending work than those just mentioned, bears evidence of great care and judgment in its preparation, and contains much that will prove interesting as well as useful to pathologists. Its author, whose talents and industry were bidding fair to win for him a high professional rank, was called from all earthly labour before this volume had been given to the press; and his former preceptor, Dr. Watson, has prefixed to it a short sketch of his life.

It would of course be impossible to enter into anything like an analysis of a work, one of the chief objects of which is conciseness of description; suffice it to say that the catalogue of the Museum of Guy's Hospital was the model adopted in framing it, and that the collection has been classified into eight sections. These are (1) the bones; (2) the joints and tendons; (3) the digestive system; (4) the respiratory system; (5) the circulatory system; (6) the nervous system and organs of special sense; (7) the genital and urinary systems; (8) parasites. Under these heads are arranged descriptions of nearly one thousand specimens, with references attached to many of them, by which their histories may if desired be still further traced. By the regulations of the New York Hospital, the house physicians are obliged to keep ample records of all the cases treated in the institution; and in this way a very large mass of experience has been accumulated. The pages of these case-books are often referred to in the catalogue of the pathological cabinet.

We cannot but regard the publication of this volume as an important step in the advancement of medical science in America, and as a fresh recognition of the value of morbid anatomy. Dr. Ray's last work will have gained for him an enduring remembrance among his professional brethren, should it prove the earnest of greater things in the cultivation of pathology in our hospitals; and such we trust it will.

J. H. P.

ART. XXXII.—*Report of the Board of Health of Philadelphia, for 1860; Sanitary and Statistical. In accordance with an Act of Legislature, approved March 8th, 1860, for the Registration of Births, Marriages, and Deaths.* Philadelphia, 1861. 8vo. pp. 81.

THIS is an interesting document. Though somewhat superficial and defective, it nevertheless recommends itself to the notice of every citizen, inasmuch as it deals with matters which concern his safety from disease, suffering, and death. The duties intrusted to the Board of Health place it among the most important of all our municipal bodies. Presuming that those who compose it have a just appreciation of the object of their appointment, and an intimate acquaintance with the best means for its accomplishment, and that the Board is clothed by law with ample power to carry into full effect whatever sanitary reforms it shall, after due deliberation, and with all the facts necessary to the formation of a correct judgment before it, deem necessary and proper—its operations cannot fail to become an efficient agent in securing the health, promoting the comfort, and enlarging the prosperity of the community. Upon it the indwellers of the city may then safely rely for the prevention of every avoidable disease in their midst, by guarding against its causes, removing promptly such as are present, and preventing, if possible, their recurrence in future.

In the report before us, we are presented with the first fruits of the law for the registration of the births, marriages, and deaths occurring within the limits of the city of Philadelphia, which went into operation, under the auspices of the Board of Health, on the first day of July, 1860. The returns under this law, as given in the report, embrace, consequently, a period of six months. These, although insufficient of themselves to serve any valuable purpose in reference to the vital statistics of our metropolis, have been properly tabulated as reliable data for the preparation of future reports, to which we must look for materials adequate to the construction of a correct sanitary history of Philadelphia.

After disposing of the subject of registration, and the manner in which it has been inaugurated under the provisions of the law of 1860, the report hastily glances at some of the leading questions connected with the all-important theme of civic or public hygiene, with the view of directing the attention of the citizens of Philadelphia, and their municipal representatives and agents, to the necessity of their correct and prompt solution, and the carrying into full effect the principles and procedures thence deduced. In the leading views and suggestions embraced in the report, we fully concur; from the correctness of certain of the details given by the committee from whom the report emanated, we must, however, entirely dissent. Thus, we deny that the present system of *surface drainage*, even when regulated and aided by the best and most extended system of grading and paving, can ever become, in the words of the report, "well adapted to carry away the fluid refuse from our streets, houses, and yards, and to answer as conduits (?) for the rain which falls upon our city." In this climate, throughout every season of the year—summer and winter alike—it is an intolerable nuisance, and should be replaced, as quickly as opportunity presents and the financial means of the city are in a condition to warrant it, by a system of underground drainage, through conduits and culverts, of sufficient size and extent, and constructed upon strictly scientific principles.

We must express our surprise at the action of the Board of Health, as set forth in the report, in relation to the City Hospital; an institution destined expressly for the reception of patients affected with contagious or infectious diseases, who cannot be properly attended at their own residences, or in any other of our public or private hospitals, without endangering the spread of the disease to those in the same institution with them, or even beyond its walls; an institution which had been pronounced necessary by the medical and municipal authorities of our city over half a century ago, and has been maintained ever since, by each successive Board of Health, under all the various modifications in organization it has undergone. Great as is our surprise in reference to the action of the Board in closing the hospital referred to, it is even more so when we consider the reasons

presented in the report before us in justification of that action. The City Hospital has been shut up, we are informed—1st, because there is no law which makes it obligatory on the Board to maintain a hospital for the reception of patients other than those for whose benefit ample accommodations are secured at the Lazaretto; 2d, because of the large increase of private-hospital accommodations throughout the city; 3dly, because of the revolution of medical opinion in respect to the communicability and mode of propagation of many of those diseases for which the City Hospital was specifically intended; and, lastly, because the outlay for the support of the hospital has been altogether disproportionate to the benefits that have been derived from it.

In considering the validity of any or all of the foregoing reasons, we must keep steadily in mind the leading object for which the Board of Health has been organized, as well as certain of the means imperatively demanded for carrying out that object by circumstances that not only may possibly occur at any moment, but which have occurred again and again, and are at this present time in actual existence.

If a hospital for the reception of patients labouring under an infectious or contagious disease is one of the means necessary to prevent the spread of such disease in any particular locality or neighbourhood, or throughout the community, the power to open and maintain a hospital for that purpose exists in the Board of Health. Even were it not conferred by the very letter of the law under which the Board acts, it is most unquestionably by its spirit. Not only, we insist, does that law *allow* the Board to maintain a hospital of the character referred to, but it *imperatively requires* it to do so.

We will suppose that a case of smallpox—over which disease the Board of Health exercises the same jurisdiction as it does in respect to any other—occurs in an inmate of one of our hotels, boarding-houses, or prisons; in a domestic; or the disease shows itself among the occupants of a confined, overcrowded, and unwholesome neighbourhood; or among the crew or passengers of a ship that has just arrived at one of our wharves. In either case, if there is no hospital in charge of the Board of Health ready for the immediate reception of the infected person or persons, and within a reasonable distance from the centre of the city, to render it easy of access, they must be sent either to the Lazaretto, to the almshouse, or to one of the private hospitals of our city, or be left where they were found as so many sources of infection, endangering the spread of disease far and near. Now, this latter alternative the Board would be criminal in resting contented with—will they be able to adopt either of the others?

The project of sending smallpox patients out of the city to be treated at the Lazaretto, is one that should not be entertained. It would cause some delay in preparing the means necessary for their transit, when prompt removal is always of importance. The distance, also, of the Lazaretto from the city, the difficulties which lie in the way of their comfortable and safe transportation there, and the expense that would be incurred by this manner of disposing of those, affected with smallpox and other contagious or infectious diseases, are reasons sufficiently weighty to place it out of the question.

The idea of sending smallpox patients to the almshouse, is equally preposterous. In the first place, the Board of Health have no power to allow the Guardians of the Poor to dispose of cases of malignant or contagious disease, even when these occur in paupers, excepting in a manner perfectly satisfactory to it, and under the immediate superintendence of its officers; for it is an express duty of the Board of Health to guard against the introduction and spread of pestilential and contagious diseases. It cannot itself send, nor permit others to send, patients affected with these diseases to the almshouse, because of the danger which would thereby be incurred of spreading the disease throughout its crowded wards. Were a properly constructed building—ample in size and perfectly isolated—to be provided by the Guardians of the Poor, there would, of course, be no valid objection to its being made use of as “a pest-house” for paupers—but for paupers only. The idea of sending to such an institution any one who is able and willing—or his friends for him—to pay all expenses incurred for his support and treatment in an institution separate and apart from that appropriated to the accommodation of paupers, is preposterous in the extreme, and should receive no countenance from any quarter.

It is very true that, in consequence of the change in medical opinion in respect to the origin and propagation of many diseases heretofore classed as infectious or contagious, the same isolation of these as was formerly believed to be necessary is no longer insisted upon. Hence, those affected with yellow, typhoid, and perhaps typhus fever, in some of its forms at least, may not be excluded from the wards of any of our general hospitals; but we know of no one among the many admirably arranged and skilfully conducted institutions of this kind which exist in our midst, that would be willing to admit into its wards a case of the mildest form of smallpox. Even were any one of them to become so far derelict to the duties it owes to the patients placed under its care as to be willing to introduce among them a case of smallpox, it would become the imperative duty of the Board of Health to prevent this being done. We are to recollect that it has been constituted the curator of the health of the poor as well as of the rich—of the pauper as well as of the millionaire. It is the province of the Board to watch over the sanitary condition of the prison, the almshouse, and the hospital; of the confined neighbourhoods and hovels where dwell the humblest and most abject of our population, as well as of the open streets and ample mansions inhabited by the proud and the wealthy.

We believe that a hospital, properly planned and judiciously administered, exclusively appropriated for contagious and infectious diseases, under the jurisdiction and control of the Board of Health, is an institution which cannot, with safety, be dispensed with. Occasions are constantly occurring in which the want of such a hospital would necessarily entail much discomfort, if not actual suffering, upon not alone the pauper classes of our community, but upon respectable citizens, whose only misfortune is that they have become the subjects of an infectious or contagious malady, or that their families are threatened with the invasion of such disease, in consequence of their vicinity to those labouring under it and amid circumstances well adapted to favour its spread.

The large outlay required for the support of the City Hospital is one of the reasons urged for its discontinuance. We confess that we are unable to perceive how the expense to the community is to be in any great degree lessened, if at all, by shutting up the City Hospital, provided the Board of Health is true to its obligations to protect the community from the spread of contagious or infectious diseases. Every time the latter makes its appearance in our midst, there will be a necessity for the removal and isolation of cases. These must be taken charge of by one or other of the municipal bodies, and become, necessarily, a public charge. We are in favour of every judicious reform having for its object the curtailment of the present enormous expenditures of our city government, but we are, at the same time, fully convinced that every movement calculated to jeopard the health and lives of the community, no matter what may be the *apparent* pecuniary saving effected by its inauguration, would become, in the long run, wasteful in the extreme.

D. F. C.

ART. XXXIII. *Theory and Practice of the Movement-Cure: or, the Treatment of Lateral Curvature of the Spine; Paralysis; Indigestion; Constipation; Consumption; Angular Curvatures, and other Deformities; Diseases incident to Women; Derangements of the Nervous System; and other Chronic Affections—by the Swedish System of Localized Movements.* By CHARLES FAYETTE TAYLOR, M. D. With illustrations. Philadelphia: Lindsay & Blakiston, 1861. 12mo. pp. 295.

ALTHOUGH it would be neither right nor expedient to affix to the code of medical ethics a sweeping prohibition of all specialization of professional labour, yet it is only a sound conservatism that throws a heavy *onus probandi* upon every new, or newly systematized, method which proposes to displace long recognized modes of practice, and that denounces, unconditionally, all schemes of *exclusive* therapeutics.

Dr. Taylor's work on the "Movement-Cure" is, in the preface, addressed to

the medical profession; and, notwithstanding the unavoidable prepossession against it produced by the title itself, we have thought it right to give it a fair and attentive reading. It is a plausible and not uninteresting exposition of the system of medical gymnastics, originated by Ling, of Sweden, who died at Stockholm in 1839. The name of *Kinesipathy* was at one time given to it. The author's reading has been well adapted to the handling of his subject, and his claims on behalf of the system are advanced without much pretension. Yet, with all candour, the best we can say of it is, that its truths are trite, in the knowledge of the profession at least, if not out of it; and that its novelties in principle have the disadvantage of being unsound, while those of detail are comparatively unimportant.

The essential idea of the book is, that passive exercise, localized in particular groups of muscles, can be made to act with voluntary movements, and often to more decided advantage than they, in controlling and modifying the local conditions of nutrition, so as to promote the cure of chronic diseases. The two radical elements in the pathology of organs containing muscular tissue are considered to be, *retraction* and *relaxation*. These are to be opposed and remedied, respectively, by favouring the entrance of arterial blood into the capillaries of the region affected, or by assisting the exosmotic escape of fluid from the tissue into the capillaries and veins. The former is accomplished by *eccentric*, and the latter by *concentric* muscular manipulations and movements.

The hypothesis at the base of this reasoning involves at least two very doubtful statements; that any decided influence upon the flow of arterial blood into a part is exerted by stretching the muscles passively, *i. e.*, the operation being performed by another than the patient himself; and, secondly, that any important action upon the venous circulation can be maintained by the reverse process of passive flexion of the limbs or trunk. These propositions comprising the whole essence of the movement-cure, so far as it is at all novel or peculiar, we are forced to consider it unsustained by any well-grounded physiological reasoning.

Many of the remarks in Dr. Taylor's book upon the general physiology of exercise, and upon popular errors in hygiene, are sound enough, and sufficiently well expressed. If the first part of the work, without the above-mentioned hypothesis, had been issued as an essay upon such subjects merely, we might have thought it likely to be useful. But, when we study the application of the scheme to the treatment of disease, such an opinion cannot be entertained. In lateral curvature of the spine, it is true, the principle of invigorating the non-symmetrical muscles has been long familiar to surgeons; the merits or demerits of particular appliances used, is a topic of secondary consequence. In paralysis, the quotation by Dr. Taylor from Dr. Todd, shows that exercise has been well understood to have a certain value. The mechanical pullings and twistings of the kinesipaths must, it appears to us, have a vastly less exhilarating effect in this disease, and still more in dyspepsia, consumption, and nervous disorders, than riding on horseback, or walking, with those who are able, or driving or sailing with weaker invalids. Nor do we believe that they possess one whit of advantage over ordinary gymnastics, pursued merely upon the idea of developing all the muscles, and promoting the circulation in all parts of the system. It is possible to carry out the details of the plan of treatment described in this work only in an establishment fitted up for the purpose, with apparatus and assistants dedicated to it. Will the superiority of this medication, if it can be called such, be commensurate with the preparation required? We should prefer the Turkish bath, or the *thermæ*, recently introduced into England, as auxiliary to appropriate medical treatment. The danger of such specialties is, moreover, that, in proportion to the absence of real and tangible advantage in the scheme, is the inducement for overweening enthusiasm to take its place, and to fortify itself by denunciation of regular medicine, and by efforts to impair the confidence of the public in all measures but its own. For these reasons, we are obliged to confess that, notwithstanding the interest of many of the subjects treated of in Dr. Taylor's book, we cannot regard it, in a practical aspect, as likely to confer benefit either upon the profession or upon the community at large.

QUARTERLY SUMMARY
OF THE
IMPROVEMENTS AND DISCOVERIES
IN THE
MEDICAL SCIENCES.

ANATOMY AND PHYSIOLOGY.

1. *On Pulmonary Osmosis, or Researches on the Absorption and Exhalation of the Respiratory Organs.* By LOUIS MANDL.—In the author's researches on the intimate structure of tubercles (published in the *Archives* for 1855), he arrived at the conclusion that these morbid productions are devoid of all organization, consisting solely of a solid, amorphous substance, the result of a coagulation of matter first dissolved in the blood and then exuded. He compared this amorphous element with analogous products of various diseases, and demonstrated that pathological histology does not furnish sufficiently distinctive characteristics.

Continuing these researches, and examining into the causes which give rise to tubercular exudation, the author has arrived at the conclusion that it is sometimes due to a general cause—a diathesis—and at others to a purely local or accidental cause. In the course of the investigation, he was struck with the frequency of the coincidence of tuberculization and diabetes; and he sought to ascertain the influence of glucose, spread throughout the organism and penetrating every tissue, upon the production of tubercle. This has led to the performance of a series of experiments, an account of which it is the object of this paper to give. The experiments consisted in placing certain aquatic animals in saccharine solutions. The following is a *resumé* of the results:—

1. The life of animals breathing in water is incompatible with the presence of a more or less considerable quantity of saccharine matter, whether this consist of the various kinds of sugar or non-fermentable sweet principles, such as glycerine or mannite. 2. Infusoria, pulmonated mollusks, annelides, crustacea, batrachians, aquatic insects, and fish, all perish in a period more or less short, according to the kind of sugar, the strength of the solution, and the species of the animal. Thus, fish, from twelve to fifteen centimetres in length, die in forty minutes in a solution containing a tenth part of glycerine, and live four or five hours in a solution of sugar of the same strength. 3. Numerous experiments have proved that death cannot be attributed to poisoning, to chemical action on the blood, to fermentation, to absence of air, or to viscosity—it being solely due to the *osmosis* (endosmosis and exosmosis) induced by the saccharine solution. 4. This action is excited through the permeable membranes, and especially through those of the respiratory organs; the non-fermentable sweet principles possessing superior osmotic power to that of the true sugars, which explains the celerity of their action. Infusoria comport themselves as vesicles, the osmosis operating through the whole of their delicate envelop; and they are seen first to collapse (exosmosis) and then to distend (endosmosis), even sometimes to bursting. In animals of higher development, in which the thickness of the integument confines the osmosis chiefly to the branchiæ, the blood is observed

to become thickened in these, and the circulation arrested in them, in consequence of the exosmosis of the liquid portions. The circulation in the lungs of a frog may be instantly arrested, over a limited space, by the application of a drop of glycerine, and in a few minutes by a drop of syrup. Experiments made on the fœtus of the salmon, and on the circulation of plants, have furnished similar results. 5. Many hundred experiments were tried with the endosmometer, having various substances (the pericardium of the sheep being the most suitable) for its diaphragm, and it resulted that the water of the blood, charged with the salts of the serum, first passed into the saccharine solution, next the albumen, and then the colouring matter. The passage of all these elements took place in a short time, when only a small portion of the osmogenous substance lay on the diaphragm; but the passage of the albuminoid elements becomes slow in proportion as the quantity of the saccharine fluid is considerable, so that, for a long time, the water containing the salts is then all that passes through. 6. Development is also impeded by saccharine solutions, as is shown by experiments made with muscular tissue macerated in them, and by those instituted on the fertilized ova of fish. 7. The following physiological and pathological circumstances are elucidated by the foregoing investigation: (a.) Infusoria, the excessively thin envelop of which is very sensible of osmotic action, can neither live nor become developed in fruits impregnated with sugar, as plums or grapes; but insects with their thicker integument may become developed in apples, plums, etc. Doubtless, it is by the thickness of its envelop that the cysticercus is protected from the osmotic action of the glucose in the liver. (b.) The antiseptic and preservative power of sugar, known for so long a period, may be explained by its power of arresting the development of organized beings. (c.) The thirst induced by the ingestion of sugar is due to the exosmosis of the serosity of the blood. (d.) The abundance of glucose in diabetes explains the characteristic thirst in this disease, and the impossibility of any serous effusion. (e.) The coincidence of gangrene with diabetes has been frequently observed, and may not the arrest of circulation, leading to gangrene, be attributed to the existence of glucose in the vicinity of the capillaries? (f.) The absence of venereal desires and of procreative power observed in the diabetic may be due to the presence of glucose in the semen—this substance in the experiments causing the immediate death of spermatozoa. (g.) Small quantities of sugar induce an exosmosis of the gastric juice, and thus powerfully aid digestion in some persons; while large quantities introduced into the blood increase the osmotic power of the fluid; and this explains the utility said to have been derived from the exhibition of sugar in dropsies. (h.) The advantageous employment of glycerine as a dressing in gangrene, hospital gangrene, etc., is explained by the impossibility of the development of organisms in pure glycerine. Purulent and other serosities are resorbed by the glycerine; and the utility of the application of this substance to dry surfaces arises from the osmosis establishing a greater humidity. (i.) Various experiments have been undertaken in reference to the local production of tubercle by the injection of osmogenous substances, and especially saccharine substances, into the pulmonary vesicles. The results, which, when more multiplied, will be published, give good reason for the belief in the production of localized tubercles in the lung by the exudation (exosmosis) of plastic matters, independently of diathesis. These experiments bring the subject back to its original starting-point—the accidental production of tubercles in diabetes, in which affection all the tissues are impregnated with glucose. These inquiries open a new field to therapeutics, in explaining the (in the author's belief) frequent spontaneous or artificial cure of tubercles of the lung, when these are independent of diathesis.—*Med. Times and Gaz.*, Jan. 12, 1861, from *Archives Générales*, tom. xvi.

2. *Experiments on some of the various Circumstances influencing Cutaneous Absorption.*—WALLER's researches on cutaneous absorption lead to results which appear to us important with regard to practical medicine. The subjects of the experiments were guinea-pigs and albino rats. When the leg of a half-grown guinea-pig was immersed into a mixture of equal parts of chloroform and tincture of aconita, the part was after fifteen minutes insensible, and the symp-

toms of poisoning by aconite soon followed—viz., nausea, efforts at vomiting, coldness of surface, weak circulation, laborious respiration, slight convulsive symptoms, and death. The immersion in simple tincture of aconite without chloroform did not cause any of the symptoms of poisoning. If, however, the sciatic nerve had been previously divided, then the immersion of the leg in the simple tincture of aconite was sufficient to poison the animal, evidently through the distension of the capillaries and the more rapid flow of blood caused by the division of the nerve. A ligature placed round the limb before the first symptoms of poisoning had appeared, prevented the toxic influence on the system, but it rarely did so after the appearance of the earliest symptoms. In albino rats the immersion of the leg in a solution of atropia in chloroform caused dilatation of the pupils already after two minutes and a half in young animals, after five minutes or later in old animals. If turpentine was substituted for chloroform, the dilatation of the pupils did not occur while the leg remained immersed in the fluid, but appeared immediately after the removal of the limb. The substitution of alcohol for chloroform as a solvent causes great retardation of absorption. An immersion of twenty to thirty minutes produced only very slight effects. Still more slow is the absorption when the atropia is dissolved in water with a slight addition of acetic acid, or when the watery extract of belladonna is rubbed over the leg. The immersion of the foot of a young rat in a solution of morphia in chloroform caused after about five minutes somnolency and great dilatation of pupils, which latter phenomenon certainly must appear very remarkable when we consider the effect of morphia, given in the usual way, on the pupil. Three minutes' immersion in a solution of strychnia in chloroform produced dilatation of the pupil, and after five minutes, the well-known symptoms of strychnia-poisoning manifested themselves. No effect was obtained when a solution of strychnia in alcohol was employed. These experiments clearly show how important is the choice of the menstruum in the endermic application of some medicinal substances.—*B. and F. Med.-Chir. Rev.*, January, 1861, from *Proceedings of Royal Society*, vol. x.

3. *Excretion of Kreatinin and Kreatin through the Urine.*—SCHOTTIN, having during several years bestowed much attention on the circumstances under which kreatin and kreatinin appear in the urine, considers that these substances are chiefly derived from the tissue-change of the striped muscles. The author inclines to the view, that kreatin and kreatinin pass by further oxidation into urea. In healthy urine he found, while merely vegetable food was consumed, no kreatin or kreatinin, and only a very small quantity under the influence of an almost entirely animal diet. In many diseases, too, these substances were not found; in some pathological conditions, however, in which either the transformation of these bodies into urea appeared to be prevented (uræmia), or in which the waste of muscular tissue was increased (as in typhoid fever), the amount of kreatin excreted by the kidneys ranges between 0.2 and 1.1 grammes (about 3 to 17 grains) within the twenty-four hours.—*B. and F. Med.-Chir. Rev.*, Jan., 1861, from *Archiv f. Heilkunde*, 1860.

MATERIA MEDICA AND PHARMACY.

4. *On Indian Hemp, particularly in relation to its property of producing Sleep.*—Dr. FRONMÜLLER first employed the Indian hemp in the case of a phthisical patient in the year 1850, and since that time he has devoted himself to the especial study of the properties of this substance. The result has been the production of a treatise founded upon the clinical observation of no less than a thousand cases in which Indian hemp was administered. This plant has been very much extolled by many practitioners in various countries, but has lately fallen into disuse, owing to the supposed uncertainty of its operation. The discredit attached to it is attributed by Dr. Fronmüller partly to the contradictory

statements published concerning its operation by various writers, and partly to the difference in its effects in the Eastern hemisphere compared with those observed in Europe. The Indian hemp of India and that grown in Europe present the same external form, but they differ in the relative proportion of narcotic resin which each contains, and which is the active principle of the plant. It appears that the amount of resin depends not only upon differences of latitude, but also upon the depression or elevation of the regions where the plant is grown. Chemical analysis has discovered that the Indian hemp contains gum, bitter extractive matter, albumen, chlorophyll, ethereal oil, and a peculiar resin. This resin is called *cannabin* by some writers, and forms six to seven per cent. of the dried plant. The ethereal oil has been obtained by Martius only in small quantity; it is of a slightly yellowish colour, of a peculiar ethereal camphor-like smell, and an aromatic astringent, and afterwards bitter taste. The preparations of Indian hemp hitherto employed are the powdered plant for pills or powders, resinous extract of hemp in powders or pills, tincture of the resin, and emulsion.

With regard to its application to the practice of medicine, Indian hemp may be considered valuable as a tranquillizing antispasmodic drug. It has been employed with favourable results in tetanus and trismus, cardialgia, rheumatism, and in some mental diseases. Of 1000 cases in which this drug was administered by Dr. Fronmüller, 552 were males and 448 females, and the patients were of various ages, from one year to fifty and more. The principal diseases of the patients were tuberculosis, inflammation, surgical diseases, rheumatism, diseases of the eyes, nervous diseases, and dropsy. The greater part of the cases were treated by the spirituous extract prepared by Merk in Darmstadt; but others were treated by the extract prepared by the late Jacob Bell, of London. It is to be observed that all the observations were made on cases in which there had been no sleep for several nights, and in which the continuance of sleeplessness was to be anticipated unless some narcotic was employed.

Out of the thousand cases it was found that the narcotic property of the hemp was completely developed in 530, partially in 215, and little or not at all in 255. With the extract of Indian hemp the best effects were produced 145 times with a dose of 12 grains, 64 times with a dose of 8 grains, 63 times with a dose of 10 grains, 35 times with 16 grains, 22 times with 3 grains, 17 times with 2 grains, 15 times with 14 grains, 14 times with 20 grains, 13 times with 6 grains, 12 times with 5 grains. The period of falling to sleep, and the duration of sleep in the cases, are numerically recorded by Dr. Fronmüller, and also the number of cases in which unfavourable results ensued on the day of taking the drug, or on the next morning. Comparative observations were also made with morphia in cases where the Indian hemp had failed. Out of 29 cases in which Indian hemp had produced no effect, sleep was produced by morphia in 24. The dose of morphia was in general rather a strong one—from one-sixth of a grain to 2 grains—in order to induce sleep. In the remaining 5 cases the morphia produced no effect.

The conclusions to which Dr. Fronmüller arrives as the results of his observations are the following: 1. That Indian hemp, among all the known medicines which cause stupefaction, is that which produces a narcotism most completely supplying the want of natural sleep, without occasioning any great excitement of the vascular system, without special stoppage of the secretions, without the supervention of unfavourable consequences, and without subsequent paralysis. 2. That Indian hemp, on the other hand, is not so strong nor so certain in its operation as opium. 3. That Indian hemp may given in all acute inflammatory diseases and in typhus fever. 4. That it is worth a trial to alternate the Indian hemp with opium in cases where the latter fails. 5. That the best mode of administration is the alcoholic extract in small pills which contain an addition of the powder of the Indian hemp. The lowest dose for producing sleep may be estimated as eight grains given in pills of one grain each.—*B. and F. Med.-Chir. Rev.*, 1861, from *Vierteijahrschrift für die practische Heilkunde*, 1860.

[It is proper to add a caution in regard to such large doses. If the extract be of the *best quality*, eight grains might not always be a safe dose.—Ed.]

5. *Oil of Turpentine as an Anæsthetic*.—Mr. JOHN WILMSHURST, in a communication in the *Lancet* (March 2, 1861), states that he has successfully

employed the ol. terebinthinæ rect. as an anæsthetic. The first case, he says, "in which I tried its effects was that of Mrs. H——, matron on board the emigrant ship *Indiana*, of which I was then surgeon-superintendent. About twelve months ago, having exhausted my little stock of chloroform, and the patient suffering from violent neuralgia in the course of the supra-orbital nerve, it occurred to me that of the remedies at hand the most likely would be the vapour of turpentine. This I immediately applied, sprinkled on a handkerchief, to the nostrils, similarly to chloroform, and was surprised to find it not merely soothe and allay the pain, but, after a few inhalations, produce a gentle sleep and state of anæsthesia, from which she awoke without any headache or other unpleasant symptoms, and quite free from pain.

"I may mention, without going into detail, that I have since tried it in one or two slight but painful operations—as extracting a broken needle from a sensitive part, and in some cases of cramps, convulsions, nephralgia calculosa, &c. Its effect seems to be to allay nervous irritation, spasm, and pain, without deranging the action of the heart, and to produce a calm anæsthetic sleep."

6. *On the Therapeutical Employment of the Double Iodide of Iron and Quinine.*—Dr. VIOLETTE has given the double iodide of iron and quinine in several affections in which the preparations of iron have been recommended, and he has been enabled to prolong its use without meeting with the symptoms so often produced by the separate use of iron and quinine. He has never observed either weight at the stomach or offensive eructations, or gastralgia. In many affections of the uterus, after the local treatment has succeeded, the iodide of iron and quinine has seemed to restore the strength and revive the appetite. He has witnessed the same results in convalescence from typhoid fever, and from all diseases which induce a considerable impoverishment of the blood. Dr. Violette considers this medicine peculiarly efficacious in chlorosis. M. Becquerel allowed him to administer it to some young chlorotic girls, whose leading symptoms were complete loss of colour of the mucous membranes, a waxy complexion, excessive weakness, total loss of appetite, blowing murmur in the heart and carotids; in such cases Dr. Violette found that a rapid improvement followed the use of the double iodide. The appetite returned very quickly, and at the same time the strength was restored; a slight rose colour replaced the pale tint of the mucous membrane, and the extreme paleness of the face was changed for the natural colour.—*B. and F. Med.-Chir. Rev.*, Jan., 1861, from *Gazette des Hôpitaux*, July, 1860.

7. *Kukui or Kekune Oil.*—Now that the oil of *Aleurites triloba* is spoken of so highly in France as a purgative oil, a few particulars concerning it may not prove uninteresting.

The plant producing the fruits from whence this oil is extracted, belongs to the natural order Euphorbiaceæ, and is plentiful in the Sandwich, Society, and other groups of islands in the Southern Seas. It is also to be met with in some parts of Jamaica and the East Indies. The oil has been for some time known in Jamaica as Spanish walnut oil, and in India as Belgaum walnut oil. In Ceylon the oil is called kekune oil, and in the Sandwich Islands kukui oil. The tree is known in some parts of Polynesia as the candle nut tree. The fruits are nearly as large as a walnut, and the kernel is inclosed in a thick, hard shell. These nuts are often strung together by the natives, and burnt, without any other preparation, as torches. In the history of the Mutiny of the Bounty it is stated, that the rooms in Pitcairn's Island were lighted up by torches made of "doodoe" nuts, strung upon the fibres of the palm leaf, forming a good substitute for candles. These nuts are also so strung and used by the San Blas Indians in Central America, and a child is in attendance to knock off each nut as it becomes burnt out.

The following is the method adopted in obtaining the oil in Jamaica. Each nut is carefully cracked or broken, and the kernel as carefully separated from the hard shell, lest the latter, having a brown dye quality, should affect the colour of the oil. The kernel is then put into a large mortar and pounded as fine as possible. It is afterwards thrown into a caldron with plenty of water

and boiled. It is allowed to simmer for hours, until all the oil is well extracted and floats on the surface. Meanwhile, and until all is gathered together, the oil is skimmed off into another clean vessel. The oil thus collected is then boiled over again in a smaller vessel for a short time, in order to throw off any aqueous particles remaining after the first skimming. If the oil is not then perfectly pellucid it is run through blotting paper. Eight quarts of kernels will yield about three pints of oil. The yearly produce of this oil in the Sandwich Islands is about 10,000 gallons. It has been shipped to the markets of Chili, New South Wales, and London, but, hitherto, without much profit. It realized about £20 per imperial ton in London. In 1843 about 8,620 gallons were shipped from Honolulu, valued at 1s. 8d. per gallon.

This oil has been used as an artist's oil, for which purpose it is said to possess valuable qualities, although it cannot be applied as a drying oil. It is only lately that attention has been called to its medicinal properties. It is purely purging, and, not like the croton, jatropha, caper-spurge, sandbox, and other euphorbiaceous oils, productive of vomiting at the same time. It is affirmed to be as mild as castor oil, and, being more fluid, is better to take. It is without either taste or smell.

The nuts have, within the past twelve months, been sold in the London market under the name of kekui nuts, and there is no doubt that, upon inquiry, some of the oil could be procured, and it evidently well merits the attention of the profession. A purgative oil which shall possess all the advantages, and none of the disadvantages of castor oil, is a desideratum worthy of being secured.—*London Med. Review*, Sept., 1860.

8. *Glycerine*.—Dr. W. R. GORE, Surgeon to the Limerick Infirmary, states (*Dublin Med. Press*, Dec. 26, 1860) that he has been using glycerine for several years in the city infirmary with great advantage, and can confirm the observations of Dr. Jacob as to its utility in keeping soft the vegetable extracts when combined with it for external application. "Three or four years since it struck me that the tendency of belladonna to dry upon the skin could be obviated by admixture with glycerine, since which time I have been using it with all the advantage involved in such an object. I have been in the habit of applying a paste over the brow in those ophthalmic cases in which intolerance to light forms a prominent feature, composed of ext. belladonna, ext. valerian, pulv. opii. and acid hydrocyan, made into a soft paste with glycerine, which remains soft and absorbable to the last, and is very often most useful. I also use it with extract. conii and acid hydrocyan, as an internal application to the eye with belladonna or aconite, and with the most marked benefit in similar cases. In those strumous and other chronic affections of the cornea which render that organ defective in translucency, I have been using with advantage iodide of lead and atropine dissolved in this fluid, which is, in my opinion, one of the best media for the application of any of those active agents known to be useful in medicine. It keeps the part in a state more suitable for absorption, and permits the active agent to operate through it more readily than ordinary lard, oil, or ointment. We have found it very valuable in keeping dressings soft, pliable, and easy of removal. Sutures are kept, when painted over with it daily, in a similar state. Lotions are prevented from drying off too readily, and cloths moistened with it prevent too rapid an evaporation. In frictions it is the nicest possible thing to grease the skin, and in those obstinate scaly eruptions of the scalp it is most useful in softening the scab and rendering the medicinal agent operative. I do believe it will be found a most valuable agent in medical hands, applied as experience and ingenuity may dictate."

9. *Honey and Glycerine in Surgery*.—Dr. C. F. MOORE, Medical Superintendent of Middleton Hospital and Infirmary, says (*Dublin Med. Press*, Dec. 26, 1860) that several months ago he commenced the use of glycerine to bedsores in bad fever cases, and finding it to exceed my expectations, he used it in all cases where a healing dressing was required. It occurred to him, some three or four months since, that honey might answer as well; and as a very great difference in price exists, that an important saving might thereby be effected by using an

equally efficient agent, "as some of the properties of honey previously known to me led me to think it to be. I have now used it in several cases in hospital and private practice, as in ulcers left by scalds, in a wound after the removal of a large tumour presenting some malignant characters from fascia of the leg in a woman of seventy-six years of age, in a case in which dead bone was being discharged after injuries to a man's leg, bed sore from fractured thigh, &c. &c. As I hope to bring the value of honey as a dressing more fully under the notice of the readers of your valuable journal, I will only now state that it seems especially useful in cases where a tendency is manifest in a wound or ulcer to throw up unhealthy granulations instead of healing kindly. It seems also to protect the skin adjacent by its own conservative agency, so to speak, as well as by correcting the sceptic properties of the discharges. In evidence of this latter property, I may state that, in cases which I have treated with it, after having used the more ordinary dressings previously, the honey most unquestionably removed all unpleasant smell from the discharges, itself no small gain. I must observe, however, that honey, when first applied especially, causes some considerable degree of pain, indicating its stimulating effects. In one case I used a wash of tinct. opii and water on first applying the honey dressing, poured over the fine linen which had been applied, saturated with fine clear pure honey. In all the cases, however, the patients themselves soon got reconciled to the pain, considering it indicative of benefit being received from the dressing. It is well, however, to bear this point in mind, especially in cases where a very large surface has to be dealt with, or a patient is very susceptible of or sensitive to pain. In such cases, glycerine, which possesses more soothing qualities, might be preferable. In conclusion, I would insist on the necessity of using pure clear honey quite free from any of the numerous articles said to be used in its adulteration."

10. *New Method of employing Coal Tar for Medicinal and Hygienic Purposes.*—M. DEMAUX, whose researches into the value of various disinfectants, conducted in conjunction with M. Corne, have acquired considerable reputation, has recently announced the discovery of a means by which coal-tar can be made up into the form of a soap.

This product, which, from the facility with which it may be prepared, from its moderate cost (which is less than sixpence for each pound), from the quantity of coal-tar which it contains, and from its ready solubility in water, appears destined to be of great service, is prepared in the following manner: Take of coal-tar, soap, and alcohol, equal parts, mix, and warm over a water-bath until entirely dissolved. Upon cooling, a perfect soap is obtained, which is very soluble in water and forms, upon being dissolved in that liquid, a permanent emulsion.

This preparation would be found very useful, whether in hospitals, in dissecting-rooms, or in manufactories, for the purpose of preventing real dangers to health, or of counteracting emanations which are both unpleasant and unwholesome.

The emulsion of coal-tar might be employed in baths or in lotions and fomentations to the body, and be productive of good results in certain diseases of the skin; body or bed-linen might be impregnated with it, and also dressings for patients whose excretions are of a fetid character. Its ready solubility in warm or cold water would prevent it from staining the body or the clothes.—*London Med. Review*, Feb., 1861.

11. *Solution of Gutta-percha in Chloroform.*—Dr. W. R. GORE (Surgeon to the City of Limerick Infirmary) extols this as an extremely useful preparation. It is made by putting some cut-up shreds of gutta-percha into a small bottle of chloroform, which soon dissolve, forming a solution thick in proportion to the quantity of the gutta-percha. A good syrupy or mucilaginous consistence is best. Whenever it is desirable to exclude the atmosphere from a wound, to preserve the skin, or otherwise form a protective coating, there is no similar agent of equal value. It can be laid on with a brush, layer after layer, to form any required degree of thickness; it forms an admirable bond of union, and is entirely protective. The skin must be quite dry, and if possible, warm and dry.

Warm cloth kept for a few moments on the part will aid the process. There must be no oozing. Where there is much surface of wound, it is advisable to protect it with a little isinglass plaster or a bit of lint from the smarting effects of the chloroform, which, however, pass off when the first coat dries. If the coats are put on in too rapid succession it does not consolidate nearly so rapidly, nor is it so effectual as a bond. It adheres to the dry skin for a long time. The approach of a warm iron at any time softens it to a removable consistence, or it peels off in due course naturally.—*Dublin Med. Press*, Jan. 23, 1861.

12. *Purifying and Scenting Cod-Liver Oil and Castor Oil.*—M. JEANNEL, calling to mind the remarkable effect which bitter almonds exert in removing the odour of so powerful a scent as musk, has made various experiments on the removal of the disagreeable odour and taste of cod-liver oil. The results are : 1. That the essential oil of bitter almonds in the proportion of seven and a half grains to twenty-five drachms of even the most infected oil, is completely successful: the dose of the essence requisite varying with the degree of the fetidity of the oil. 2. One grain of anhydrous hydrocyanic acid dissolved in water, will disinfect, but does not perfume, the same amount of oil. 3. Distilled laurel-water is, however, the best means of effecting the result, it sufficing to shake well in a bottle, the oil with once or twice its volume of distilled laurel-water, according to the strength of this, and the degree of infection of the oil. The liquids are then to be separated by a funnel after forty-eight hours' rest; and if the oil is not sufficiently clarified, it may be rendered limpid by filtering through paper. This confers upon even the brownest oil a slight and pleasant flavour of bitter almonds, which abides in the mouth. Of course it does not remove rancidity, which is a very different thing from the fishy odour and taste. Three drops of essence of bitter almonds communicate a very agreeable flavour to twenty-five drachms of even nauseous castor oil, and render its administration easy.—*Med. Times and Gaz.*, Dec. 29, 1860, from *Journ. de Pharm.*, 1860.

MEDICAL PATHOLOGY AND THERAPEUTICS, AND PRACTICAL MEDICINE.

13. *Restoration of Suspended Animation in Persons apparently drowned.*—A paper on this subject, by Dr. CHRISTIAN, was recently (Jan. 22, 1861) read before the Royal Medical and Chirurgical Society.

Two societies, the Royal Humane Society and the National Life-boat Institution, issue instructions, which are widely circulated, as to the best mode of restoring suspended animation in persons apparently drowned. These rules differ not only in detail, but in principle. 1st. As to the mode of performing artificial respiration. 2d. As to the propriety of using the warm bath. On each of these matters the author stated his desire to elicit an expression of opinion from the Fellows of the Society, after laying before them some considerable experience acquired during twelve years as Medical Officer to the Royal Humane Society in Hyde-park. The number of cases of submersion for twelve years was 443. Of these, 181 were rescued and recovered without treatment; 165 were brought to and recovered by treatment at the Receiving-house; and 97 were brought dead or the treatment was unsuccessful. The number of cases of submersion for the last four years was 140. Of these 68 were rescued and recovered without treatment; 38 were brought to Receiving-house and recovered; and 34 were brought dead or were not restored. 15 of these cases were treated by the Marshall Hall Method, and 3 recoveries followed: the rest were treated by the rules mentioned below. As to the mode of performing artificial respiration, the method recommended by the Life-boat Institution is what Dr. Marshall Hall called his "Ready Method," while that now used by the Royal Humane Society is the method of Dr. Silvester. On Dr. Marshall Hall bringing his method under the notice of the Royal Humane Society, the committee

adopted means immediately to give it a fair trial. Copies of his instructions were sent to all their medical officers, numbering 120, and the boats of the Society on the Serpentine had a platform made to each, on which to manipulate directly the body was taken from the water, and the boatmen were instructed and practised in the performance of the operations as he directs. After giving the method a full trial in about fifteen cases, the very intelligent superintendent, the boatmen, and the author became so satisfied of its inefficiency to restore animation, and of the difficulty of properly carrying out the manipulations, that he felt himself justified in representing those facts to the committee, and in adopting the plan recommended by Dr. Silvester, which he believed in every way to be superior, more manageable, less likely to injure the patient, will fill the chest with and expel air from it more fully, and will not force the contents of the stomach upwards, and in the way of respiration. The following are the directions for treating the asphyxiated at the Receiving-house, Hyde Park :— “ Wipe the mouth and nostrils directly the body is taken from the water. Use Dr. Silvester’s method; at the same time let the body be taken as quickly as possible, to the Receiving-house, and place it in the bath up to the neck. Raise the body in 20 seconds from the water, and dash cold water against the chest. Pass ammonia under the nose. Use again Dr. Silvester’s method, and the inflating apparatus if it fail. Remove the body from the bath, and rub the surface with dry hot towels, perseveringly continuing the other treatment.” After many experiments the author had come to the conclusion, that inflation of the lungs by Dr. Silvester’s method, or by the Society’s apparatus, is the first remedy, and the shock of the warm bath the second; that, after eight minutes’ complete submersion, recovery is hopeless, and that, when ten minutes elapse after being taken from the water without any effort at respiration, it is equally so. On the subject of the warm bath, which has excited so much discussion as a remedy, he remarked, that it must be understood that it is used as an immediate and powerful excitant; and it had so frequently happened (twice while he was actually present) that a person brought in as asphyxiated, who to the bystanders was apparently quite dead, immediately on being placed in the bath gave the sob or gasp which is the precursor of respiration, that it might be boldly stated to be a most valuable adjunct to treatment, and, properly managed, in no way pernicious. He concluded by citing a letter from Sir Benjamin Brodie to the Royal Humane Society, confirming the preceding conclusions.

Dr. Sharpey said that he had on one occasion spoken favourably of Dr. Marshall Hall’s method, but after more mature consideration and after hearing the practical experience of the officers of the Royal Humane Society, he had found reason to alter his opinion. Dr. Sharpey considered that Dr. Marshall Hall’s method could only claim one advantage; and it was not clear that it had even that. The supposed advantage being that the tongue falls forward and thus does not embarrass respiration. He thought that Dr. Silvester’s method attained this object without any of the disadvantages of the “ready method.” The disadvantages of the “ready method” were several. It must be remembered that a body which has been some time submerged is an insensible and inert body, and serious mischief has not unfrequently arisen from rough handling. Again, the constant turning of the body, renders it very difficult to apply warmth, or carry out the other auxiliary means systematically, but the greatest objection is, that it does not even fulfil its professed object, viz., that of changing the air in the lungs. Dr. Marshall Hall had cited experiments in support of his views; but the want of precision in making them was very apparent from his description. He (Dr. Sharpey) could not attach any importance to the results of experiments so loosely conducted. Dr. Silvester had repeated them with greater care, and did not succeed in getting a displacement of more than a cubic inch of air. It would be said, however, What answer should be made to the many statements of the success of the ready method? He would reply that the subjects of many of these cases were still-born infants, and probably would have recovered without any assistance if left to themselves. Then, again, there is no air to be displaced in the lungs of infants. In reference to adults, he (Dr. Sharpey) believed himself right in saying that in many of the cases of recovery after submersion, respiration commences spontaneously soon after the

patient reaches the air. If in such cases Dr. Marshall Hall's method were begun at once, it would be unfair to give the credit solely to it. May it not even be, as suggested by Sir Benjamin Brodie, that recovery often follows, not from the means, but in spite of them? Dr. Sharpey considered that there were just grounds for reviving the old method of insufflation, which was, he thought, given up without sufficient reason. The objection to this plan was that it produced emphysema; but then, as he knew by experiment, this was from forcing in air too quickly and in too great quantity. Dr. Sharpey then alluded to the various instruments which might be used in insufflation. In reply to the objection that often insufflation could not be practised from the want of instruments, etc., he would remark that there could be no reason why the insufflation should not be carried on with the mouth, through a pipe put into one nostril of the patient. A card, or letter-envelop, might be made into a pipe till better was got. The force and pressure used would be no more than that sustained by the operator's own lung. He believed that emphysema in reference to these cases was a bugbear. Dr. Christian had observed that in unsuccessful cases in which insufflation had been practised, no emphysema was found at the autopsy; and a friend had told him (Dr. Sharpey) that he had never found emphysema in the lungs of still-born children who had been unsuccessfully treated by insufflation at the Edinburgh Royal Maternity Charity. The objection that the air would be deficient in oxygen was not practically valid, as the operator might, by several deep inspirations, quite change the air in his lungs, and then, by employing his chest for insufflation quickly after an inspiration, he would not leave time for any considerable change in it. There was an outcry against the warm-bath, founded on mere speculation. The Royal Humane Society's officers had found it to be very efficacious. Edwards and Dr. Brown-Séquard made their experiments on puppies and kittens, and their experiments were not, he believed, to be fairly compared with the use of the warm-bath in cases of suspended animation.

Dr. SIBSON had recently been engaged in prosecuting experiments on the restoration of suspended animation by the "ready method." They were only eight in number, and were not yet complete. In only one of them did the "ready method" appear to answer. The experiments were not complete, and he did not therefore speak decidedly from them. So far, however, they did not bear out Dr. Marshall Hall's plan. There was very little change produced in respiration, but more than one cubic inch—generally two or three. Dr. Sibson believed that emphysema did occasionally happen, and alluded to three cases of restoration of suspended animation, in one of which it had occurred. He then alluded to a case in which dislocation and fracture between the fourth and sixth cervical vertebræ had been produced by, he believed, zealously, but too roughly, carrying out Marshall Hall's method. The result of the injury was complete paralysis below the seat of injury.

Mr. SPENCER WELLS believed that insufflation from the mouth was the best method, and that it was best accomplished by passing a tube through the nostril into the glottis.

The PRESIDENT remarked on the importance of the subject, which made it a matter of great regret that such a wide difference of opinion should still prevail.—*Med. Times and Gaz.*, Feb. 2, 1861.

14. *Palliative Treatment of Asthma*.—M. T. L. PRIDHAM, in an interesting paper on the treatment of asthma (*British Medical Journal*, Nov. 10, 17, and Dec. 22, 29, 1860), mentions the following as the most effectual palliative remedies which he has employed, though he states that no one of them is to be relied on in any second attack, for what succeeds to-day may fail to-morrow:—

"The first on the list is stramonium, the fumes of which may be collected in an inverted glass bowl with a narrow mouth; the bowl being charged to its full is placed under the mouth of the patient, who is directed to inhale to the fullest extent in his power the smoke which has been collected in the bowl, taking care to hold his head away from the bowl when an expiration takes place. Chloroform, both taken internally or inhaled, is a powerful remedy, but it must be employed with caution, and never administered except by a medical attendant.

The fumes of nitre paper in a state of ignition, well inhaled, is often a valuable remedy. Care should be taken to procure the best prepared from a good chemist. Chloric ether and the tincture of the lobelia inflata will occasionally relieve. Bicarbonate of soda, as well as chlorate of potass, given in full doses, I have frequently seen produce a good effect. Again, I have seen repeated doses of sulphate of alum procure relief, the powder being allowed to dissolve on the tongue before it is swallowed, in ten grain doses. I have also seen the fumes of tobacco, inhaled as I have recommended in the use of stramonium, relieve, when other remedies have failed; but I do not like this remedy, it produces such deadly faintness and nausea. Small drinks of the best Mocha coffee, made strong, will often procure relief. On two occasions, when every other remedy failed, I succeeded in procuring almost instant relief, by injecting two grains of morphia and a drachm of tincture of assafoetida. These were cases where mental distress appeared to be the exciting cause.

"I have often sat at the bedside of one, suffering from the severest form of the disease, watching with great anxiety the result of prescribed remedies, and it has not unfrequently happened that many have been tried without relief, the patient all this time gasping for life with sufferings the most intense, when relief has at length come from a remedy apparently the most unlikely to procure it—so capricious is the disease, and so uncertain the remedy in asthma cases of this particular character."

15. *Sore Throat*.—Dr. THOMAS INMAN, in a recent paper (*British Medical Journal*, Jan. 26, 1861), maintains that the pain of sore throat and dysphagia is due to the muscles of deglutition being unusually irritable or inflamed, and in the treatment of these affections regard must be had to these muscles, as well as to the condition of the mucous membrane covering them. The new plan of treatment does not materially militate against the old, and the two may always be combined in one way or another.

He adds that the anodyne treatment is applicable to all forms of cynanche, whether it is catarrhal, scarlatinal, diphtheritic, syphilitic, or croupy; and that he has not yet found the laudanum act prejudicially in any case.

Acting on this idea, Dr. I. states that he has used opiate epithems externally for some time, and a few weeks ago began to use laudanum and water as a gargle. The effects have surpassed his expectations, and no case could more fully corroborate his views than the following: Mr. C., aged 30, after being much exposed to scarlatina, had ulcerated sore throat; this was so exquisitely painful, that he was unwilling to swallow anything; and he could not sleep, for he was constantly awakened by involuntary efforts of deglutition, which occurred as soon as he began to be oblivious; and he could not even speak intelligibly. Dr. I. used no internal application whatever but a gargle of laudanum and water. The patient reported the effect as magical, for he was able both to speak and swallow comfortably as soon as he had finished gargling; the ulceration remained, the soreness had gone. He had only to repeat the application three times in all. The only adjunct Dr. I. adopted was the use of iodine paint externally.

16. *Perchloride of Iron in Diphtheria*.—M. AUBRUN, in a communication to the *Académie des Sciences*, states that the greatest success has attended his administration of the perchloride of iron in diphtheria and croup. He gives the doses in rather a vague manner, stating that he adds from 20 to 40 drops of the solution of the perchloride—according to the severity of the disease and the age of the patient—to a glass of water, and causes the patient to take about two teaspoonfuls every five minutes during the day and every quarter of an hour during the night. Immediately after each dose of the perchloride some cold milk, without sugar, is given. This treatment must be scrupulously followed for some days, without regard to the patient's sleep for the first three days. It is not until after the third day that the false membranes begin to soften and separate. The author considers that from 140 to 350 drops of the solution, representing from $1\frac{1}{2}$ to $4\frac{1}{2}$ drachms of the perchloride should be taken during each 24 hours; and he carefully avoids giving any article of diet likely to decompose it.—*Union Med.*, No. 146.

17. *Severe Primitive Gangrenous Angina successfully treated by Perchloride of Iron.*—Dr. HENRY MUSSET relates a case of gangrenous disease of the back of the mouth which resisted all other remedies, but yielded to the employment of perchloride of iron. This medicine was administered in a draught at periodical intervals, and under its use the fever abated, the delirium ceased, and a large patch of ecchymosis which had appeared on the right side of the chest began to grow pale. Broth and wine were then administered, and every day the improvement continued; the breath was no longer fetid, the tissues became clean, and at length the patient, who had appeared moribund, was restored to life. Dr. Musset attributes the recovery entirely to the use of the perchloride, because it was the only medicine employed internally from the time when the worst symptoms appeared, and because the improvement continued regularly as soon as its administration was begun. With regard to the patch of ecchymosis which appeared on the chest, the perchloride seems to have acted beneficially in removing it, whether this interstitial hemorrhage was produced by the gangrene having destroyed some vein, or whether it was the consequence of an alteration of the fluids under the influence of a general cause. It is certain that under the influence of the perchloride of iron both the gangrenous disease of the throat and the ecchymosis on the chest were completely cured.—*B. and F. Med.-Chir. Rev.*, Jan., 1861, from *L'Union Méd.*, Sept. 6, 1860.

18. *Alcohol as an Abortive Agent in Intermittent Fevers.*—Dr. JULES GUYOT proposed to treat the epidemic cholera in 1849 by the administration of alcohol, and the disease was often arrested by the adoption of this plan, which was carried out with still greater success in the epidemic of the same disease in 1854. As Dr. Guyot regards cholera as a species of fever, he proposes the same treatment in intermittent fevers, the alcohol being administered at the commencement of the paroxysm. Several cases are recorded in which this mode of treatment was successful. In one case, a man was attacked with a tertian fever, and two paroxysms were allowed to pass without treatment; but at the commencement of the third, just when the cold shivering was most developed, two small glasses of rum were administered to him with immediate relief; he became warm and felt better, and a third glass of rum was given. In half an hour afterwards the patient was dressed and walking out in the air, and the paroxysm never returned. Several other cases were treated in the same manner with equal success. In the case of a gentleman who had contracted an intermittent fever in Africa, and who came on a visit to Dr. Guyot, the paroxysm was arrested by the same means. Dr. Guyot's treatment of intermittent fever is explained in the advice given to this gentleman when consulting Dr. Guyot for his complaint. "If," said Dr. Guyot, "you have still eight hours before the commencement of the paroxysm, take twenty centigrammes of sulphate of quinine, together with a cup of warm infusion of chamomile, and in two hours repeat the dose. If you have only three hours, take three cups of strong coffee, and put your feet before a good warm fire. Lastly, if you have no time, or if you are not sure of the hour when the invasion of the paroxysm will happen, allow it to come on, and as soon as you are certain that it is fully developed, take two small glasses of rum, and five or ten minutes afterwards take a third." The last alternative was adopted, and with complete success.—*Brit. and For. Med.-Chir. Rev.*, Jan., 1861, from *L'Union Méd.*, Sept. 11, 1860.

19. *Topical Febrifuge.*—M. de Castelnau, editor of the *Moniteur des Sciences Méd. et Pharm.*, states in his No. for 23d Feb. 1861, that he has received from M. SÉZÉRIE an elaborate memoir, containing a great number of cases demonstrative of the efficacy of the following topical application in intermittent fevers of every type: R.—Essence of turpentine, 125 grammes; Sydenham's laudanum, 6 grammes; camphor, 3 grammes; olive oil, 60 grammes.—M. So soon as the pyrexia commences, the whole length of the spine is to be rubbed with this liniment for about ten minutes, and the application to be repeated every six hours until the next paroxysm. The next paroxysm is generally much slighter, and, after the third or fourth does not recur. It is well, however, as a matter of precaution, to repeat the frictions a few times after the cessation of the paroxysms.

20. *Apiol in Amenorrhœa and Dysmenorrhœa.*—By M. JORET. During the investigations into the antiperiodic power of apiol, carried on some years since by the author and M. Homolle, they had occasion to note its emmenagogue power, and subsequent trials have proved it to be not only one of the most active and most certain, but also a very harmless emmenagogue. In his preliminary observations, the author strongly insists upon the important precept that the treatment of amenorrhœa and dysmenorrhœa must be based upon an exact appreciation of the cause, thus: 1. When the cause is plethora, general or local bleeding, baths and emollients are the best emmenagogues. 2. When it depends upon a chloro-anæmic condition, whether spontaneous or accidental, martial preparations, good diet, a healthy abode, and exercise in the open air are the means most likely to act in restoring the menstrual flux. 3. When the disturbance of menstruation is connected with general nervous excitability, prolonged cold baths should predominate in the treatment. 4. But when the cause is a deficiency of general nervous stimulus, or of that localized in the genito-uterine apparatus, a perversion in the vitality of this apparatus, and especially of the ovarian sensibility, the stimulants termed emmenagogues are indicated; and it is in these cases, which are very numerous in practice, that apiol seems to possess decided superiority to any other means. 5. In those complex cases, in which several causes seem to combine in the production of the amenorrhœa or dysmenorrhœa, we should combat the various general or local conditions already adverted to before having recourse to emmenagogues.

Not only must we first investigate the nature of the cause of the amenorrhœa or dysmenorrhœa, but also determine the exact time at which the emmenagogue should be administered. This should be always at the period at which menstruation ought to return. There may be difficulty in exactly fixing this, especially when the menses have been long absent: but there are almost always symptoms arising from a congested state of the uterus which will sufficiently fix the period for commencing the remedy. The apiol may be given in capsules, each capsule containing four grains of apiol. Generally a capsule is given morning and evening, with a little sugared water. These are continued during the four or five days of the menstrual epoch. The month after, and even the third month, if menstruation has not become sufficiently abundant and quite regular, exactly the same procedure is to be observed. As a general rule, the menses appear more or less abundantly after the first course of doses, and it is rare for the medicine to be required to be taken after the third month. In dysmenorrhœa precisely the same course is to be observed. M. Joret quotes several cases exhibiting the decided efficacy of the apiol in primary amenorrhœa, or deficiency of secretion; in accidental amenorrhœa, or suppression, and in dysmenorrhœa.—*Med. Times and Gaz.*, Jan. 26, 1861, from *Bulletin de Thérap.*, tome lix.

21. *Analysis of fifty-two Cases of Epilepsy.*—Dr. EDWARD H. SIEVEKING presented to the Royal Medical and Chirurgical Society (Feb. 12, 1861) a very interesting analysis of 52 cases of genuine epilepsy observed by himself. This was the second contribution of the same character as the one admitted into the *Transactions* of the society for 1857, and the author has limited the analysis to the same number of cases, taken in the order of observation, as had been subjected to analysis on the former occasion. Only those points were brought forward on which satisfactory evidence could be obtained. *Sex.*—23 were females, or 44.2 per cent.; 29 were males, or 55.8 per cent. Taking the two series together, the ratio of females to males was as 45.2 to 54.8 *Age.*—The following was the distribution throughout the different periods of life: Under 10 years, 12 cases; from 11 to 20, 25 cases; from 21 to 30, 7 cases; from 31 to 40, 3 cases; from 41 to 50, 2 cases; above 51, 3 cases. The basis of this calculation is the time at which the epilepsy first showed itself. Arranged according to sex, we find during the first decennium 8 males and 4 females; during the second, 11 males and 14 females; during the third, 5 males and 2 females; during the fourth, 3 males; during the fifth, 1 male and 1 female; during the sixth, 1 male and 2 females. *Causes.*—An hereditary taint was traceable in 14 cases, but in 8 only of these was there evidence of epilepsy having occurred in a near relative of the patient.

The exciting causes were traced in 37 cases as follows: uterine derangement, 9 cases; venereal excesses, including masturbation, 6 cases; fright, 4 cases; overwork, 4 cases; anxiety, 3 cases; dyspepsia, 3 cases; dentition, 2 cases; scarlet fever poison, 2 cases (doubtful); meningitis, anæmia, blood-poison, and the development of puberty, each 1 case. *Premonitory Symptoms*.—Comprising under the term “aura” all symptoms indicative of a near approach of a paroxysm, the author found it in 21 cases, or forty per cent., or less frequently than it was met with in the first series, where 52 per cent. of the cases exhibited premonitory signs. *Headache*.—Headache is very commonly associated with epilepsy, but its significance varies much according to its period of occurrence; it may be habitual, or it may be connected with the attacks only as a precursor of a sequela. It was constant or frequent in 9 cases, 17.3 per cent.; it occurred after the seizures only in 12 cases, or 23.0 per cent.; it occurred only immediately before or after the fits in 3 cases, or 5.7 per cent. *Biting the Tongue*.—Although a large number of genuine cases of epilepsy occur in which this symptom is never met with, it is important as a corroborative symptom. In the first series it was met with only in 32.7 per cent.; in the present, 28 patients, or 53.8 per cent., exhibited this feature. It did not appear that the female sex, as might have been anticipated, were less prone to inflicting this injury upon themselves than males; because of the 24 cases in which the tongue had not been bitten, 16 were males and 8 females. *Urine*.—The author has found no constant derangement in the urine associated with epilepsy. He has always failed to discover any sugar, nor has he met with a constant or even frequent excess of phosphates, or a diminution of urea. In several cases he found the urine presenting a specific gravity ranging about 1030, and containing a large excess of urea. There was in the present series no case of albuminuria, except, perhaps, in one instance, in which a small quantity of albumen appeared to be present for a brief period. An excess of phosphates was observed twice, oxalates in one or two cases, and an excessive deposit of lithates in a small number of cases. *Treatment*.—The author, while urging the value of treatment in alleviating the disease and indefinitely postponing the seizures in a large number of cases, admitted the doubt which always attached to any absolute cure of epilepsy. He advocated no specific, but the employment of all rational means indicated by the constitution and peculiar symptoms of each individual case. Above all, he considered perseverance in a combination of moral, regiminal, and medical treatment essential. As many of the cases analyzed had not been under the author's care permanently, he did not regard the results as to treatment indicative of what might have been effected in the whole series. He considered that he could lay claim to eight cures, that fifteen were decidedly benefited, while the remainder were either uninfluenced by treatment or did not continue under observation for a sufficient period to justify any positive statement as to the result.—*Med. Times and Gaz.*, Feb. 23, 1861.

22. *Embolia and Thrombosis*.—It is now nearly fourteen years since the theory of the detachment of fragments of vegetations from the valves of the heart, and from coagula in the large veins, and of the consequent secondary occlusion of the minute arteries, was first propounded by Virchow. The term *emboli* has been applied to the small plugs detached from the valves of the heart; *thrombi* to those derived from the veins. The effects produced in the different organs by these obstructions, and the entire doctrine of embolia and thrombosis, have of late been the objects of considerable investigation. A most elaborate work upon the subject has recently been published by Professor Cohn, of Berlin (*Klinik der embolischen Gefasskrankheiten mit besonderer Rücksicht auf die ärztliche Praxis*, 8vo., Berlin, 1860 p. 696). Still more recently, an able memoir on the same subject, and a review of the entire doctrine of embolia have been published in the first number of *Schmidt's Jahrbücher* for the present year, by Dr. H. MEISSNER, of Leipzig. He considers the subject under the following heads:—

1. Obstructions of the cerebral arteries.
2. Obstructions of the veins and sinuses of the dura mater.
3. Obstructions of the aorta and arteries of the lower extremities.

4. Obstructions of the pulmonary arteries.

5. Obstructions of the gastro-epiploic, splenic, and other abdominal arteries.

Particular attention is devoted to emboli of the cerebral arteries, of which the author has collected 32 cases from various sources. From these cases, the following conclusions are drawn:—

1. Both sexes are equally liable to cerebral emboli. Of the 32 cases, 15 were females, and 17 males.

2. As to age, eleven of the cases were between 14 and 30 years of age; ten, between 30 and 40; five, between 40 and 50; and six, between 50 and 60.

3. The left half of the brain (16 cases) was more frequently affected than the right (11 cases). In 5 of the cases, the obstruction was in the middle line, and both sides of the brain were affected.

4. The most frequent site of the obstruction was in the cerebral carotid (nine times in one, and twice in both); the next, in the profunda cerebri or posterior cerebral artery (8 cases); and the next, in the artery of the fissure of Sylvius (7 cases). It was in the basilar artery in 4 cases; in the vertebral in 2 (in one of these, both vertebral arteries were blocked up); and in the arteries of the corpus callosum, in 2 cases.

5. Softening of the corresponding parts of the brain almost universally follows the obstruction. In 2 only of the cases, was there merely hyperæmia of the cerebral matter; and in 2 others no change whatever could be discovered.

6. In all but two cases, there was paralysis of the side of the body opposite to that of the obstruction.

7. Loss of consciousness during the attack was only observed twelve times.

8. The attacks were almost invariably sudden (30 cases). In 2 cases only had there been premonitory symptoms. Eight times, repeated attacks of paralysis had occurred. In one case only was the commencement of the paralysis gradual; while, in one other, its mode of commencement was unknown.

9. The heart was diseased in 25 cases. In 22 cases there was disease of its walls or valves; in 2 there was merely hypertrophy; and in one case there was an aneurism of the ascending aorta. In 7 cases, nothing abnormal could be detected in the heart.

10. In 15 cases there were obstructions of vessels in other parts of the body, or infarctions.

11. Among the coexisting diseases, phthisis, syphilis, and alcoholism were the most frequent.

From these cases, collected by Dr. Meissner, it is obvious, that the so-called emboli of the cerebral arteries cannot always be derived from vegetations on the valves of the heart. Cases are quoted from Bristowe, Jenner, Tüngel, and Hasse, where there were all the ordinary appearances of emboli of the cerebral arteries, but where the heart was perfectly healthy, and where no other source could be discovered in the body, from which clots could have been detached. In such cases, the lesion must be attributed to a local coagulation or spontaneous arteritis.

In the case of the pulmonary arteries, the plugs may be derived from coagula in the larger veins, or the coagulation may be of a local character, and result from a morbid condition of the blood, from dilatation of the right side of the heart, a firm fatty degeneration of its muscular tissue. Either of the two last mentioned causes will impede the progress of the blood through the lungs, and so favour its coagulation and the development of obstructions.—*British Med. Journ.*, Feb. 2, 1861, from *Schmidt's Jahrbücher*, 1861, No. 1.

23. *Remarkable Case of Hydrophobia*.—Dr. W. R. GORE relates (*Dublin Med. Press*, Jan. 9, 1861) the following remarkable case:—

“Some time since I was called to see a man who was reported to be in a state of ‘great excitement, labouring under fits of violent screeching.’ On entering the room where he was, I found a large number of persons, but not near the bed. The man’s wife was endeavouring to hold a stout middle-aged person, who took a terrific fit of screeching, as I entered the room. His aspect was wild, and he grasped violently at the wife when the fit came on. Having got rid of the visitors, I commenced to explore the case, during which I was repeatedly inter-

rupted by the violence of the fit and the intensity of the screeching. In the intervals he was calm and rational. The *cause* of the fit he attributed to an excruciating agony which, at each time, suddenly seized upon his *penis*, throwing him into the excitement described. He declared he had no other disease, and that if this was not cured he must soon die, 'he could not stand the pain.' I examined the parts, but they were perfectly healthy. During the process he had two attacks, but they produced no physical alteration in the organ. During one of these he attempted to seize and bite me, but I escaped his grasp. He made no such attempt upon the wife. I was puzzled to account for these strange symptoms, there being no other to give me a clue to the cause of them at the moment. I recollected the countenance of the man, but he made no reference to any previous acquaintance with me. Having called in some of his male friends to learn somewhat of his history, one asked me if I did not remember the man who had been some fourteen months previously under my care in the infirmary with a wound of the leg from a bite of a mad dog. The case was soon revealed. Returning one evening from the Messrs. Fitt's brewery, where he was employed, he was seized in Peter Street by a dog said to be mad. He was brought to the infirmary, where the part was burned well with caustic, and though he remained there for two months it did not heal sufficiently to permit him to work. He spent five or six months in the workhouse hospital with similar results; but the sore became so chronic he returned to his usual avocation, carting with an ass and car. He daily continued this avocation for six or seven months without any sickness, save a chronic ulcer of the left leg, which he regularly dressed with ointment.

"Early on the morning of the day I saw him, he was suddenly and unexpectedly seized with a fit such as I have described. He refused all nourishment. I visited him about two P. M. Having made myself master of these facts, I proceeded to test the case. Every time I approached him with a drink, or that he saw the bowl of water, he got a violent spasm, screeched, thrust his hands to seize the penis, and shivered, with contraction of the mouth and jaws. The Rev. Mr. Fitzgerald, C. C., having entered the room at this time, I held with him a consultation as to the best and safest course to be adopted to insure safety and prevent terror and annoyance to other patients. We agreed that a lunatic ward in the workhouse was the proper place for his treatment under the circumstances. The clergyman sent for a covered car, and wrote to the excellent physician of that establishment, Dr. Brodie, under whose care he was soon placed. Notwithstanding the most energetic treatment, scarcely twenty-four hours elapsed before death put an end to his sufferings. Incessant spitting, extreme terror, violent convulsions, wild delirium, were succeeded by sudden prostration and death. The sore on his leg never healed. He had no recognizable premonitory stage, none of the pharyngeal spasm, except when attempting to drink, and the characteristic anguish was transferred by some sort of reflex action to the genitals with electric intensity. These peculiarities render the case remarkable and worthy of record. The bite was a severe one, the parts much torn, nitrate of silver and the knife the first remedies employed, every effort completely to cicatrize the sore failed, and the issue took fourteen months or so to smoulder before developing itself in a rapid and tragical death."

24. *On the Causes of True Leprosy.*—Mr. W. E. C. NOURSE read a paper on this subject before the Royal Medico-Chirurgical Society (Feb. 12, 1861). The author, having seen this disease at Bergen and elsewhere, first adverted to the well-known fact that leprosy was a blood disease, or dyscrasia; that an altered condition of the blood, demonstrable by chemical analysis, accompanied the premonitory symptoms which preceded every fresh appearance of leprosy blotches, both in the tubercular and in the anæsthetic form of leprosy, the blood at such times containing excess of albumen and fibrin; and that when the blotches were fully developed, the blood was found to be restored to its usual condition. Since, then, a changed condition of the blood was the proximate cause of leprosy, it was natural to look to the well-known agents concerned in making blood for the next link in the chain of causes of the disease. And here there was abundant evidence. Leprosy was frequent in Iceland, where the people inhabited small,

close, dimly-lighted huts; were dirty in their personal habits; breathed a moist marine atmosphere; often had their garments wet; and lived on inferior food, badly cured. In Norway, where the food was inferior and too long kept, leprosy occurred; and on part of the west coast, where the food was unusually bad—where the people inhabited small, dirty, unventilated huts in damp and cold situations, and often had their garments drenched with rain or salt water—leprosy was peculiarly frequent. During the middle ages, when population had become too numerous to be sustained by the produce of the chase, and before the art of producing large supplies of good food was understood, leprosy had been common in all parts of Europe; and it had disappeared precisely as more copious and better supplies of fresh meat, vegetables, and bread were produced. It had then vanished first from England, then from Ireland and Scotland, and more recently from the Isles. On the Continent it had followed a similar rule, and the disease now only remained in particular maritime situations, where the standard of living was miserably low, the habits of the people dirty, and their dwellings small, dark, and ill-ventilated. The author believed that these conditions would always be found to coexist with it, and that it would be endemic wherever they obtained, irrespective of latitude. In Egypt, cases of leprosy were occasionally seen; and some of the Egyptians lived in a miserable squalid manner, and in small dirty huts. In Palestine, lepers were still found among the very poor, distinguished by want and dirt. Dr. Webster's account of the leprosy in Spain was precisely to the same effect, and he had shown that it prevailed in damp maritime districts, among people living in a poor miserable way, on bad and decaying food, and in close dirty huts. Mr. Nourse dwelt on these circumstances as the main causes originating the disease, and concluded by expressing his belief in its hereditary transmission.—*Med. Times and Gazette*, Feb. 23, 1861.

25. *Hypochondriacal Insanity as a Precursor of General Paralysis.* By Dr. BAILLARGER, of the Salpêtrière, Paris.—General paralysis is a common and most serious phase of mental disease. It attacks patients of all ages, and its progress towards a fatal termination exhibits stages of the most melancholy and humiliating nature. All medical men accord it as being insidious in its approach. It may be long in becoming fully developed, presenting at first only the most trivial indications, in many cases so trifling as to pass altogether unobserved; and when the malady does at last attract attention, it may be too late for arresting its advance. It is therefore most important to attend to this disease at the very first; and it is with this object that it seems useful to describe the intimate relation existing between the hypochondriacal form of melancholia and general paralysis.

This relation being understood, it becomes one means of detecting the advent of that disease at the very commencement of its attack. It is of importance to distinguish this symptom, as the melancholy accompanying general paralysis very much resembles melancholia in its simple form. The conceptions or illusions of the hypochondriac, however, although of considerable variety, are yet of such a tendency as often to present something of a special character in their nature. The patients believe that their various organs are changed, destroyed, or completely obstructed; they pretend, for example, to have no mouth, no abdomen, no blood—that their gullet is stopped up, their stomach quite full, their bowels shut up; they imagine that their food passes from its ordinary channels—that it gets into their skin, or even their clothes. Four patients believed their body to have become putrid. Many among such are afflicted with hallucinations of smells. Some keep their eyes closed, and allege they are blind; others cease to speak, and state afterwards that it was impossible for them to open their mouth; they assert that they cannot swallow, nor defecate, nor make water; they affirm that their members are altered—that they are larger or smaller; they say they do not exist, or even go so far as to believe themselves dead; they remain motionless, the eyes shut, and when their limbs are lifted they let them fall, as if completely paralyzed. These different delusions lead to serious consequences; many of the patients refuse, more or less obstinately, to

take food, and sometimes it becomes necessary to feed them by means of the stomach-pump; and such patients speedily become much emaciated. I have seen, says M. Baillarger, a lunatic die in eight days from obstinately resisting the employment of the stomach-pump, under the impression that his stomach was completely full, and his gullet obstructed. One patient pretended he could not make water, and used every effort to retain it; his bladder became enormously distended, and he was at last attacked by a veritable retention, and it was with great difficulty the catheter could be used. In the end a false passage was made, and the patient died, while yet in the first period of the disease.

The tendency to gangrene, which is one of the characteristics of general paralysis in its latter stages, exists in these cases markedly, and before its usual period. Four cases had large eschars over the sacrum, without ever having been confined to bed; one woman, who for a year had exhibited all the symptoms of commencing general paralysis, preserved every appearance of health otherwise, when, all of a sudden, she became affected with hypochondriacal melancholia, and six weeks afterwards died of gangrene in both feet.

Hypochondriacal delirium is thus not only a mere premonitory symptom of certain forms of paralysis, but it is a serious symptom, and one very unfavourable, in prognosis.

In reference to this affection—viz., that of hypochondriacal insanity—viewed as one of the precursors of general paralysis, this being the fact of most practical value in connection with it, the delusions of which we have spoken seem to claim especial attention, as they are sometimes to be detected in patients as yet evincing no indication of paralysis—this supervening at a later period. Such a termination is certainly not invariable; but it is so common after this symptom, and the prognosis in such cases is so unfavourable, that considerable importance seems to attach to the subject. Thus Dr. Combes published some remarks on a case of “*Lypémanie*,” with stupor, and other serious symptoms—nothing, however, indicating that at a later period this patient should be attacked with paralysis; and, after fifteen months’ residence in this asylum, where he was treated, he was dismissed as cured. In reading Dr. Combes’ remarks, I was struck, observes M. Baillarger, with certain of the delusions affecting this patient. He had believed that he was about to die, if indeed not already dead; that his limbs were atrophied, that he had none, etc. These appearing to be good grounds for suspicion, I wrote Dr. Combes to know what had become of the patient. The answer confirmed my suspicions to be correct—the result having been that, after a year’s return to his occupations, he had been attacked with general paralysis. We may see by this example, that, had hypochondriacal delirium been held as a certain precursor of general paralysis, this affection might have been foretold two years before it actually took place.

It may appear strange that one form of insanity should thus be urged as premonitory of paralysis. Singular as it may seem, however, it is not the first time that such a doctrine has been urged. Since the writings of Bayle, no medical man doubts the fact of certain forms of insanity, such as the ambitious form, being symptomatic of approaching paralysis. And if one form of delirium be held, in mania or monomania, as indicating the advent of paralysis, there seems no reason why this particular hypochondriacal form should not serve the same purpose, and with equal certainty, in melancholia.

We do not attempt explaining these facts; and we may add, it seems useless to do so, either here or in the case of ambitious insanity. One point connected with the ambitious form may be mentioned; and that is, the relative frequency of general paralysis among females in different ranks of society. While this malady is equally common among males of all classes, among women it is not so. It is very common among the poor, and very rare among the rich. It would appear, however, that this circumstance has been forgotten by those who would explain the greater number of cases of ambitious insanity as induced by ideas of speculation—by the desire of suddenly arriving at honours and fortune.

In conclusion, it appears evident that hypochondriacal no less than ambitious insanity may, in different circumstances, be considered as a prognostic of general paralysis. The intention of the present paper has accordingly been to direct attention more particularly to the latter of these forms. As for the first, I have

frequently had occasion, before now, to refer to it in all its remarkable psychological characters.—*Ed. Med. Journ.*, Dec. 1860, from *Gazette des Hôpitaux*, Sept. 1860.

26. *Perversion of the Mental and Bodily Faculties as Premonitory Symptoms of General Paralysis.* By Dr. BRIERRE DE BOISMONT.—This paper, read at the meeting of the "*Académie des Sciences*," Sept. 24, 1860, may be considered as a sequel to that of M. Baillarger, read at its previous meeting, and just noticed. After alluding to the medico-legal aspects of such cases as are characterized by a tendency to theft and other criminal propensities, the author states that his observations have been carefully collected from a hundred examples falling under his own care, and respecting which he had already communicated to the *Société Médico-psychologique* those alterations in character and disposition throwing light upon the question under discussion.

According to his observation, the most frequent symptoms—in fact, what occur in three-fourths of such cases—are, great irritability, movements of impatience, anger, and violence. In a much smaller proportion of cases, the disease assumes, on the other hand, characters of indolence, and apathy, and gentleness. Such patients are reasonable and well-conducted; but between their words and actions there is an insurmountable discrepancy observable.

In place of either of these states of mind, or along with either of them, we frequently find perversion of the moral faculties; individuals who until then had been of unimpeachable character, suddenly becoming irreligious, immoral, or dishonest. These indications are important to be observed, as they are often unsuspected by any one as being connected with the diseased condition existing. The propensity to steal is perhaps, of all others, the commonest among this class of symptoms, and seems to some extent connected with that peculiar flow of spirits common in such patients, and evinced in their delusion that they are rich, powerful, and lords of all that they see—a state of mind which, in its results, sometimes entails the most painful consequences.

It is thus certain that the greatest change in the character and conduct is often observed in connection with general paralysis, giving rise to acts of an eccentric or reprehensible nature. Such acts are, no doubt, frequently to be met with as ordinary manifestations of the disposition, but their sudden and unaccountable accession results from mental disease, and is especially connected with general paralysis; they are its premonitory symptoms—*avant-courriers*—as they have been styled by Dr. Forbes Winslow in his work *On Obscure Diseases of the Mind and Brain*.

Our principal reliance in the diagnosis of such cases must rest on the general bearings of the disease. In the greatest number of instances where sudden alteration in character, disposition, and conduct, become apparent, there is reason to fear the accession of general paralysis; if the age is from 35 to 45 years, and excess of some kind, such as sexual or intellectual excess, and hereditary predisposition can be added, the prognosis becomes all the more certain.

Besides these characteristic symptoms, we must not lose sight of a very common occurrence connected with them—that is, the frequency of attacks of the nature of cerebral congestion. This may occur in the shape of a transient stunning sensation or giddiness, and pass off without attracting much notice; but it does so more commonly, and is of serious importance. Such congestions entail weakening of the intellectual faculties, loss of memory, and absence of mind. The mind loses its precision, its ordinary power. If the patient resume his occupation, and take to any work requiring application, the difference is at once observable in his capability of conducting it. His benevolence is greater than usual; there is a confidence betraying itself in his speech, which foretells the advent of insanity in its ambitious form. On the other hand, but less frequently, there is a state of dejection, the tendency to melancholia and hypochondriasis.

But the disorders of the muscular system are the key or touchstone for our guidance in this disease. One of these is of much importance, and manifests itself in a passing, transient trembling of the lips; a scarcely perceptible embarrassment in speech; a hesitation in pronouncing a certain word or letter, which

sometimes does not occur except at long intervals. Taken by itself, this symptom may not be determinative, although it is of great assistance; but if it be added to diminution of motive power, such as may be observed in asking the patient to grasp one's hand, or his own limb, the certitude becomes increased tenfold. To these symptoms may be added, inequality of the pupils, exaltation or failure of the sexual functions, diminution of the cutaneous sensibility, tremors of the muscles, and the results of analysis of the urine, and the indications afforded by means of electricity. We have also, in many cases, adds the author, observed paralysis of the sixth pair, amaurosis, and attacks of deafness, precede by several years the occurrence of general paralysis, and serve as the means for its prognosis.—*Ibid.*, Dec. 1860, from *Gazette des Hôpitaux*, Oct. 1860.

27. *On the Communication of Syphilis by Vaccination.*—M. VIENNOIS, of Lyons, published, last year, an interesting paper on this subject in the *Archives Gén. de Méd.* The following are his conclusions:—

1. Syphilis has in many instances been observed to follow vaccination, ever since the introduction of that operation, and by authors worthy of credit, French, English, German, Italian, &c.

2. When a syphilitic subject is vaccinated, in whom the disease is in a latent state, syphilitic symptoms may be developed by the vaccine influence; these symptoms often consist in general eruptions of a papular, vesicular, or pustular character, but a chancre never forms at the seat of the vaccinal puncture.

3. On the contrary, if a healthy subject be vaccinated with vaccine virus taken from a syphilitic subject, and the lancet be charged at the same time with a little blood, as well as vaccine matter, the two diseases may be conveyed by the same puncture—the vaccine with the vaccine matter, and syphilis with the syphilitic blood.

4. In these cases, of which a number are on record, vaccination is first developed because its period of incubation is shortest, and its evolution less rapid than that of syphilis. The latter appears subsequently, and manifests itself by its characteristic lesion at the inoculated spot.

5. The initial lesion, then, by which syphilis, following the vaccinal pustule, manifests itself, is an indurated ulcer, with adenitis; in a word, all the phenomena of primitive syphilitic chancre. The great law announced by M. Rollet, that syphilis always commences by a chancre, even when it results from secondary symptoms, or even from syphilitic blood, is thus fully confirmed.

6. After the primary chancre is developed at the inoculated spot, and in the usual period, secondary syphilis occurs, and runs the usual course, as if transmitted in any other way.

7. When the mixture of virus does not take place accidentally, but is effected intentionally (as practised by MM. Spéreno and Daumés, by mixing the vaccine matter with the pus of chancre), the result is the same; one virus does not destroy the other, but each runs its separate course.

8. The vaccine matter thus acts as a simple vehicle for the virus contained in the syphilitic blood, which it divides and dilutes, as a drop of water would do, without at all modifying its properties or its effects.

9. It is important, then, never to take the vaccine virus from a suspected person, or from an infant whose parents are unknown before the age at which hereditary syphilis usually manifests itself.

10. If circumstances make this last necessary, great care should be taken to collect only the vaccine matter, free from blood or any other syphilitic humour.

11. In no case should a healthy subject be vaccinated with matter taken from a syphilitic subject, for, in spite of all precautions, there can be no certainty as to the purity of the vaccine matter.

12. These precautions are the more important, because, with the matter from one syphilitic subject a number of persons may be vaccinated, and syphilis conveyed to nearly all (as seen by Ceriale, of Cremona).

13. It is sufficient to point out these precautions, to avoid new evils, and to remove the cavils of the enemies of vaccination; for, in these cases, the propagation of syphilis is not the fault of vaccination, but of the vaccinator.—*Gaz. Méd. de Paris*, Jan. 26, 1861.

SURGICAL PATHOLOGY AND THERAPEUTICS, AND OPERATIVE SURGERY.

28. *Contributions to the Subject of Compound Fracture; being an Analysis of 302 Cases.*—MR. THOMAS BRYANT read a paper on this subject before the Royal Medical and Chirurgical Society (Feb. 12, 1861). Having recognized the fact that the science of medicine and surgery was one purely of observation, and that its principles and practice could only be regarded as sound so long as they were based on facts, the result of recorded and carefully-observed phenomena, the author went on to state that he believed it to be the duty of every practitioner to communicate to his professional brethren any positive material he may possess, and thus to add his quota in building up the noble art and science to which we have the honour to belong. In conformity with such a principle he had the pleasure of presenting to the society a brief analysis of upwards of 300 cases of compound fracture of the extremities, omitting only those minor examples in which the smaller bones of the hands and feet were involved, believing that the results educed were not unfruitful either of interest or instruction. The cases were all taken from the experience of Guy's Hospital within the last twenty years the earlier ones having been collected from the records of the institution, and the author's own notes having furnished him with the particulars of every case admitted within its walls during the last seven years. He then proceeded to the analysis of the whole number—302 cases:—

17 or 5.6 per cent.	were of the thigh
193 or 63.9	“ “ leg
35 or 11.5	“ “ arm
57 or 18.8	“ “ forearm

11 out of the 17 cases of compound fracture of the thigh proved fatal, or 64.7 per cent. 74 out of 193 examples of compound fracture of the leg were fatal, or 38.3 per cent. 4 out of the 35 cases of compound fracture of the arm died, or 11.4 per cent. And 7, or 11.2 per cent. of the 57 cases of compound fracture of the forearm. Of the whole number of 302 cases, 96 were fatal, or 31.7 per cent. A table then followed showing the causes of death in these 96 cases, from which it appeared that in cases subjected to amputation pyæmia was twice as fatal as in others treated on conservative principles, and that exhaustion and gangrene were more common causes of death; that in cases treated on ordinary surgical conservative principles delirium tremens and tetanus were more common causes of a fatal result. Analyzing the 17 examples of compound fracture of the thigh—9 underwent primary amputation; 6 died, and 3 recovered. 1 underwent secondary amputation; fatal. 7 were treated on conservative principles; 4 died, and 3 recovered. 10 were thus treated by amputation, and 7 of these died, or 70 per cent. 7 were left to nature's efforts for repair, and of these 4 died, or 57 per cent. Having given an outline of the fatal cases, the author went on to remark, first, on the rarity of compound fracture of the thigh, the accident bearing the proportion only of 5.6 per cent. to the other cases involving the larger bones of the extremities. This fact, he stated, was well borne out by the experience of all surgeons. The excessive mortality of these cases was the second point to which he alluded, 64 per cent. of the whole number of cases proving fatal. In nearly 60 per cent. amputation was had recourse to, and 70 per cent. of these cases subsequently sank. The extreme severity of the majority of these cases, he remarked, rendered primary amputation absolutely essential. He showed that more than half the examples died, or 57 per cent., in which attempts had been made to save the limb; and that the cases in which recovery had taken place were in young and healthy subjects. The author passed on to review the opinions of some military surgeons on the subject, and pointed out how Dupuytren, Hennen, Larrey, Guthrie, and others all agreed that in compound fracture of the thigh from gunshot wounds, “in rejecting amputation, we lose more lives than we save limbs;” and also “that in the exceptional cases, which result in consolidation, the condition of the limb is not encouraging.” He quoted Macleod's Crimean experience as indicating the same opinion, this surgeon advising

amputation in all such cases when taking place in the middle and lower third of the thigh, and hesitating only in a like recommendation in cases of fracture of the upper third, on account of the extreme mortality of such amputations. Malgaigne's and Bauden's experience was then quoted to prove the poor success of conservative treatment in these injuries, the latter surgeon saving only 2 out of 25 cases, and these two retained useless and deformed limbs. The author then went on to say that the experience of civil surgeons was not unlike that of their military brethren, although it was much more limited; and expressed an opinion, which he believed to be generally entertained, that a satisfactory result can rarely be obtained by conservative treatment, except in the most favourable cases—that is, when the subjects are young and the fracture uncomplicated; when the soft parts are not materially damaged nor the bone comminuted. When the bone was comminuted and the soft parts seriously involved (such a condition being generally produced by local mechanical violence), he believed that a satisfactory termination of the case must be regarded with doubt. If the patient should be old or unhealthy, amputation should be at once performed; and if there should be a doubt as to which line of treatment to adopt, the safest is to decide on amputation. But if the subject should be a young and healthy one, and not subjected to injurious hygienic influences, the author believed that a satisfactory termination might be obtained by removing at once the broken and disconnected fragments of bone (enlarging the wound if necessary), and by maintaining the absolute repose which is so essential. He stated that he believed it to be bad practice to leave the broken fragments in position, with the hope of union; and that by doing so we left a constant source of irritation, which must retard the local process of repair, as well as seriously weaken the powers of the patient, a subsequent operation being almost necessarily required to remove what will become necrosed bone.—*Med. Times and Gaz.*, Feb. 23, 1861.

29. *Complicated Fracture of the Leg, followed by the Formation of a False Articulation; Cure effected by the use of a Seton.*—M. JOBERT DE LAMBALLE, at a late meeting of the Imperial Academy of Sciences, narrated the following case, which is of some value in both its physiological and pathological relations:—

A gentleman, forty-two years of age, was thrown from his carriage on the 30th of June. A severe direct fracture of the right leg was the result of this accident. Various applications were employed, including plaster, and starch bandages, but at the close of about three months it was ascertained that union of the ends of the fracture had not taken place. For two months subsequently the limb was put in an ingenious apparatus devised by M. Sention, which admitted of free motion of the limb whilst the fracture was kept immovable, an opening being left in front, so as to allow of its being easily painted with tincture of iodine.

At the expiration of this period no change had occurred in the condition of the patient, and he was desirous of submitting to an operation with a view to recovering the lost functions of the limb. A seton was accordingly introduced between the opposed ends of the fracture, the thread being brought into contact with the periosteum without touching the bony extremities; it was kept in position for ten days, and the suppuration became exhausted six days after its removal.

This operation was performed on the 17th of January; on the 12th of February the mobility between the fragments had become obscured, and at the end of the same month there was no longer any trace of it; and the patient was soon after able to walk.

Evidently, there was no exfoliation and no necrosis, in consequence of the application of the seton, so that the callus must have been necessarily formed under the influence of the excitation of the periosteum, an evident source, in similar circumstances, of all bony cicatrization, as has been demonstrated by M. Flourens.

The question is, consequently, not concerning a secondary callus resulting from the formation of granulations occurring after a superficial necrosis of the fragments, but rather of a cicatrix which has for its origin the deposit of a pro-

duct furnished by the enveloping membrane of the bones upon which has devolved the property of reproducing and regenerating them.

The duration of treatment was very different from, being much shorter than, that of patients in whom M. Jobert applied the seton by placing it next the surfaces of the extremities of the fragments, and this will be readily understood if we take into consideration the fact that the periosteum alone furnished the means of cicatrization, whilst in the other cases there were necrosis and granulation of the ends of the bone.—*London Med. Review*, Sept., 1860.

30. *Digital Compression in Aneurism*.—M. MIRAULT, of Angers, related to the Paris Surgical Society the following two cases of aneurism, in which digital compression had been successfully employed:—

1. A man, aged 23, exhibited an aneurismal tumour at the bend of the arm, some time after being bled. When admitted into the hospital this tumour was about the size of half an egg. Digital compression was made on the brachial artery, at about the middle of its course, from 11 o'clock A. M. to 9 P. M., and next day it was resumed from 6 A. M. to 9 P. M. The tumour became more firm, and the compression was employed again at 6 A. M. of the third day; at 8 the pulsations had become indistinct, and at 12 they had completely disappeared. Thirty-one hours altogether had been occupied in making compression.

2. A child, aged 9, having had the trunk of the temporal artery opened, just anterior to the ear, an aneurism about the size of a nut appeared eight or ten days after the accident. Direct digital compression was employed for five hours on the first day; for ten and a half hours on the second; for eleven hours on the third; for nine and a half hours on the fourth; for eleven hours on the fifth; for ten hours on the sixth; for ten and a half hours on the seventh; and for nine and a half hours on the eighth—making a total of eighty-five hours, at the end of which time the aneurism had become cured.—*Med. Times and Gaz.*, Jan. 19, 1861, from *L'Union Méd.*, No. 1, 1861.

31. *Arrest of Venous Hemorrhage*.—In the course of an interesting paper by Prof. LANGENBECK on the "Surgical Pathology of Veins," illustrated by numerous cases, he observes that styptics are not suitable for the arrest of venous hemorrhage. The best of these, the liquor ferri sesquichlorati, is dangerous, owing to the extensive thrombus formations and subsequent irritating effects it gives rise to. In all cases, when obstinate venous bleeding proceeds from several small veins, he gives decided preference to the actual cautery, as most certainly guarding against the breaking up of thrombi and pyæmia. When the bleeding proceeds from a large vessel, compression, ligature of the vein, or ligature of the corresponding artery, should be resorted to. In wounds of the large veins of the extremities, *compression* of the peripheric end by means of the finger will usually suffice; and in wounds of the jugulars, we should at once apply the finger to the central end to prevent the entrance of air, and then to the peripheric end to arrest the bleeding. In the case of a large wound of the jugular, the finger can only act provisionally, and the best means of proceeding consists in closing the lips of the outward wound by strips of plaster (which must not extend to the uninjured side of the neck, where they would compress the opposite jugular), so applied as to exert the most equable compression around the wound without impeding the circulation. In the case of the veins of the extremities, bandages may also be exactly applied, commencing at the toes or fingers. When the injured vein is at the bottom of a wound, the author places some cerated linen in contact with it, fills the wound with charpie, and then brings its edges together with plaster.

Ligature of the Vein.—In general, tying the peripheric end of a wounded vein of the extremities suffices; but a ligature both above and below the wound may be required when a considerable branch enters just above the central end. To avoid the loss of blood during the removal of large tumours, the provisional ligature of several large subcutaneous veins, which sometimes acquire the size of the finger, may be requisite; and in such cases the author always applies a double ligature, and divides the vein between, removing the ligatures after the

completion of the operation. This practice is the more to be recommended from our ignorance of the conditions under which air gains entrance by dilated subcutaneous veins. In wounds of the external jugular, the central end should always be tied, as it should be prior to operations likely to lead to its being opened. Under other circumstances, the ligature of large veins should be avoided as much as possible, especially in hospitals, where the danger from thrombosis and pyæmia is increased. The internal coat of a large vein is not divided by the ligature, as in the case of an artery; and the inner walls approximated by the ligature may unite before the separation of the latter—the vessel remaining pervious, though somewhat narrower, up to the very seat of the ligature. When inflammation follows a wound or a ligature, more or less extensive thrombosis may succeed, which may lead to detachment of coagula or pyæmia. When this is not the case, the vein gradually becomes pervious again; and so great is the regenerative capacity of veins, that, even when large portions have been removed, these may be reproduced, effecting a junction between the separated ends of the still pervious vessel.

Ligature of the Artery.—It is obvious, *à priori*, that compression or ligature of the corresponding arterial trunk, by preventing the access of blood, must arrest hemorrhage from a large vein; but it does not appear that any one has yet practised the ligature for such a purpose. One reason of this seems to be derived from the fact that when compression of an arterial trunk, as the carotid, subclavian, or femoral, has been made, through the soft parts, at some point between the heart and the bleeding wound, the bleeding from the jugular, axillary, or femoral vein has not immediately ceased. This is partly because the peripheric veins still continue to pour their blood into the injured vein, and partly because an effectual compression of the carotid or femoral artery is not possible without accompanying compression of the jugular or femoral vein. Compression of the arterial trunk, therefore, not having been attended with the same immediate effect as in arterial hemorrhage, it has been believed that no good result was to be expected from the ligature. Again, it has continued, until the most recent times, a cherished opinion among most surgeons, that the simultaneous tying of a large venous and arterial trunk must give rise to gangrene—a fear which experience has shown to be unjustifiable. In fact, when both artery and vein are tied, not only does no gangrene follow, but there is less disturbance of the capillary circulation than when only one of these vessels is submitted to the ligature. In two cases related by the author in which the carotid and common jugular were both tied, no disturbance whatever of the cerebral circulation took place, and neither patient exhibited any of the symptoms which have been met with when ligature of the carotid alone has been practised. During the establishment of the collateral circulation an equilibrium between the arteries and veins has been maintained. In this ligature of the artery, then, we have a safe means of treating venous hemorrhages which may otherwise prove fatal, and the author relates a case in which he made application of it. During the removal of a large sarcomatous tumour from the thigh, the much-enlarged and brittle femoral vein was opened. The hemorrhage was excessive, repeatedly resisting every attempt to arrest it, and the patient was well-nigh lost. The femoral artery, already exposed during the operation, was tied, and the bleeding at once ceased. The ligatures which had been passed around the vein were removed, and the wound dressed. The patient did well. This, as far as the author is aware, is the only case in which an arterial trunk has been intentionally tied for the arrest of a dangerous venous hemorrhage. He refers, however, to cases quoted by Dr. Crisp, in which bleeding from wounds of the vein made during the operation for popliteal aneurism ceased after the application of the ligature to the artery. The author recommends that as soon as compression proves without avail in hemorrhage from large venous trunks that the artery should be at once tied—the simultaneous tying of the injured vein being unnecessary and unavoidable from the danger of thrombosis it gives rise to.—*B. and F. Med.-Chir. Rev.*, Jan., 1861, from *Archiv für Klinische Chirurgie*.

32. *Ovariectomy in London Hospitals.*—The following table, taken from a late number of the *British Medical Journal* (Dec. 1860), embraces, it is said,

all the operations performed during the last three years in the London hospitals, except those in the practice of Mr. Baker Brown at the "London Home."

The operations are numbered in the order of their performance.

No.	Hospital.	Operator.	Date of operation.	Result
1	Samaritan	Mr. S. Wells	Feb. 1858	Recovered
2	Metropolitan Free	Mr. Hutchinson	Aug. "	Do.
3	Ditto	Ditto	Aug. "	Died
4	Ditto	Ditto	Sept. "	Do.
5	Samaritan	Mr. S. Wells	Sept. "	Recovered
6	Metropolitan Free	Mr. B. Childs	Nov. "	Died
7	Samaritan	Mr. S. Wells	Nov. "	Recovered
8	University College	Mr. Erichsen	Nov. "	Died
9	Samaritan	Mr. S. Wells	Jan. 1859	Do.
10	Guy's	Mr. C. Forster	Feb. "	Do.
11	Metropolitan Free	Mr. B. Childs	Feb. "	Do.
12	Samaritan	Mr. S. Wells	May "	Recovered
13	Ditto	Ditto	June "	Died
14	Ditto	Ditto	June "	Recovered
15	Ditto	Ditto	July "	Do.
16	Ditto	Ditto	Oct. "	Do.
17	Westminster	Mr. Holt	Oct. "	Died
18	Samaritan	Mr. S. Wells	Dec. "	Do.
19	Ditto	Ditto	Jan. 1860	Recovered
20	London	Mr. Curling	Feb. "	Died
21	Samaritan	Mr. S. Wells	Feb. "	Do.
22	Middlesex	Mr. Nunn	Oct. "	Do.

The result, it will be perceived, is 13 deaths and 9 recoveries. Two of the latter, however, after recovering from the operation, died some months afterwards of cancer.

33. *Ovariectomy in Canada.*—Dr. REGINALD HENWOOD, of Brantford, C. W., records a case of ovarian tumour in an unmarried woman, 38 years of age, in whom he removed the tumour, through an incision extending from two inches above the umbilicus to within an inch of the pubis. No anæsthesia was used. The tumour proved to be the right ovary, and weighed $7\frac{1}{2}$ pounds, twenty-four hours after removal, when it must have lost a considerable portion of its weight. The wound healed by the first intention. In seven weeks the patient was able to go home.—*British Am. Journ.*, Dec. 1860.

34. *Extirpation of the entire Parotid.*—M. MARZOLO relates (*Gazetta Medica Italiana*) a case of a woman 50 years of age, in whom he extirpated, he asserts, the entire parotid, preserving the facial nerve and the external carotid artery. In six weeks the cure was complete, and eleven years afterwards she remained in perfect health, presenting no indication of a return of the disease.—*Gaz. Méd. de Paris*, Jan. 5, 1861.

35. *Removal of Inferior Maxilla for Osteo-Sarcoma.*—Dr. J. R. DICKSON, Prof. Surg. in Univ. of Queen's Coll., Kingston, C. W., reports (*British Am. Journ.*, Feb. 1861), a case of this in a man 60 years of age, having a rapidly increasing malignant osteo-sarcomatous tumour extending along the entire left half of the inferior maxilla. The operation was performed on the 18th of June, 1855, the patient being partially under the influence of chloroform. The case went on favourably, and on the 22d of July the patient was discharged, apparently cured. On the 13th of the following Dec. the patient returned, with a reappearance of the disease in the cicatrix, and on the 16th of March, 1856, death terminated his sufferings, nearly nine months after the operation.

36. *Injuries requiring Amputation.*—Mr. SPENCE showed to the Edinburgh Medico-Chirurgical Society (Dec. 5, 1860) preparations from three cases in which amputation had been rendered necessary by injury.

The *first* was a very beautiful preparation of the bones of the lower extremity. The history of the case was this: Five weeks ago, a young man, walking on the top of an embankment of no great height, slipped his foot and fell, the leg being slightly bent at the time. He was at once brought to the hospital, when it was ascertained that he had sustained a compound fracture of the thigh, the periosteum being extensively denuded. In addition, there was a simple fracture of the bones at the lower part of the leg, involving the ankle-joint. The fracture of the thigh evidently extended into the knee-joint, and there was great laceration of the soft parts. Amputation was accordingly performed high up in the thigh. The preparation showed that the condyles of the femur had been broken up, that there was an oblique, almost longitudinal, and comminuted fracture of the lower part of the bones of the leg, that the ankle-joint had been opened into, and that a portion of the astragalus had been broken off. This case, Mr. Spence remarked, showed how a very severe injury might be occasioned by a comparatively slight fall.

The *second* preparation illustrated a rather rare form of injury. A child, six years old, while playing in the Grassmarket three weeks ago, was ridden over by a cart, the wheel passing over the arm. He was brought into the hospital in the afternoon, and was seen by the house-surgeon. The limb was swollen and very cold, and it was evident that the humerus was fractured. As the parents would not allow the child to remain in the hospital, the limb was put up very lightly, and directions were given, that if the swelling increased, the bandages should be loosened. On being brought back next day, pain had ceased, the limb was much swelled, and the hand was cold, dark-coloured, and mottled. Mr. Spence was unwilling to operate at once, but retained the child under observation. The limb was laid straight, and surrounded with cotton wadding; no bandage was applied. Two hours afterwards the hand was still cold; the mottled appearance was not increased. In the evening, it was evident that there was no attempt at recovery; the limb was cold and dead. Amputation, accordingly, was performed through the surgical neck of the humerus by means of an external flap. The preparation showed that the artery (which had been injected) had been ruptured opposite to the point of fracture, having apparently been pressed by the cart wheel against the sharp edge of the bone. Anastomosis not having been established, the limb had perished, not by gangrene, properly so called, but by direct death. Mr. Spence remarked that in many cases of fracture it was wonderful how the arteries escaped injury, being generally pressed out of the way of the bone. Here, however, all vascular supply had been fairly cut off.

In the case from which the *third* preparation had been obtained, a severe burn of the arm, forearm, and trunk had been sustained three months ago. After the general health had been somewhat re-established, it became evident that such destruction of the true skin of the arm and forearm had occurred, that there was no chance of a satisfactory recovery, for even if cicatrization took place, such contraction as was unavoidable must render the limb useless. Latterly the health of the patient was evidently giving way, owing to constant discharge, and pain and irritation from the large weak ulcer. Amputation was accordingly performed at the shoulder-joint, the only flap obtainable, from the condition of the soft parts, being from in front of the articulation.

In the three cases described, the patients had gone on favourably.—*Ed. Med. Journ.*, Jan., 1861.

37. *Tracheotomy in Croup.*—Dr. WATSON exhibited to the Medico-Chirurgical Society of Edinburgh (Dec. 5, 1860) the larynx and trachea of a child, æt. ten months, in whom, three weeks ago, he had performed tracheotomy on account of croup. In this case, when seen by Dr. Watson, suffocation was impending, the respiratory acts were attended by drawing in of the chest, and expiration was much prolonged; the surface was blanched and cold, the lips purplish, the face and neck œdematous. The opening of the trachea afforded complete relief

for twenty-four hours. At the end of that time, however, respiration again became obstructed, and on removing the tube (which was a double one, and had been repeatedly cleansed) a loose portion of false membrane was seen flapping up and down with each respiration. On removing this, and clearing the bronchi, by means of a pair of small dressing forceps, of more of the same exudation material, respiration became easy, and continued so for four hours, when symptoms of obstruction again recurred. Although relieved five or six times in this way, the frequent reproduction of the obstructive material, and the increasing weakness of the child, speedily proved fatal, the child dying thirty-eight hours after the operation. The preparation showed patches of croupous deposit both in the trachea and larynx. The bronchi were plugged with a quantity of thick glutinous muco-pus, and the lungs, except in the upper lobes, were completely non-crepitant and collapsed. In opening the chest an unusual phenomenon presented itself, viz., the emphysematous distension of the anterior mediastinum, by which the heart was partially displaced towards the left side. This appeared to have resulted from the air finding a more ready access to the expanding thorax through the wound, and external to the trachea, than through the obstructed bronchi.—*Ed. Med. Journ.*, Jan., 1861.

38. *Suppression of Suppuration, and the absolute Disinfection of Wounds by the Permanent Application to their Surface of a Sponge soaked in a Chlorinated Solution.*—Dr. HERVIEUX extols the efficacy of sponges steeped in a chlorinated solution as an application to suppurating wounds. The following are his conclusions in regard to this method of treatment :—

1. The permanent application of a sponge steeped in a chlorinated solution to the surface of severe suppurating wounds has the effect of transforming them into healthy-looking sores of a vermilion tint, free from exuberant fungous granulations and from suppuration. 2. While this mode of treatment suppresses the suppurative process, it favours cicatrization, which is never more regular and more certain than in the absence of suppuration. 3. This application resolves in the negative the question whether suppuration is the method employed by nature to repair the physical lesion inflicted on the living parts. 4. Among all the disinfectants of suppurating wounds there is none more efficacious than the sponge soaked in a chlorinated solution, because it suppresses the very source of fetor—namely, suppuration and its products. 5. With very few exceptions, this application causes no appreciable irritation on the affected surfaces, or on the surrounding parts. 6. This application is most advantageously employed in the treatment of phagedenic gangrene, of the eschars which succeed to severe fevers, of eczematous, scrofulous, or bony ulcers, of hospital gangrene, of perineal laceration, and generally of all suppurating wounds of an unhealthy character.—*L'Union Méd.*, Oct. 25, 27, and 30, 1860.

39. *Treatment of Blennorrhagia by Injections of Subnitrate of Bismuth.*—The subnitrate of bismuth has been lately recommended as a local application in blennorrhagia, but as the results were not altogether satisfactory, M. MOURLON has investigated the circumstances which prevent the success of this kind of medication. He found that the injections of the subnitrate of bismuth often irritated the urethra; and on testing the salt, he ascertained that it reddened litmus paper. In order to obviate the acidity of the injections, he caused the salt to be washed until it presented no acid reaction; and under these circumstances he has found the injection almost uniformly successful, and has introduced its use into the military hospitals. In a memoir published by M. Murlon, he states that out of 37 cases, 32 were cured, and that the average duration of the treatment was twenty-one days. With the exception of four patients who came into the infirmary for relapses, all the cases presented blennorrhagia of five to eight days' duration, and they were all acute cases. In none of them did the inflammation extend to the deep parts of the urethra, so rapidly was it arrested in its progress by the subnitrate of bismuth.—*B. and F. Med.-Chir. Rev.*, Jan., 1861, from *Bull. Gén. de Thérap.*, Sept. 30, 1860.

OPHTHALMOLOGY.

40. *Operations for the Relief of Glaucoma, &c.*—There is no department of our science, in which more startling developments have been made within the last few years than in Ophthalmic Surgery. The impetus to this seems to have been given by Dr. Graefe, of Vienna, who announced about four years since that he had discovered a remedy for that, fortunately not very common, but hitherto intractable disease, glaucoma. This consisted in making an incision through the cornea as close as possible to the sclerotic, grasping through this opening with a forceps, the iris, drawing it out and cutting off a portion of it. The larger the portion of the iris thus excised, the better, it is said. How the general hyperæmia of the eyeball, existing in acute glaucoma, was to be cured by incising the eye, dragging out the iris and excising it, struck most reflective surgeons as an enigma. No satisfactory explanation was offered, all that was said was that the phenomena of glaucoma resulted from "intra-ocular pressure," and that the removal of this pressure restored the sight. This explanation could not bear examination, but opposition was tried to be silenced by the assertion that in numerous cases the operation had effected marvellous cures.

Some enthusiastic young English surgeons who saw Dr. Graefe operate, soon introduced the operation into England; it was resorted to at the London Royal Ophthalmic Hospital, and great success at first claimed for it. From the number of cases operated on, it would appear as if a convenient epidemic of the disease had sprung up. But a careful examination of the iridectomy operations reported by Dr. Bader, as performed within a little over two years, at the hospital just named, hardly justify the claims set up for it. We take the following analysis of Dr. Bader's report, from a paper by Mr. Hancock (*Lancet*, Oct. 13, 1860).

"Of 107 cases, 16 are admitted to have been injured by the operation. The eyeball in several cases had to be extirpated after the operation, in consequence of hemorrhage and the escape of vitreous humour. The suspensory ligament of the lens was ruptured in others, and the vitreous humour escaped; in one case, from difficulty of seizing the iris, the vitreous humour, with the hyaloid fossa and lens, escaped, followed by half an ounce of blood.

Of 72 cases, bleeding from the iris into the anterior chamber occurred in 12, in one case beginning a quarter of an hour, in another a day, after the operation, the latter being followed by suppuration of the globe; and one case required a counter-puncture, to allow the blood to escape.

In 3 cases, hard portions of transparent lens escaped at the time of operation. In another, the lens and capsule, with about one-third of the vitreous humour, escaped through the wound six hours after the operation. Some portions of the lens escaped in another case, and some vitreous humour also while removing the remainder of the lens with a scoop; whilst in 7 cases the lens became opaque, without any fragments escaping. The opaque lens, however, was subsequently extracted favourably in several of these cases.

Dr. Bader's tables include the result of operations upon 84 eyes. Of these—

2 were not so well after operation.

36 were not benefited.

Of the remaining 46:

At the time of operation—

7 had no perception of light. Of these—

5 recovered perception of light.

2 recovered the power of recognizing fingers, one being also able to distinguish distant objects.

22 had perception of light in degrees varying from faintest, faint, fair, to actual perception. Of these—

3 recovered improved perception of light in portions of the retina only.

5 recovered improved perception of light.

1 recovered the power of recognizing objects by a portion of the retina only.

9 recovered the power of recognizing objects.

4 recovered the power of reading: 2 small, 2 average type.

6 had perception of light by portions of the retina only. Of these—

1 was somewhat better.

2 obtained perception of light by whole retina.

2 recognized objects by portions of retina.

1 recovered the power of reading type of average size.

6 had perception of objects. Of these—

2, who had perception of large objects, were improved.

1, who could see small objects, such as keys, could read average type.

1, who could count fingers, acquired the faculty of reading small type.

2, who had no perception of distant objects, and could only count fingers with difficulty, recognized fingers and distant persons.

4 could read large type. Of these—

1 was improved.

1, accommodating at from 3 to 10 inches, reads average print.

2 recognize distant objects and read average type.

1 patient could see, but not read, large type :

Reads large letters and tells the time on a distant clock.”

MR. HANCOCK, imagining that the pathological and ophthalmoscopic appearances in glaucoma were due mainly to the constriction exercised by the ciliary muscle, reprobrates the injury done to the eye by Graefe's operation, and claims to have attained better results by a simple division of the ciliary muscle. This he accomplishes, he says, as follows: “I introduce a Beer's cataract knife at the outer and lower margin of the cornea, where it joins the sclerotica. The point of the knife is pushed obliquely backwards and downwards until the fibres of the sclerotica are divided obliquely for rather more than one-eighth of an inch; by this incision the ciliary muscle is divided.” It is not easy to understand how that microscopic muscle—the ciliary—can produce any important stricture, or how a slight incision of a few of its fibres, if really effected, could relieve such stricture, while it seems more probable that, in the operation under consideration, a few fibres are in reality only separated. But Mr. Hancock claims for it much greater success than is attained by Graefe's operation, and adduces 16 cases which he considers demonstrative of this.

MR. NUNNLEY, of Leeds, in a very recent paper (*Lancet*, Jan. 26, 1861), expresses his conviction that neither the theory nor the practice, as applicable to the theory of Mr. Hancock, can possibly be true.

“Supposing,” he says, “the ciliary muscle were really hypertrophied and in a state of tonic spasm, how could it induce hardness of the whole globe of the eye, flattening of the cornea, and much intra-ocular pressure? Muscles unstriped as well as striped act only in proportion to their length, in the direction of their fibres, and towards their most fixed attachment. The fibres of the ciliary muscle, firmly attached to the scleroto-corneal junction, radiate outwards and backwards from this point upon the soft yielding choroid. The fibres are not circular, but straight, and not more than one-seventh of an inch long. They can have no action whatever upon the sclerotic coat, to which they are not attached posteriorly; they therefore cannot render it hard. Whatever effect they may have upon the choroid would rather be to pull it away from the sclerotic, and hence, if any effect upon the latter, to render it less tense. Though the ciliary muscle be called by some the tensor of the choroid, it can hardly act much in this manner upon this coat, whatever it may upon the ciliary processes. But even were this tensile action proved, and the muscle contracted to its entire length, the choroid coat is too extensive, too lax, and too yielding to allow of the muscle dragging the whole coat inwards with sufficient force to compress the retina and vitreous humour in any great degree, and it certainly could by no possibility cause any atrophy or cup-shaped depression of the optic-nerve papilla, as all agree does occur in glaucoma; since the cribriform part of the sclerotic coat is *outside* the choroid, and therefore altogether beyond even the theoretical power of the ciliary muscle.

“But even were this theory of compression by the ciliary muscle as true as it appears untenable, how could a simple puncture in the direction of its fibres interfere with the action of the entire circle of its fibres? A broad transverse incision in the direction of the corneal curve, by dividing the fibres, might act powerfully in proportion to the number of fibres divided; but that a simple mo-

mentary separation from each other of two or three adjoining parallel fibres, without any division of their structure (for such must be the effect of a fine, sharp, thin knife), could permanently arrest strong spasmodic action in an entire muscle spread over a large circle, it is impossible to conceive."

With regard to Graefe's operation, Mr. Nunneley says: "While no operation has ever been brought forward with more authority, nor more highly extolled by its introducer, whose deservedly great name has induced many surgeons in this and other countries to adopt it, and who have agreed with him in certifying to its complete efficacy in curing diseases hitherto almost incurable, so, on the other hand, perhaps none has ever been proposed, the *rationale* of which is so obscure, or the *modus operandi* of which is so little known and is so unsatisfactory. That cures, more or less complete, have followed the performance of it, we have the abundant testimony of several most competent and trustworthy men; indeed, like myself, most ophthalmic surgeons have seen improvement follow it. The reasoning, however, by which this result is attempted to be explained appears to me very much less satisfactory than the facts themselves. So much is this the case, that when it was first proclaimed that the removal of a large portion—the more the better¹—of what had hitherto been supposed to be an important, nay, essential, tissue for satisfactory vision, and the injury of which in a much less degree would, in the great majority of cases, render a sound eye useless, would be found to be a perfect cure for an eye already almost hopelessly diseased, appeared so astounding, that, like many others, I waited before doing it until the reports of some of those who had more faith than I had gave the result as so uniformly successful that doubt gave way before recorded facts, and though unconvinced by the reasoning, longer resistance to them appeared like obstinacy."

All, however, "that iridectomy accomplishes, in the cure of acute glaucoma and glaucomatous diseases is," according to Mr. Nunneley, "in the greater degree and more permanent manner in which it affords relief to intra-ocular pressure than paracentesis," as previously performed, and he thinks that he has devised simpler means for obtaining this desired result.

"Observing," he says (*Lancet*, Jan. 19, 1861), "that the eyeball is often distended to the utmost limit which the comparatively unyielding sclerotic and cornea will allow, and that the pain and acutely distressing symptoms in the ball and about the orbit commonly occur in proportion to the rapidity with which the distension takes place, whether the disease be glaucoma, iritis, or choroiditis, and knowing that the most unyielding portion of the globe is the point of junction of the sclerotic, cornea, iris, and ciliary muscle, which may not unfrequently, in very decided cases of hydro-ophthalmia, be observed as a depressed ring between the bulging sclerotic and cornea, it occurred to me that division of this part would afford the desired relief, and that not improbably the good gained in Von Graefe's operation in reality depends upon the removal of the resistance of this part, and not upon the ablation of the iris. The result, I think, has been proved. I have waited before announcing it until I have had a sufficient number of cases as fairly to admit of a comparative estimate of it with iridectomy. I have now operated upon about fifteen eyes by the method of Von Graefe, and on certainly not less than twenty eyes in the manner I propose to describe. The result has been in favour of the latter operation. It has, so far as I can judge, afforded all the relief that the more serious proceeding has done, and appears to be free from its inconveniences. It is easier performed, produces much less deformity, inflicts much less mischief upon the eye, is followed by as much diminution of the intra-ocular tension, by as great or even greater increase in the corneal curvature, and the relief has been quite as permanent."

Mr. Nunneley describes his operation as follows:—

"The manner in which I have operated is to puncture the sclerotic coat with the point of a sharp, thin knife—a small cataract or very narrow, short bistoury answers very well—not less than one-eighth of an inch behind its junction with the cornea, and carry it on to about the same extent through the cornea, making

¹ "The excised piece must be as large as possible." Von Graefe, vol. v. p. 354, New Syd. Soc. Pub.

altogether an incision about one-third of an inch long. Care must be taken to pass the knife sufficiently deep to completely divide these textures, and yet not so deep as to touch the lens, which I once did, owing to the patient starting at the moment the incision was made. Care also must be taken not to make the incision too long. A larger incision in the sclerotic, besides unnecessarily wounding important tissues, is useless, and if carried too far towards the centre of the cornea, though allowing this afterwards to yield more, is bad, for it may allow the lens to be displaced into the aqueous chamber; and if the iris should adhere to the whole extent of the corneal section, as it is likely to do, particularly if a portion of its whole breadth has been removed, not only will there be dragging of it, but the section becomes opaque, and hence the field of vision is lessened. In making the section, if the point of the knife has been well kept in, the outer margin of the iris will be divided. Sometimes the iris bulges through the section. I have tried the effect of simply leaving the prolapsed iris in the wound, of cutting it off, and also of pulling out a larger portion, and cutting off a strip through the entire width. In this latter plan the operation more nearly assimilates with Von Graefe's iridectomy, only that the section through the unyielding tissues is made directly across their junction, instead of into or parallel with it, whereby a greater expansion in it is allowed, and not nearly so much of the iris is removed. If none of the iris be cut off or tied, the pupil usually recovers its circular form; if some be excised, it remains oval and attached to the corneal cicatrix in proportion to the size of the piece removed, but in a much less degree than *à priori* be anticipated. The degree of deformity is very slight indeed.

"The spot where the incision is made, so far as the relief obtained is concerned, can make very little difference; but I have usually selected the centre of the lower corneal curvature as being likely to produce less noticeable alteration in the appearance of the eye; and, when both eyes are operated upon, as interfering less with accuracy of vision than any other. On the whole I think, in severe cases, the removal of some small section of the entire width of the iris advantageous, as affording a longer continuous drain of the aqueous humour than simple incision of the scleroto-corneal junction does. I have in one case only seen much bleeding into the aqueous chamber, and what blood is there effused is soon absorbed. In one case the iris gave out more blood than usual—perhaps twenty drops; but as this at once escaped by the section, it was of no moment, and was easily arrested by cold, wet cloths. In two of the cases in which I performed iridectomy, the quantity of blood effused into the eye was sufficient to fill the aqueous chamber; and in one of them it was a long time before being entirely absorbed. Usually, however, all trace of it is lost within a week, or, at most, ten days. In two or three of the cases severe pain in the side of the head ensued, which an opiate relieved; and in one instance, as I have before said, considerable inflammation; but in all other cases very little inconvenience followed."

There are other surgeons who have no pet operations of their own devising for the cure of glaucoma, who distrust all these operative procedures, and we must believe, until additional and more satisfactory experience is obtained, that it is safest to hold the mode of treatment now under consideration, if not of questionable utility, at least as requiring further evidence to sanction its adoption.

It will be seen from the succeeding article what very high authorities still entirely repudiate it.

The benefit claimed by Mr. Hancock to have resulted in glaucoma from division of the ciliary muscle, stimulated Mr. James Vose Solomon, Surgeon to the Birmingham and Midland Eye Institution, to enter upon "an experimental inquiry into the value of incision of the ciliary muscle in the treatment of certain diseases of the eye and disorders of its accommodation," and in a series of papers published in the *Medical Times and Gazette* (Nos. for January 19 and 26, February 9, and March 2), he gives us the results of his investigations. The operation which he has practised and which he terms "Intra-ocular Myotomy," is performed as follows: A Beers cataract knife is entered "at the corneo-sclerotic union and pushed through the pillars of the iris into the muscle the flat surfaces of the blade being opposed on one side to the sclerotic, and on

the other to the rim of the lens." The incision in the muscle is limited to two lines or two lines and a half. In some few cases he thinks the puncture may have been even less than one-sixth of an inch.

By this operation Mr. S. asserts that "the adjustment of the near-sighted eye for near and distant objects is increased." And he relates several cases of near-sightedness said to have been completely cured by this operation. But what is more surprising, Mr. S. claims to have cured "by such an operation" "asthenopia—a form of 'weak sight,' where the patient is unable to maintain the eyes in occupation upon near and small objects, except for small periods, and regains power by resting them with looking into distance." Now it must be remembered that according to the prevalent theory of asthenopia, which Mr. S. evidently adopts, the ciliary muscle has become incapable of a continued effort to draw the lens forward, in this affection, so as to adjust it for near sight, and after a short effort becomes relaxed, and allows the lens to fall back and thus the patient becomes far-sighted. The division of this muscle consequently weakens its power in near-sightedness and strengthens it in far-sightedness!

Furthermore, Mr. S. states that his observations led him "at an early period" "to a conviction that the *nutrition of the tissues of the eye was improved by the performance of intra-ocular myotomy.*"

Again he says: "In some cases of symmetrical myopia, the performance of intra-ocular myotomy upon the 'worst eye' has effected a corresponding improvement in the accommodation of the other, only that the reading distance was an inch shorter, and features were not seen by one or two yards so far as by the operated eye; congestion of the choroid has also been removed." He asks, "Does not incision of the ciliary muscle give promise of enabling surgeons to regulate, without a recourse to spectacles, the focal adjustment of eyes in which the accommodation is not the same?"

"The accommodation to objects between the nearest and furthest points of distinct vision is not in any degree interrupted, and for distinguishing human features, is made equal to that of a person whose adjustment is normal, namely, twenty-five or thirty yards."

He states, "moreover, conical cornea, which is a disease that is not permanently improved by paracentesis, derives considerable benefit from incision of the ciliary muscle. The sides of the cone become more flat, and the vision of the patient has been permanently improved in all cases (eight eyes) in which I have operated, and opacities when present have also been rapidly absorbed. These results appear to point to an improved nutrition in the cornea, and to a diminution of the secretion of aqueous humour, which it will be remembered is furnished by the iris and tips of the ciliary processes—parts which are immediately implicated in the operation.¹

"Intra-ocular myotomy exerts a marked influence upon the circulation of the choroid, quite irrespective of any loss of aqueous or vitreous humour which may attend its performance; also in diseases where the globe is of normal tension, and devoid of those signs which are attributed, erroneously, as I think, to spasm or constriction of the ciliary muscle, such as conicity of the cornea, 'a circular depression at the point corresponding to the ciliary muscle,' and bulging of the sclerotica (staphyloma).

"If I can succeed by the relation of cases in establishing this proposition, it will follow that the operation must influence the condition of the vitreous and lens which depend upon the choroid for their nutrition, and which suffer in so marked a manner in glaucoma and choroiditis."

It would seem almost incredible that this little, delicate, semi-transparent muscle—the ciliary—only one-eighth of an inch broad, should be productive of so much and such varied mischief, had we not Mr. Solomon's experience to demonstrate the vast benefits resulting from a division of it. Nevertheless we must confess that we are inclined to wait until it shall be proved by further observations, whether it is the surgeon or the patient who derives most benefit from the operation.

¹ "The vision in the cases of conical cornea, which I have treated, has been better where the muscle was divided enocularly than by the other method; but more cases must be observed before this point can be considered as settled."

41. *Iridectomy and Glaucoma.*—Dr. W. R. WILDE, of Dublin, is, and with good reason we are persuaded, no believer in glaucoma and iridectomy, and it appears from the following communication that more than one English ophthalmic surgeon of note is of the same way of thinking. Dr. Wilde writes:—

“The senior member of the oculist art in England, the venerated and now venerable William Lawrence, whose writings on the subject are known all over the world, is opposed to the operation, and has thus expressed himself to the author of the review¹ in a letter dated 12th of August, which is now before me: ‘This able and well-timed exposure of the iridectomy delusion is of great service to the public, not only on account of the mischievous character of the proceeding, but from the quarter of its introduction into England, which has given it undeserved respectability and patronage.’ Mr. Dixon’s opinion, in which he denounced the operation, is already before the public, and has been quoted in the review in question. In addition, I may insert the following letter just received from him:—

“‘You ask me what is my present opinion of iridectomy? To answer this question I must go a little into details. If the operation is to be judged of simply by the general result of the cases in which it has been performed, I should say that no mode of surgical treatment, proposed within my remembrance, has been more frequently done without benefit, or *even with positive damage to sight*. The definition of glaucoma, especially of chronic glaucoma, is so vague, that those who have had but slight practical acquaintance with eye-diseases, and know glaucoma chiefly from written descriptions, are constantly liable to mistakes in diagnosis; while the comparative ease with which iridectomy may be performed encourages many to undertake it who would be deterred from attempting an extraction or an artificial pupil by the acknowledged difficulty of such delicate operations.

“‘Cases have come under my observation in which iridectomy had been performed for so-called chronic glaucoma, but where the real disease was partial cataract, detached retina, or simple atrophy of the optic nerve. Nay, I have known instances of the reflexion from old corneal opacities, or even that from an opaque lens, dislocated into the vitreous chamber of a blind eye, to be mistaken for the peculiar tint of glaucoma.

“‘It would be matter for deep regret if merely negative results had followed these needless operations; but it is truly lamentable to reflect that iridectomy, hastily undertaken without a previous careful diagnosis, has often transformed a partial into a complete opacity of the lens, in patients ill suited to undergo subsequent extraction, at the same time, perhaps, giving rise to serious internal hemorrhage.

“‘Of course, it is not fair to hold the originator of a new invention responsible for all the excesses of his followers and imitators; and, therefore, it would be unfair to test Graefe’s operation by the strange applications of it which rapidly followed the announcement of his discovery. One cannot but feel, however, how much mischief was done by the broad and unqualified manner in which iridectomy was first put forward as a “cure for glaucoma,” without a due explanation being given of the limits of its applicability.

“‘These limits appear to me to be narrowed to those cases of acute inflammation, characterized by the following symptoms: a sudden attack of pain, at first dull, then rapidly becoming acute, and assuming the character of neuralgia throughout the ophthalmic division of the fifth nerve; loss of vision within a few hours or days, the second eye being often attacked very soon after the first. Along with these subjective symptoms, the following, of an objective kind: great injection of the sclerotic, an irregularly dilated and fixed pupil, with peculiar slaty discoloration of the iris; an uneven and hazy condition of the corneal epithelium, and an unnatural hardness of the eyeball.

“‘You well know how hopeless these cases have always been, coming on, as they so often do, in enfeebled and unhealthy persons, and extinguishing sight before our remedies have time to act. Now, in these cases, I have seen marked

¹ Glaucoma and Iridectomy Epidemic, in the ‘Dublin Quarterly Journal of Medical Science,’ August, 1860.

relief attend the evacuation of the aqueous humour through a large corneal opening, and so great has been the benefit of this proceeding that I have often found it unnecessary to interfere with the iris; but, in other cases, the acute symptoms have returned, and I have then removed a portion of the iris with permanent benefit. I have made my incision in the cornea quite close to the sclerotic, but not absolutely in the latter tissue, as recommended by Graefe himself. Neither have I removed such a large portion of iris as he does, for it seems to me that to establish a free communication between the anterior and posterior chambers of the aqueous humour is the aim of iridectomy, and that, if this is effected, all further removal of iris can only do harm by the after-deformity of too large a pupil.

"But there seems to be a disposition just now to treat the iris with very little ceremony, for the latest German discovery consists in removing a cataract by making a *small* corneal section, and pulling out the nucleus of even a firm lens with a large spoon-shaped scoop, the iris being freely cut away as a preliminary step, by way of preventing prolapsus iridis, so troublesome a complication of the ordinary operation of extraction. Certainly a very effectual means of preventing prolapsus iridis; but what is to be said of the prolapsus *humoris vitrei*, which the spoon is so likely to cause when thrust behind the lens?

"If iridectomy had been announced as an operation for relieving certain forms of acute inflammation of the eyeball, it would have conferred a real benefit on ophthalmic surgery; but brought forward, as it has been, as a "cure for glaucoma," extravagantly praised as forming a new era in surgery, and illustrated by such a multitude of incredible cases, it has proved a fruitful source of delusion and disappointment."

"Mr. France, ophthalmic surgeon to Guy's Hospital, and already known by his writings as a sound practical oculist, writes to me as follows: 'I have been strongly tempted to write a note of acknowledgment to the author for the good service to the cause of truth, humanity, and rational surgery, which it is calculated to effect. I, for one, cordially approve the fearless exposure this paper contains of as transparent a system of loose unconsequent reasoning and disregard of results as has ever called down the reviewer's castigation. It is well that so spirited a protest should be made in the name of legitimate medicine, against this prevailing epidemic, which is, however, but one manifestation of a *cacoethes operandi* now reigning. Mr. Bowman has, it is stated by medical reporters, cured many cases of glaucoma by iridectomy; but he himself has never put forward to the profession a single specific instance thereof under, as stated by the reviewer, "his hand and seal." How many of these *cures* may be among the eighty-four eyes operated on in the fifty-five cases in Moorfields Hospital, the sad but not unexpected results of which have been so precisely set forth by Dr. Bader, is more than I can tell.' Mr. White Cooper has thus written to me: 'Apart from its being yet on its probation, I think that the operation of iridectomy is too lightly regarded; the impression appears to exist, among a large number at least, that it is a mere trick, and that no harm can possibly result from it; that serious consequences *must* and *do* result I know, and I certainly attach more importance to the proceedings than appears to be the case with many.' Mr. Haynes Walton, author of our best work on ophthalmic operative surgery, says, in a communication I have just received from him: 'The author of that article has done the world a favour, and I thank him.' Why, I would ask, should these gentlemen, together with Jüngken of Berlin, Minten of Stockholm, Sechel of Paris, Dr. Jacob, myself, and many others, protest against an operation which would *restore sight*, relieve suffering, advance science, and bring money into their pockets—if such a mode of cure were really true? The only answer I know of is that which the ignorant public bring daily against the 'prejudices of the profession' in the matter of Turkish baths, 'brandy and salt,' or such other ephemeral delusions.

"One word more before I have done. It was well known that Sir Benjamin Brodie's sight had been failing for some time past; the cause was believed to be cataract, a diagnosis to that effect having been made by a sound practical surgeon. When I was in London some weeks ago, it was rumoured that he had

been operated on for glaucoma! It would be unjustifiable in me to enter into the details of the case as they were rumoured about, but I may remark that the profound silence which was observed by the friends of the baronet, and others, as well as by the officers of the Royal Society, gave an air of mystery to the proceeding which was unaccountable, except upon the supposition that in due process of time this great triumph of iridectomy in glaucoma, submitted to by the acknowledged head of the profession in England, and the president of one of the first societies in Europe, would be trumpeted abroad, and forever crush the opponents of the new German operation. Pending the result, I refrained even from speaking upon the subject—although Lord Brougham had at the recent statistical congress, when apologizing to a large assemblage of medical men, and the public, for his not being present, ‘regretted the *cause* of Sir Benjamin Brodie’s absence.’ Now, however, the matter is no longer in private keeping; it is public property. A *Taunton* paper having published an account of Sir B. Brodie’s defective sight, &c., that statement went the round of the newspapers, when the following cautiously worded article appeared in the *Times*, and was copied into many papers: ‘We are authorized to state that he has lately undergone an operation for the improvement of his sight, and that a satisfactory result is anticipated.’ So say all of us; but in the last number of the *Medical Times and Gazette* we have a short leader on the subject of that announcement, to the following effect: ‘Iridectomy was performed under chloroform. We deeply regret to say that the result is not so satisfactory as the leader in the *Times* would lead the profession to hope. The left eye may be believed to be much in the same state as before the operation, if anything slightly improved; but in the right or better eye, vision is quite lost. The ground of hope in this case is, that as there is *now* a cataract very evident in the right eye, that this is the cause of the impaired vision; that the eye is *not glaucomatous*, and that, hereafter, vision may be restored by extracting the cataract.’ It is, therefore, manifest—if this be true—that in this case there must have been something very peculiar, either in the diagnosis or in the practice. The announcement, thus made public by a London periodical, will, I am sure, be received with extreme regret by the medical profession in Ireland, by whom Sir Benjamin Brodie’s merits, in every walk of life, were duly appreciated. And should the case unfortunately turn out as conjectured, it will prove a severe blow and a heavy discouragement to the promoters of ‘The Glaucoma and Iridectomy Epidemic.’”—*Dublin Hospital Gazette*, September 1, 1860.

42. *New Methods of Curing Cataract*.—MR. TAVIGNOT, in a communication to the Academy of Sciences, proposes to cure cataract by puncturing the cornea at two opposite points of its circumference by needles, one of which is attached to the positive and the other to the negative pole of a galvanic battery. The two needles are approximated on the surface of the anterior capsule, and brought to a white heat by means of the battery. The capsule is thus destroyed, and by a little manipulation, says Mr. T., the whole of the opaque lens can be charred and broken up.

This *discreet* proposition is rivalled by that of Dr. M. LANGENBECK, of Hanover, who has recently recommended what he calls *isolation* for the cure of cataract. This consists in the application of the concentrated rays of the sun to the lens, which is done by directing the focus of a burning-glass into the eye for several minutes, so that it falls exactly on the opaque lens. He repeats this operation three times within a quarter of an hour. He boasts of having cured nine cases by this plan.

We must not omit to add, that Prof. Weber tried this method on rabbits, in which he had previously artificially produced opacity of the lens, and, as might have been anticipated, in no one case with benefit, while several of the animals died from inflammation of the brain.

43. *Artificial Cornea*.—It is stated (*Abeille Médicale*) that Dr. HEUSSOR, of Richterschweil, has successfully inserted artificial corneas of glass in a young girl, blind from opacity of the cornea. The eyes, it is stated by the narrator,

bear this foreign substance very well and will probably do so hereafter. (*Mon. des Sc. Méd.*, 23 Feb. 1861.)

This operation was some years since proposed by Dr. Nussbaum, of Munich (see No. of this Journal for July, 1854, p. 95 *et seq.*), but it was not supposed that it would succeed in the human being, and we must still entertain doubts on the subject.

44. *Proptosis, with Goitre, Palpitation, &c.*—Dr. C. Handfield Jones read an account of a case of this before the Royal Med. and Chirurg. Soc. (Nov. 27, 1860). Dr. C. J. B. WILLIAMS said that he knew of no class of cases more curious, or more peculiar in their prominent symptoms, than that referred to in the paper. There was none in which the effects of treatment were more striking. At first, these cases seemed to present all the signs of excessive arterial action, requiring depletion, sedatives, or other lowering treatment; but we now know that they were only to be combated by powerful tonics and general support to the system. Their pathology had at one time puzzled him (Dr. Williams) much. Many cases had been presented to him under the garb of heart disease or consumption. The physiognomical character of the disease was too remarkable to be mistaken. The prominence of the eye was striking: the eyeballs projected beyond the brows, sometimes to such an extent that the eyelids did not close during sleep. The vessels on the forehead, face, head, and neck were more prominent, throbbing, and larger than natural. This feature led him to regard the pathological character of the disease as consisting mainly in a kind of aneurismal varix of the thyroid. Enlargement of vessels would account for the prominence of the eyes, which were also, probably, in some cases rendered more prominent by effusion into the orbit. The whole cerebral circulation was in the same state of enlargement, producing excitement and other symptoms of disturbance of the brain. This state depending on diminution of nervous power, and not on increased power, explained the use of tonics in the disease. The action of tonics was remarkable. It might be necessary in some cases in the first instance to allay pain and excitement by the use of sedatives; but it was surprising how well the patient bore tonics when so much excitement prevailed. He had found the best tonics to be those of a more astringent kind, such as the perchloride and the phosphate of iron, which acted almost as specifics in the disease. Formerly he had been in the habit of using the nitrate of silver, and other of the milder tonics; but little benefit ensued until the more decided course of action was persevered in. This, with the occasional use of narcotics at night, highly nutritious food, pure air, and a careful husbanding of the strength, soon effected a beneficial change. He related the case of a lady to whom he was called in the early stage of this affection (he might here remark that all the cases which he had seen occurred in females). This patient was highly nervous, and suffered from great excitement and pain of the head; the eyeballs seemed starting from their sockets. Temporary relief was obtained by the exertion of pressure on the eye, and binding a handkerchief tightly round the head. There was enlargement of the arteries of the forehead, face, and neck. Was this a case for tonics? The answer was in the affirmative. Sulphate of iron with an excess of sulphuric acid was ordered. Under this, with a local application of Fleming's tincture of aconite, the patient perfectly recovered in a few days. He inquired if any member had examined, post-mortem, any of these cases; and suggested that the arteries of the brain and the orbit should, when possible, be carefully scanned.

MIDWIFERY.

45. *The Value of Anæsthetic Aid in Midwifery.*—Dr. KIDD read before the Obstetrical Society of London, December 5, 1860, a paper on this subject. In obstetric practice, the instances where the author has found the inhalation of

ether or chloroform to be called for in an especial degree, and where anæsthetic aid has proved decidedly useful, have been cases of version, forceps, twins, convulsions, and crotchet operations. He has known chloroform used in puerperal mania, but its apparent effect is perhaps a coincidence, and not of a curative nature. The author referred the Society to his previous work on *Anæsthetics*, where the result of 360 cases of midwifery treated under ether, and 1,700 under chloroform, without accident from fatty heart, are described. Of these two agents (though there have been twenty-five deaths from ether in general surgery), he believes ether is superior to chloroform in relaxing rigid perineum in labour, and otherwise acting on the muscles of the uterus, in version cases particularly. There have been no accidents from chloroform in about 30,000 cases of midwifery conducted with the aid of these agents. The mode of applying chloroform in the lying-in chamber recommended was that which is adopted now by all the chief obstetric practitioners in Europe and America with whom the author has personally communicated on the subject. In midwifery practice, the error of using "mixtures" of ether and chloroform was explained, as a patient supposed to be inhaling a mixture is in reality inhaling pure ether, and there is a danger of confusion arising in mistaking one anæsthetic for the other. A new anæsthetic of chloroform and ergot mixed was also mentioned. Cases of twins, where the second child presents with an upper extremity, "the pains severe and continuous, so that it is next to impossible for the accoucheur to introduce his hand to turn the child," were first described, where chloroform is invaluable, if there be no contraindication of diseased heart, etc. The several indications in undilated os uteri for tartar emetic, liquor opii, or chloroform, from the result in actual practice, were explained. Next those cases of twins were pointed out where at one particular stage it is judicious to allow an interval of rest to the uterus; here ammonia and ergot act like a charm; not that chloroform is injurious, but it requires to be given before or after ergot, and not at the same time. The author said that if in journals half the attention were given in fact to these points that is given to quack compounds of chloroform, all would be right. As in some cases of twins, so is it in some cases of "tedious labour;" the patient requires an interval of rest to renew reflex action, and remove the effect of exhaustion, "false pains," emotions of a depressing kind, sleeplessness, etc. Thus, an inexperienced or constitutionally delicate young woman, with her first or second child, becomes alarmed, sleepless, etc., during her labour. Some indigestion, diarrhœa, or fright has hastened her labour before its time. The first stage of labour has been attended with suffering, followed by fatigue. Here the pains are ineffectual in advancing the labour; but if there be no diseased heart or other contraindication to anæsthetics, the author strongly advocates their administration in the manner pointed out in the memoir. How does sleep during labour differ from anæsthesia? Sleep, according to the author, occurs only where there is exhaustion of sensorial or muscular power; anæsthesia is best where there is no exhaustion; it is independent of sleep. The reflex power of Marshall Hall is the frontier or limiting line between sleep and anæsthesia; in sleep it is active, in anæsthesia absent. In tedious labour the agony and pain will not permit the poor woman to sleep. Even opium is followed but by a tedious intoxication, without sleep, for hours; but chloroform is not an intoxicant, and acts at once and quite as safely. A patient in ordinary anæsthesia may be said to be doubly asleep. This is what is wanted for a short time in this class of labours, as thus reflex, sensorial, and muscular power are renewed. Emotion also is removed out of the way by chloroform sleep; and by a confident, cheerful demeanor on the part of the accoucheur, he may effect as much in two or three hours by chloroform as he might in almost as many days by delay, and opium, and waiting for nature. Cases of versional delivery, with and without chloroform, were next minutely described and compared; cases especially of excessive sensibility of the os uteri and vagina, where the waters have long come away, and the uterus has closed with spasmodic force over the fœtal hand and arm. One well-known obstetrician has had 300 such cases; and he is every year more and more satisfied with the aid afforded by chloroform. The value of versional delivery and its *rationale* were also enlarged upon, as well as the much to be desiderated probability of the abolishing of craniotomy, and of many cases where

cated in cases where there was deficient action of the uterus—as in feeble and tardy labour from inertia, and in cases where hemorrhage was expected. He had seen it stop the course of labour midway, and he believed that post-partum hemorrhage and retention of the placenta occurred more frequently after its use than without it. One good effect of the discussion on the present paper lay in the difference of opinion which had been elicited. The causes of these differences would be studied, and the truth brought out. It could not possibly be correct that chloroform relaxed the uterus so as to facilitate turning, and made it contract so as to increase the difficulties of this operation; or that it could both cause and prevent hemorrhage. He had himself no doubt of its usefulness in difficult cases of turning. He had met with cases in which version had been accomplished by its aid, where without it the operation would have been utterly impossible. He had seen mania follow its employment, and he thought that in some cases the relation was that of cause and effect. He had also met with bad cases of rupture of the perineum under its use. The patients were relieved from pain, but volition was not suspended, and under these circumstances the violent and fearless straining efforts ploughed up the perineum by the foetal head in the expulsive pains. It was of very great consequence to lessen, as far as possible, the dangers attending the use of this great and beneficent agent. The influence of fatty heart, alcoholism, and other conditions, in fatal cases, had been much debated; but there was one source of danger which, so far as he was aware, had not been dwelt upon. He referred to idiosyncrasy. He had known patients affected to a poisonous extent by ordinary doses of ether or chloroform. He knew two ladies, in apparently good health, in whom a few drops of chloroform would at any time produce repeated fainting. He suspected, therefore, that some of the inexplicable cases of death from chloroform depended on idiosyncrasy, and, before its administration, it would be useful if patients were tested as to their tolerance of its effects.—*Med. Times and Gaz.*, Dec. 22, 1860.

46. *The Placenta and Membranes in Twin Pregnancies.*—Dr. SPAETH, of Vienna, has observed the condition of the foetal membranes in 126 cases of twin pregnancy. The following was the result—

In 49 cases, each had a distinct placenta, chorion, and amnios.

In 46 cases, the placenta were united; but each foetus had a distinct chorion and amnios.

In 28 cases, the placenta were united, and there was a single chorion; but each foetus had a separate amnios.

In 2 cases, the placenta were united, and there was a single chorion and amnios.

Where the placenta were united, the line of junction was often indicated by a distinct depression on the concave surface of the organ and by scanty deposits of fibrin. The line of demarcation was present even where the chorion was single, and where vascular communications between the two placenta were distinctly seen on their foetal surface.

In none of the cases where the chorion was single was there the slightest indication of its having been formed by the fusion together of two originally distinct membranes. At the level of the line which separated one amnios from the other, the chorion was always perfectly smooth, without thickening or depression. In cases where the chorions were distinct but the placenta united, the vessels were always independent and presented no anastomoses. On the other hand, vascular communications between the placenta existed in two cases where the amnios was single, and in one where there was a doubt as to its being single or double: they were also present in seventeen of the twenty-eight cases where a single chorion enveloped two distinct amniotic membranes.

The anastomoses were always formed by tolerably large branches situated superficially on the inner face of the placenta. Hence, in the majority of cases where the placenta are united, and where the chorion is single, there is free communication between the vascular systems of the two placenta. The anastomoses occur sometimes from vein to vein, and sometimes from vein to artery. In one case, M. Spaeth noticed an anastomosis between an artery of one foetus and a vein of another.—*Brit. Med. Journ.*, Sept. 8, from *Zeitschrift der Gesellschaft der Aerzte zu Wien*.

47. *Fatal Twisting of the Funis*.—Dr. BILLI, obstetrical professor at the Milan Lying-in Hospital, gives in this paper the particulars of a remarkable case of twisting of the funis, in addition to two others which he has already communicated to former numbers of the same journal. By the first of these latter, he believes that he established the fact that knots of the funis may so closely constrict this organ as to cause the interruption of the circulation through it and the death of the child. The second paper related not to a case of knotting, but of twisting of the cord, likewise inducing the death of the fœtus. About the middle of the eighth month of pregnancy, the fœtal movements became excessively violent, and so continued for about eight days, when they suddenly ceased. Two days after this, the breasts became tumid, and there was febrile action. Labour-pains came on a month and a half after the fœtal movements had ceased, and the woman was delivered of a fœtus in an advanced state of maceration, the funis being twisted upon itself in an extraordinary manner. The portion of the funis within half an inch of its insertion was, in consequence of the twisting, reduced to six lines in circumference, and resembled a tendon in colour and consistence. Water, when forcibly injected, would not penetrate this narrow portion of the funis. The author, as well as other obstetricians, has met with cases of twisting of the funis upon itself without any injury to the fœtus; but he believes that the great amount of it here noticed, so great that it was not possible to completely untwist it, can only have resulted from rotatory movements performed in one direction, which in this case would be favoured by the large size of the uterus. Endeavouring to explain the cause of this rotatory movement, the author states that the left lobe of the cerebellum was found to be a fifth smaller than the right, and suggests upon physiological grounds that this may furnish an explanation of the production of rotatory motions in one direction which gave rise to the twisting in question.

A third case which has come under the author's notice now enables him to pursue the subject. Ceasing to menstruate in June, the woman, a primipara, first felt the fœtal movements towards the end of October; and, by the middle of December, they had become so violent that she had to seek medical assistance on account of them. By the practitioner consulted, these movements were compared to those of a wheel violently turned within the abdomen. After continuing with unusual violence for a week, they suddenly ceased, and were not felt again, nor did the abdomen continue to increase in size; the breasts, however, became painful and distended for some days. A fœtus in an advanced state of maceration was born at the end of February. The placenta, which was slightly atrophied, weighed 7 oz. troy, and measured 16 inches in circumference. The funis, twisted upon itself, was 12 inches 6 lines in length. For 11 inches and 8 lines from its placental insertion, it was large, measuring 2 inches 3 lines in circumference, but for the remaining 10 lines, to its umbilical insertion, it did not exceed *two lines* in circumference, resembled tendon in consistence, and was impervious to injection of water. On examination of the cranium, the proportion of the cerebellum to the cerebrum was found to be a fifth less than it should be, the former being also more softened, and especially so on the right side.

Professor Billi then, reasoning from the deductions derived from experimental physiology in respect to unilateral lesions of the cerebellum, concludes that in these two cases the circular fœtal movements resulted from the irregular distribution of the nervous power of the cerebellum over the muscular sense. In the one case, this was due to a changed condition of one of its lobes; and in the other, to a preponderance of such change in one of these. The vast difference in size between the twisted portion and the remainder of the funis was due to the obstruction in the circulation in the former, and the consequent distended condition of the latter. The author reproduced the same condition experimentally, by imparting circular movements to a fœtus recently dead, while water was at the same time injected into the umbilical vein and descending aorta. The funis was 20 inches long and 12 lines in circumference. After the twisting, it became reduced, for an inch of its length from the fœtal insertion, to 9 lines in circumference, and was also then rendered impermeable. In the other 19 lines of its length, the circumference of the funis was increased to 20 lines.—*Med. Times and Gazette*, Jan. 16, 1861, from *Omodei's Annali*, vol. 173.

48. *Treatment of Nausea and Vomiting in Uterine Inflammation and in Diseases of Menstruation.*—Dr. E. J. TILT read a paper on this subject before the Obstetrical Society of London, Jan. 2, 1861.

Nausea and vomiting were said to be comparatively uncommon symptoms in uterine affections, but very distressing from the loss of strength and from the irritability and despondency which followed them. The fact of nausea and vomiting occurring so frequently in connection with otherwise healthy menstruation and with pregnancy was considered to explain why vomiting was a symptom of diseased menstruation; and their occurrence during amenorrhœa, dysmenorrhœa, and menorrhagia, in which the body of the womb, and more particularly its lining membrane, is implicated, was given to explain why nausea and vomiting are frequent symptoms of internal metritis, whether chronic or acute; whereas it was said to be extremely rare to meet with them when the neck of the womb was alone implicated, for they neither accompanied its various kinds of ulceration nor the catarrhal inflammation of its mucous membrane, which is the most common of uterine affections. Continued nausea was represented by Dr. Tilt as much more frequent than vomiting, most troublesome in the morning, going off after breakfast or dinner, increased by worry, excitement, the fatigue of dressing or talking, and being sometimes so irksome as to cause patients to refuse taking any food unless forced to do so. Some patients only vomited once or twice in the morning, others more frequently. One only vomited at menstrual periods, and then incessantly for two or three days, with but short intervals of rest. Another thought that she vomited all her food for a year; and in one case the vomiting was continued for eight years, killing the patient at last by inanition. In most of these distressing cases there were no symptoms of biliousness, the sickness being a reflex nervous phenomenon, as in pregnancy. Dr. Tilt stated that uterine treatment, such as leeches to the womb, or the application of potassa fusa cum calce, would sometimes suddenly check the vomiting for a period; that this result cannot be depended upon; and that besides the regular treatment of the uterine affection, it was necessary to adopt some other treatment to mitigate the patient's sufferings. Even when the patient presented little signs of biliousness, Dr Tilt advised, as a preliminary measure, a full dose of calomel, followed by alterative doses of blue-pill, to be continued for a week or ten days. This would sometimes very much diminish the vomiting and nausea; if not, the well-known minor remedies for sickness might be tried in succession. Strychnine was also mentioned as having been useful with some patients; and various interesting cases were related, showing the utility of a solution of morphine, given in effervescing draughts, and repeated after every fit of vomiting, two grains having been, however, sometimes given without quelling the sickness. Blisters to the pit of the stomach, dressed in the usual way, or with acetate of morphine, were favourably mentioned; and, as a last resource, Dr. Tilt advised an issue to the pit of the stomach, by which means he was able to check vomiting which had lasted incessantly for a year in a patient who, last winter, was only kept alive by brandy. The issue had been discharging for six months, and still continued to check the sickness, notwithstanding a severe relapse of internal metritis, which had caused this distressing symptom. In another case of chronic inflammation of the womb, vomiting seemed to relieve the still more distressing pains, so Dr. Tilt did not think himself justified in recommending the application of an issue. When nausea was protracted, he urged the necessity of forcing patients to take a few mouthfuls of food repeatedly in the course of the day, as in the sickness of pregnancy; and he advised those who suffered from morning sickness to take a little tea, milk, and rum or brandy on waking or before getting up.—*Med. T. & Gaz.*, Jan. 19, 1861.

49. *Puerperal Vaginitis.*—M. BEAU calls attention to a severe form of vaginitis which is sometimes met with supervening on the puerperal condition, which may readily be mistaken for gonorrhœal discharge. It is not very rare, and the women have some difficulty in getting their account of its origin believed. M. Beau, by careful observation, has satisfied himself of the purely puerperal character of the discharge. Like gonorrhœal discharge, it resists treatment

obstinately. He finds a solution of nitrate of silver to be the best application. *Gaz. des Hôpitaux*, No. 1, 1861.

50. *Disease of the Umbilical Vein.*—Dr. ALEX. R. SIMPSON showed to the Obstetrical Society of Edinburgh (Dec. 21, 1860) a preparation of an umbilical cord, in which the vein was seen to have undergone a peculiar kind of degeneration, such as he had nowhere been able to find described as occurring in this particular vessel, and which gave it the appearance of being affected with atheromatous degeneration of its inner coat. But, on microscopic examination, it was found that the morbid change that had occurred was seated chiefly in the muscular coat, and consisted of a kind of fatty degeneration of the muscular fibres. The child to which the cord belonged had arrived to the end of the fifth month, and was born dead; and its death had perhaps been due to the diseased condition of the umbilical vein, as the placenta had been healthy, and no morbid change could be detected in the body of the fœtus.—*Ed. Med. Journ.*, Dec., 1860.

51. *Pyæmia in an Infant.*—Dr. WILLIAM ZEIGLER related to the Obstetrical Society of Edinburgh the history of a case where pyæmia had sprung up in an infant a few days after birth. The disease seemed to have taken origin in ulceration and inflammation occurring at the umbilicus, immediately after the decedence of the cord.—*Ed. Med. Journ.*, Dec., 1860.

52. *Death after the Injection of an Ovarian Cyst with Iodine.* By Dr. REINHOLD LÖWENHARDT, of Prenzlau.—Mrs. R., forty-one years of age, the mother of several children, menstruating regularly, a midwife, and pursuing her laborious occupation with scarcely any intermission, came under my care two years ago on account of a very considerable enlargement of the abdomen, which did not cause her much inconvenience, and which was found on examination to depend upon the presence of an ovarian tumour. The tumour appeared to consist of a single cyst, to be non-adherent, and to be situated on the left side, although the patient persisted in ascribing its origin to the right side, to which she had repeatedly applied leeches on account of attacks of pain. I punctured the tumour four times, at gradually diminishing intervals, with the effect of giving exit to a clear watery fluid, containing a large proportion of albumen; the more accurate exploration of the abdomen, which was rendered possible on these occasions, confirmed the previous diagnosis. The general condition of the patient during this time remained pretty satisfactory, although unmistakable symptoms of anæmia had presented themselves.

Keeping in view what must be the ultimate result, and believing that this case was in every respect well adapted for being operated upon with a view to a radical cure, I proposed to the patient the employment of iodine injection; considering, from the accounts I had read of it, that it was the least dangerous of the means at my disposal to obtain the desired end.

The patient gave her consent, and, after emptying the bladder and rectum, I chose, as the point of operation, the linea alba, halfway between the navel and the pubis—the same point, in fact, where I had punctured on the former occasions. Any interference per vaginam was rendered impossible by a slight arching forward of the tumour in this situation.

About a fortnight before the menstrual period, I made a puncture with a straight trocar, the canula of which was two inches long, and scarcely two lines in diameter, and into which I introduced a horn tube one line in diameter, which projected an inch and a half beyond the canula. I drew off about ten quarts of fluid, when the belly fell into loose folds, and no more could be got out by pressure. Into the projecting end of the horn tube, I then introduced a syringe, and very slowly injected a fluid consisting of two ounces of tincture of iodine, two ounces of distilled water, and ten grains of iodide of potassium. I only allowed the fluid to remain in the abdomen (which I rubbed gently, to insure the proper application of the injection) for four or five minutes, as, from the entrance of the first drop, there were manifestations of severe pain, coupled with repeated asseverations of a sensation of approaching death. This appeared to me the more extraordinary, as I knew that the patient was not an unusually sensitive person;

nevertheless, consoling myself with what often happens in the case of the injection of hydroceles by the most skilful surgeons, and with certain reminiscences of West's excellent book on Diseases of Women, I allowed the solution of iodine to flow out as completely as possible, and removed the canula.

Even although the countenance remained pale, the sense of impending death continued, the pain did not abate, and the pulse could with difficulty be felt, I ascribed the whole train of symptoms to the temporary depressing influence of shock, gave analeptics, ordered the application of a wet bandage, and hoped for the best.

At the end of the first hour after the operation, however, the alarming symptoms remained in full force; there was repeated vomitings, the extremities were cold and covered with a clammy sweat, and the pulse was almost imperceptible. After effervescing draughts with morphia, slight inhalation of chloroform, and wine had been employed with scarcely any effect; after the patient had complained alternately of violent desire to micturate, of pain in the loins, in the right shoulder, and especially in the abdomen, as well as of a sensation as if the menses were coming on, death occurred at four o'clock in the morning, fourteen hours after the operation. Consciousness was perfect to the last, and it is worthy of remark that the patient, very soon after the operation, had predicted with great decision that she would die at this hour.

The publication of the unfortunate result of the operation seemed to me called for, on the one hand, because the method practised appears to be coming more and more into vogue, and consequently makes accurate statistics desirable; and, on the other, because in the books I have had an opportunity of consulting, I have not found a case in which a simple injection has been followed in so short a time by death, and in which no important anatomical change has been met with to account for the fatal result. In conjunction with Dr. Pahl, I examined the body thirty-six hours after death. Connected with the left ovary was a non-adherent cyst, almost entirely collapsed, attached by a pedicle about the thickness of the finger; the walls were not very thick, there was no trace of inflammation in its interior, and it contained about a pint of a dark-brown fluid containing iodine. The wound made by the trocar was entirely closed; a little bloody effusion surrounded the spot. The right ovary was occupied by three small cysts; the lining membrane of the healthy uterus was decidedly congested, and covered with bloody mucus. In the cavity of the peritoneum was a small quantity of a light-brown fluid, which contained a small quantity of iodine; there was no trace of inflammation of the peritoneum, of the stomach, or of any of the other abdominal organs.

It accordingly appears to me, although I have come to this conclusion with reluctance, that the probable cause of death was the violent shock to the nervous system caused by the injection. That this is often very considerable, is generally recognized, and that it was the cause of death in this case seems to be proved both by the fact that the alarming symptoms which made their appearance at the very beginning of the injection continued till death, and by the absence of any recognizable alteration in the organs involved; that in cases already published, similar symptoms have disappeared after at most a few hours, cannot be used as a counter-argument. Now, as the resorption of the iodine cannot be charged with the unfortunate result, but simply the irritation, at first local and then general, caused by the solution, I intend in future only to employ irritating injections while the patient is under the influence of chloroform, and to keep up this influence, in a moderate degree, for some time after the operation.—*Edinb. Med. Journ.*, Jan. 1861, from *Monatsschrift für Geburtskunde*, Oct. 1860.

53. *Rupture of the Uterus*.—LEHMANN states that of 7000 women delivered in the "Gebäranstalt" at Amsterdam, rupture of the uterus occurred only three times: in private practice he has observed eight cases. In all the rupture took place during labour. It is in the third or fourth stages of labour that the uterine wall gives way, seldom until after the membranes have been broken. The author divides cases of rupture of the uterus into two classes—the spontaneous, and the accidental or mechanical ruptures. Under the first head are included

cases where the cause is a pathological condition of the uterus itself, a degenerated state of the uterine tissue, fibroid formation combined with irregular contraction, or abnormal thinness of the uterus, softening, hysterosomalachia, or thickening of certain parts of the fundus and body combined with great tenuity of the cervix. But rupture may occur when these pathological conditions are absent, as in cases of pelvic deformity, disproportions between the size of the child and the pelvis, transverse presentations, or in cases of hydrocephalus in which the pains are extremely violent. Here, however, the uterine tissue is rarely in a normal condition at the time of the rupture. The author believes that rupture only occurs in such cases when part of the uterine wall has, in consequence of pressure, undergone contusion, softening, detritus, or even gangrene. To the accidental or mechanical class of cases belong those in which the rupture occurs in consequence of operations badly or roughly performed, such as turning, perforation, the use of hooks, tearing of the vagina by splinters of bone, &c. Rupture of the uterus occurs more frequently in multiparæ. The lower segment of the organ is generally the seat of the rupture; the placental surface is not more liable to rupture than other parts; it occurs more frequently on the posterior than the anterior wall; its direction is more generally horizontal than longitudinal; the body may be entirely torn from the neck. The edges of the rupture are jagged, the extent various. There may be simply small perforations in a portion of the uterus, the texture being there softened from continued pressure. The rupture may be complete or incomplete; in the latter case the whole thickness of the uterus is not torn through. The cervix uteri and the vagina are frequently involved in the rupture; more rarely the bladder and rectum. The rupture occurs suddenly, severe pain in the abdomen, followed by cessation of pains, escape of blood from the vagina, fainting, pulselessness, coldness of extremities, &c. The abdomen is changed, the parts of the fœtus can be felt. Emphysema of the cellular tissue the author has not observed. The *prognosis* is very unfavourable, death generally occurring soon after the rupture. In rare cases the fœtus has been retained in the abdomen, as in a case of extra-uterine pregnancy. In reference to the prophylaxis, it is recommended that the uterus be supported from without. As to the treatment, the fœtus is to be extracted as quickly as possible; if it have escaped into the abdomen and is alive, the Cæsarean section is to be performed.—*The New Sydenham Soc. Year Book for 1859, from Mon. Sch. f. Geburtsh. xiii.*

54. *Induction of Premature Labour.*—GERMANN records, as the basis of an elaborate paper, twenty-three cases of the artificial induction of premature labour, out of 339 operations and 1019 labours, the large proportion of which he explains by the unusual frequency of rachitic pelvic distortion in the particular neighbourhood the scene of his observations, and his practice of ascertaining by examination the state of the pelvic diameters during pregnancy. The greater mortality for the children the earlier the operation is performed, is not dependent, he remarks, entirely upon the degree of pelvic contraction, state of development, &c., but is partly explainable by the fact that the presentation is often found defective in operations performed too early. The general indications for the operation, to be gathered from the examination as to the state of the pelvis, are given. The *mode* adopted in the twenty-three cases was various; in only nine of them was one method exclusively adopted, viz., one by the method of Kiwisch, five by that of Cohen, two by that of Braun, and one by that of Scanzoni (mammary). In the other cases, a combination of more than one method was necessary. Cohen's method was employed singly or otherwise in nine cases, and of these nine cases the children were born alive in seven instances.—*Ibid., from Mon. Sch. f. Geb., xii. and xiii.*

55. *Epileptiform Convulsions in the third week of the Puerperal State.*—Dr. UVEDALE WEST reported to the Obstetrical Society of London (Feb. 6, 1861) a case of this. After some premonitory symptoms of headache, stomach and intestinal derangement, a lying-in woman was attacked, towards the end of the second week, with frequent fits of epileptiform convulsions, which persisted throughout the third week. There was slight albuminuria, and the fits themselves were

followed by a condition of hemiplegia more or less persistent, with other alarming symptoms. The patient recovered under the use of remedies directed, in the first instance, to the relief of the gastric and intestinal irritation, which was looked upon as the chief exciting cause of the symptoms observed, and, towards the decline of the disease, to the removal of the anæmic and debilitated condition in which the patient was left. The author concluded with the remark that this case, in which the patient recovered perfectly, and in which, therefore, there could not have been any cerebral mischief, notwithstanding the formidable appearance of the symptoms, shows how little we can rely on symptoms alone in forming a diagnosis in cerebral affections, especially when the case is compared with one, a report of which, by the author, was read before the Society last October, in which latter case, with less formidable symptoms, the patient died in a few hours, and serous effusion in the brain, with other cerebral disease, were found on a post-mortem examination.—*Med. Times and Gaz.*, February 16, 1861.

56. *Puerperal Fever complicated with Diphtheria, in which life was saved by the Tincture of Sesquichloride of Iron.*—MR. ROBERT DRUIT reported the following example of this to the Obstetrical Society of London (Feb. 6, 1861). A healthy woman, aged 42, was attacked on the 7th July, after labour, with shivering and violent pain in the left leg. The pain soon left the leg, and seized the left side below the mamma. There were extreme restlessness, heat of skin, and prostration. On the third day of the illness, violent fetid diarrhœa was established. On the ninth day, the diarrhœa had continued incessantly; the motions thin, reddish, and offensive, passed involuntarily; the patient drowsy, wandering, and picking the bedclothes; when there came on great constriction in the throat, difficulty of swallowing, and copious salivation, together with a diphtheritic exudation at the back of the fauces. The most lavish administration of port wine, brandy, beef-tea, opiates, and quinine, had failed to stay the progress of the malady, and the patient was evidently sinking, when, at the suggestion of Dr. Frere, who was called in consultation, the tincture of sesquichloride of iron was given. The effect was decided and unmistakable. The contents of the alimentary canal were deodorized, and the diarrhœa ceased within twenty-four hours; and, spite of difficulty of swallowing and the throat complication, the patient rapidly became convalescent. The diphtheritic exudation extended over the posterior half of the soft palate. The upper lobe of the left lung was left consolidated; but in the twelvemonth which has since elapsed it has entirely recovered its normal condition. The quantity in which the tincture of steel was administered deserves notice. Three fluidounces of the tincture were swallowed in five days, besides an ounce wasted in lavements. During the first day that it was given, the patient took two fluidrachms every two hours. It is evident that these doses did good, and it is probable that smaller ones might have failed.—*Ibid.*

MEDICAL JURISPRUDENCE AND TOXICOLOGY.

57. *On the Nature of Death from the Administration of Anæsthetics, especially Chloroform and Ether, as observed in Hospitals.*—DR. CHARLES KIDD read an interesting paper on this subject before the Physiological Subsection of the British Association for the Advancement of Science. We are indebted for the following abstract of this paper to the *British Medical Journal* (Sept. 22, 1860):—

From data already collected and tabulated, it appeared not difficult to explain and to offer some new directions as to the nature of death from anæsthetics, a subject of serious importance in medical practice: and the author having collected and tabulated 109 deaths from chloroform and 24 from ether (including 2 from amylene), believes himself in a position to give an explanation of these accidents.

Of these 133 deaths, 90 occurred in male patients; and about half, or 43, were

females, though anæsthetics have been largely used in midwifery practice. Accidents in children, too, have been almost unknown, though chloroform has been used in such cases very extensively; this immunity from danger in diseases of children and women being very marked.

There have been already about from 250,000 to 300,000 operations of various kinds performed under the influence of anæsthetics, chiefly under chloroform, including every variety of serious surgical accident; several hundreds of the patients had been, for such very serious operations as resections of joints, large amputations, ovariectomy and other female cases, ligature of large arteries, etc., more than an hour fully anæsthetized; yet in this large mass of serious surgical operations, there had not been any well attested instance of death from stoppage of the functions of life, or narcotism of the system by the chloroform. On the contrary, fully 80 per cent. of the deaths, and nearly all the deaths from chloroform, had occurred in trivial operations, from very small doses, *suddenly* before the anæsthetic had produced its full effect. It was not sought to be argued (though not very clearly established) that death in the human subject might not be produced by long-continued inhalation of chloroform; it seemed only established that this was not at all the mode in which death had been observed to occur in hospital practice.

The author suspected that, anæsthesia once established in a favourable subject, respiratory action was diminished, and that inhalation or normal respiration continued on a diminished scale also, almost after the manner of hybernation in warm-blooded animals; but, respiration once disturbed, attended by spasm of the glottis through the recurrent laryngeal nerves, especially in strong healthy but nervous subjects, for trivial operations, as tooth-drawing, whitlow, strabismus, caustic to warts, etc., death might occur, and usually had occurred, from a few drops of chloroform. Nevertheless, a law of tolerance of chloroform had been long established in hospitals, that where a patient was labouring under old disease, pain of a neuralgic kind, etc., he bore chloroform best; patients with long continued stertor had taken thus a pint of chloroform in well regulated doses at a single inhalation; one lady had sixty-two ounces in twelve days for intense pain, with good effect; another, two hundred ounces in six months, also with beneficial result!

The best mode of examining the statistics for future use, according to Dr. Kidd, was twofold: first, inductively; and then by comparing certain large groups of facts loosely one with another, and forming other conclusions deductively for application in every-day practice.

Single positive instances, the author thought, did mischief logically in a subject so essentially deductive, as the examination of groups of facts connected with anæsthetics. A single instance in a purely physical science will determine the fixed atomic relations of a salt or oxide, or the diagonal of two forces, or measurements of angles, tangents, or equations, etc.; but in disease and discussions on disease, the single facts are, though positive, so varying and exceptional, the disturbing forces in disease so active and numerous, that we must have recourse to groups of similar facts, or facts tending in a particular direction (statistics), and see how we may reason from what is known to what is still dubious or unknown.

It seemed well established that excitation of certain parts of the excito-motor apparatus in the nervous system produced contraction or spasm of distant parts, such as bloodvessels, muscles, etc.; thus there are spasms of the parts of the neck, laryngismus, trachelismus, spasm of the glottis, and in actual practice under chloroform these are more to be feared than deep insensibility with stertor.

As to the somewhat casual or accidental nature of some of the deaths from chloroform, there was every year additional proof. Of 45 deaths collected by Dr. Snow, he believed five or six were from fright on the operating-table. Chloroform, it might be remembered, has now to bear the obloquy of all the preliminary mischief in operations formerly placed to the account of something else. It was curious that nearly all the deaths happened before operations, and seldom, indeed never, as the result of a long tedious operation. In 85 deaths classified, 17 were operations for hæmorrhoids, fistulæ, etc.; 14 were operations for removal

of toe-nail, necrosed finger-bone, etc.; 10 were cases of tooth-extraction; 11 were operations for removal of tumours and such like; 9 were deaths in minor amputations; 6 were strabismus operations, cysts of eyelids, etc.; 9 were operations on the testis; 5 were reduction of dislocations; 1 was hernia; and 5 were delirium tremens. The broad general fact that, in 300,000 operations of all kinds, chloroform had decidedly saved 6 to 10 per cent. of lives, also tended to show that the cause of death was of a casual nature.

Looking broadly at the facts, it was found by the author that the deaths from chloroform were all sudden, and many of the nature of "fit." Chloroform has a powerful irritant action on the pneumogastric in the lung; and a like irritation by electricity, as now known, stops the action of the heart. Hence syncope may possibly occur, if this irritation or spasm of the pneumogastric and laryngeal nerves be reflected to the heart.

Chloroform, as a general fact, had had a good effect on the general result of operations in surgery. Where it has acted badly, as shown by statistics, it has probably been from a tendency of patients themselves to put off surgical operations too long, changing primary into bad secondary operations. Probably 10 per cent. of lives are still directly saved by chloroform yearly (comparing the present surgical death-rates and those in existence immediately prior to the discovery of anæsthetics in 1846). This showed, according to the author, that the cause of deaths from chloroform must be also of a casual or accidental nature, rather than as arising *ex necessitate rei*. A large number (one-fourth) of the patients also in whom death had occurred had taken chloroform before with perfect safety; one patient, a hundred times. This also corroborated the view that the cause of death is more of a casual nature than due to idiosyncrasy or the general tendency of anæsthetics.

This fact of deaths from chloroform occurring in trivial operations, and early in the administration, or in the stage of excitement, had been remarked by all the chief observers—M. Robert of Paris, M. Denonvilliers, Mr. Paget, Dr. Snow, etc. It was believed to be due to diseased heart; but this opinion had not been borne out by any group of facts; while there was the larger negative clinical fact that hundreds of surgical patients with diseased heart have had chloroform with entire safety; and that, in by far the largest number of cases of death, the heart was healthy, and the symptoms were referable to the brain. Thus, in 18 deaths, the only instances where *post-mortem* or other examination has shown any lesion, 5 deaths were the result of delirium tremens; 8 had cysts in the brain, meningitis, etc.; 2 had chorea; and there were only 3 where traces of diseased heart were suspected or found. Then, of 45 deaths collected by Dr. Snow, he believed 6 were from fright or emotional depression on the operating-table, evinced by intense nervousness; while, as regards the 24 deaths from ether, they seem to have occurred from extreme muscular relaxation, exhaustion, and hemorrhage, favoured by this condition, after, not before, operations.

There had been probably twice as many patients saved from impending death by chloroform by proper restoratives, as the number that have succumbed to its influence. All these restoratives have been directed to excite the brain and reflex system of nerves; and it is to be feared some patients were lost by venesection, used on the theory of fatty or obstructed heart. The author had found that intoxication, or delirium tremens, and hysteria, strongly contraindicate the use of chloroform; and a fact of a very curious kind, belonging to the same category, had been somewhat recently dwelt on in the battles of the Crimea, in India, at Solferino, etc., that all nervous frightened prisoners are acted on with much difficulty, and not without danger, by chloroform; excess of emotion acting like excess of sensation, necessitating very large doses of anæsthetics—a state of "exaltation of sensibility" not very far removed from that which causes spasm of the glottis, trachelismus, etc., in fatal cases. As to delirium tremens, Dr. Snow does not seem to have suspected it; but of 45 deaths, the author collected (independent of suicide cases, all through these researches omitted), he finds 9 cases probably due to intoxication or delirium tremens, though some were put down to other affections or surgical operations.

In ten cases very carefully described by accurate observers, in four there was evident sign of the heart still beating, though the respiration had ceased—one

by Langenbeck; one by Mouatt in the Crimea, the single case of accident there ("death from removal of finger; patient became violent; spasm of larynx occurred; heart's action continued even after pulse and respiration"); and two others; so that it is not impossible that, if this phenomenon were watched, it would be detected more often. It appeared to the author, indeed, that we should never be safe in chloroform administration, if the heart, as he believes it to be, were not one of the *very last parts*, if not the last, to be depressed by chloroform; he rather feared implication of the respiratory tract, in the laryngeal nerves, *nœud vitale*, pneumogastrics, etc., and a disturbance of their delicate balance.

The chief conclusions arrived at were—

1. There is little, if any, benefit in choosing ether in preference to chloroform, as an anæsthetic. In "ether mixtures," the ether is first inhaled pure. Ether is to be especially avoided where we have excessive hemorrhage or muscular relaxation to fear; though in such cases as reduction of dislocations, and in midwifery, it has some points in its favour, but not in the shape of mixture with chloroform.

2. There is less cause of apprehension in administration of anæsthetics in the cases of children and females, and in serious surgical operations, provided the anæsthetic is administered skilfully, than in the cases of men of robust frame, especially if given to intoxicated habits, or the operation be connected with tendinous parts, so often followed by syncope without chloroform.

3. All hospital experience tends to corroborate the view that there is a very remarkable "law of tolerance" of chloroform observable in all bad surgical cases. Once the conjunctiva of the eyelids is insensible, the eyelids closed and normal, anæsthesia kept up with ordinary skill, there is a period of safety, with diminution of respiratory action, established almost like hybernation, but which leaves the heart unaffected. That this, rather than a single instance of death from diseased valves and chloroform; that this, seen in tens of thousands of cases, should encourage the most hopeful views on the diffusion of anæsthetics.

4. Idiosyncrasy has probably very little to do with these accidents, if we omit intoxicated habits, hysteria, tendency to "fits," etc.; so that "trials d'essai," so common hitherto, or placing a patient under several doses of chloroform on previous days to a surgical operation, is a mistake. A patient may have chloroform a hundred times, but die of its effects as surely on the next or hundred and first.

5. There are evidently two modes in which our common anæsthetics may cause death, which require watching; ether, at some distant but uncertain interval during the first twenty-four hours after a surgical operation; but chloroform, like lightning, in an instant, if the inhalation be not stopped. Probably in one half of the latter cases, at least, the heart, as in asphyxia, is still beating; the action being more like an irritant one on the laryngeal recurrent, and pneumogastric nerves; the other cases are instances of syncope, of convulsive fits, etc.

6. In several cases of death—as in delirium tremens cases, to wit—death occurs because ordinary restoratives do not act, as we have an imperfect reflex and nervous system to work with; but, in all cases of impending death, we are justified in at once having recourse to such means, viz., artificial respiration by pressure, as it acts also on the cavities of the heart; tracheotomy, if we have to fear this form of spasm or asphyxia (the engagement of the double respiratory centre of the pneumogastric being very puzzling under chloroform, but rather of the nature of spasm); sudden cold dashing of water, to wake up the respiratory nerves; fresh air fanned on the face, etc.; too much not to be done at first, as the spasm may subside. Acupuncture of the muscles of the neck is also recommended, so as to irritate the spinal accessory, and phrenic, but not the eighth pair of nerves.

7. Hitherto our experience of this spasmodic irritant condition, and our experience of oxygen gas, galvanism, etc., as restoratives, have not been encouraging; these agents being too irritating, and requiring very exquisite skill to apply them, especially galvanism, which hitherto has only, it is to be feared, done mischief. A better restorative seems to be the injection of warm wine into the rectum, or, as tried in animals poisoned by chloroform, transfusion of any simple saline fluid into the veins, as in the analogous collapse of cholera.

58. *On the Use of Oxygen Gas as an Antidote in Cases of Poisoning by Chloroform and Ether.*—M. OZANAM, after numerous experiments upon the lower animals, has arrived at the conclusion that oxygen gas is more efficacious than any other substance in neutralizing the effects of these highly poisonous vapours. The animals upon which he tried this antidote were aroused more promptly by oxygen than by atmospheric air, the difference in the time occupied being in every instance more than one half of the full period.

In other experiments, it was found that when the anæsthetic vapour and the oxygen gas were simultaneously inhaled, the length of time required for rendering the animal insensible, compared with that necessary for accomplishing a similar result when the atmospheric air was inhaled in the place of the oxygen, was as 3 to 1.

According to M. Ozanam these experiments afford a further proof that the various modifications of ether and chloroform act injuriously, in consequence of their being a source of carbon, which is readily assimilated, and which the oxygen disengages from the blood by facilitating its combustion.

This author has established so firm a reputation for the accuracy and value of his researches, that we believe that his suggestion, if properly carried out, will be found useful in preventing a repetition, in many instances, of those occasional disastrous consequences of the inhalation of chloroform which have, of late, made many practical men timid in employing it; oxygen gas can, as our readers are aware, be easily procured, provided the cheap and simple ingredients required for its production are at hand. It should be borne in mind that its efficacy is only apparent, as will be understood upon a slight consideration of the mode of death, in cases in which respiration has not entirely ceased, and not when collapse from a sudden, fatal, cessation of the heart's action has taken place.—*Lond. Med. Rev.*, Sept., 1860.

59. *Rapid Delivery.*—Dr. JAS. C. L. CARSON relates (*Med. Times and Gaz.*, January 26, 1861) the two following cases, which show how necessary it is to be cautious in investigations regarding infanticide:—

"I was called out of bed in a great hurry one night to see Mrs. —. Fortunately I had not far to go. On my arrival I learned that my patient had been roused out of a sound sleep with a feeling of urgent necessity to go to the close-stool. The moment she placed herself on the stool a violent labour-pain came on and the baby was born. Her friends were so much alarmed that they could render no assistance, and the baby would have been drowned in the water had it not been for the timely aid of a sensible lady who happened to be staying in the house that night. She got the patient gently raised to the edge of the stool and then supported the child on her hands till I arrived.

"Cases of this description are worthy of being recorded, as they are of great importance in a medico-legal point of view. I have a very distinct recollection of another instructive case which occurred under my charge when I was residing in one of the Dublin lying-in hospitals in the year 1834. The nurse called me to the bedside of the patient, as the labour was very strong, although she had not been more than half an hour ill. By one violent effort the child and placenta were expelled together, and the waters were not discharged. I tried to rupture the membranes with my fingers, but in vain. They were so unusually thick and tough that I had to open them with the scissors. I am quite confident if this woman had been alone at the time, her child would have been lost."

Another case is recorded by Dr. S. W. POOLE. He states (*Lancet*, Dec. 22) that very lately he was called to a woman who had given birth to a child. On arriving he found her in bed, and a full-grown child (her second) beside her. Twenty minutes before she was standing on the floor, aware that she was about her full time, but having had no warnings of labour. Suddenly she felt "a cramp in the stomach," and the child dropped at her feet. She had no pain after the birth of the child, but Dr. P. found the placenta close to the vulva, though so tightly grasped by the vagina that he had some difficulty in extracting it. There was no hemorrhage from the ruptured funis, and little or no blood about the person of the mother. The woman was young and healthy, and made a rapid recovery.

AMERICAN INTELLIGENCE.

ORIGINAL COMMUNICATIONS.

Gunshot Wound in the Mouth; Secondary Hemorrhage; Ligature of Common Carotid Artery; Recovery. By CHAS. R. S. CURTIS, M. D., of Chicago, Ill.

On the 19th of April, 1857, about 8 o'clock in the evening, I was summoned in haste to a consultation with my friend, Dr. J. H. Ledlie. The history of the case was as follows: The patient, an Irishman, some ten days or two weeks previously, in a drunken row, had been shot with a pistol, the ball entering the mouth, knocked out the lower incisors, and, penetrating the tongue, passed backwards and outwards to the angle of the lower maxillary bone. Striking the bone, it was turned from its course, and, passing directly backwards, buried itself in the deep fibrous tissues of the neck. Dr. B. McVicker, of this city, was sent for, and attended the case. He was, however, unable to remove the ball, and it was left to come away by the suppurative process.

The patient, I believe, appeared to be doing very well up to the afternoon of the 19th, when a sudden and violent hemorrhage took place from the orifice of the wound. The attending physician was immediately sent for, but, as he could not be found, Dr. Ledlie was called in his stead. The doctor, after having adopted a variety of measures to stop the flow, and finding them unavailing, sent for me.

By the time I saw the patient, he had suffered much from loss of blood, and was still bleeding profusely. It was evident from the colour of the blood, and from the fact that the intensity of its flow corresponded with the pulsations of the heart, that the ulcerative process around the ball had penetrated an arterial branch of some magnitude, and, consequently, we could expect little from ordinary styptic remedies. I, however, suggested to the doctor that we should make one more effort to arrest the bleeding, before resorting to operative interference. Accordingly, we proceeded to plug the wound with fine lint dusted with tannin, after the manner of the tampon. The lint was pressed in tight and close as far as to the angle of the inferior maxilla, and apparently at first with perfect success. The hemorrhage was stopped. The patient was cleaned up, and I was about to retire, leaving the patient in the hands of Dr. Ledlie for the night, when he suddenly notified us that the bleeding had recommenced. No further time was to be lost. Our patient was already much reduced. The doctor concurred with me in the opinion that the only alternative left us was to ligate the common carotid artery as soon as possible.

Accordingly, the patient was laid upon his back, with his head depressed and a little inclined to the left, so as to place the right side of the neck in as fair a position for operating as possible (the ball having entered on the right side). I then proceeded to make an incision through the skin and superficial cellular tissue, from a point over the anterior edge of the sterno-cleido-mastoideus and on a level with the pomum Adami, downwards about

two inches towards the sterno-clavicular articulation. The next incision penetrated the platysma hyoides, and exposed the omo-hyoid, the sterno-cleido-mastoid, and a part of the sheath of the vessels. The omo-hyoid was readily pressed to one side, but, owing to an unusual development, some of the fibres of the sternal portion of the sterno-cleido-mastoideus were divided and pressed to one side, so as to fully expose the sheath of the vessels. The decendens noni nerve was observed and avoided. The sheath was carefully raised and opened, and the artery, nerve, and vein were found *in situ*. An aneurism needle was now introduced, armed with a strong ligature, between the artery and vein, and cautiously passed around the vessel, carefully avoiding the pneumogastric nerve. The instrument being now withdrawn, a double knot was applied and the ligature drawn until the inner coat of the artery was distinctly felt to yield. The ends of the ligature were suffered to hang out, the edges of the wound were brought together by the interrupted suture and adhesive plasters, and the tepid water dressing and an anodyne ordered. The operation was followed by no remarkable disturbance of the arterial or cerebral functions. The hemorrhage ceased almost instantaneously, and the patient was left for the night in a very comfortable and quiet condition.

20th. The patient doing nicely. No further hemorrhage; pulse one hundred. Continued the tepid water application. Ordered him to maintain the recumbent posture, to observe a low diet, and to take the following: R.—Liq. potass. citrat. ʒij; aquæ camph. ʒiij; spts. eth. nitrici ʒj; morph. acet. gr. vj; antimon. tart. gr. iv.—M. One teaspoonful to be given every hour.

I now left the patient in the hands of Dr. Ledlie, who continued him on the above treatment, occasionally ordering a mild cathartic. The doctor frequently reported him to me as doing well up to about the end of the second week, when we visited him together and removed the ligature, the wound in the neck having healed kindly by the first intention, except where the ligature passed out. At this time, there was a good deal of inflammation and swelling about the jaw and neck near where the bullet was supposed to be lodged. Suppuration was undoubtedly progressing, and, in order to favour its progress, outward warm fomentations were ordered. These were continued for several days, when we again visited him, and the abscess, having begun to point, was opened with a sharp-pointed bistoury. A good deal of unhealthy pus escaped, but the bullet did not come away for several days. But when it had passed off, the abscess healed kindly, and in a few weeks the patient was able to go about his business as healthy and strong as ever.

Some of the most interesting circumstances in connection with this operation were that it was done in a case of emergency, with but one professional assistant, without the aid of chloroform, and with no other light than what a miserable article of tallow candles could afford.

Strangulated Inguinal Hernia successfully treated by an Enema of Tobacco Decoction. By BEVERLY P. REESE, M. D., of Ford's Depot, Dinwiddie Co., Va.

On the night of the 29th September, 1858, I was summoned in great haste to visit a Mr. V., who, the messenger said, had been taken suddenly ill while at the supper-table and had fallen from his seat, and his wife had sent for me, with urgent solicitations to get to his aid as soon as possible. Upon my arrival, I found the patient lying on the floor, behind the door,

with his pantaloons nearly off, revealing at once the malady. He appeared in such agony that I immediately resorted to the lancet, thinking, from the distended appearance of scrotum, it would be impossible to relieve him before overcoming the muscular contractility of the parts. I bled him nearly to syncope, and then resorted to taxis without success.

Finding him difficult to manage without aid, I immediately ordered a physician who lived near by to be sent for, using during the interval warm baths, nauseants, &c., and taking advantage of position, so far as I was able, to control him by the assistance of his wife. I think I never saw an individual suffer more for the time, it being difficult to keep him from going into the fire, saying he preferred to burn than suffer as he did. In a short while assistance was at hand, and all the manipulations we could use proved to be of no effect. Finally, I inquired if he had a syringe; he answered it was at a near neighbour's. I immediately sent for it, with orders to bring some tobacco with it. With great dispatch it was brought, and I immediately made a tobacco decoction, and gave him an enema of about half an ordinary sized cupful, and placed him in a warm bath. In a few minutes, his lips began to grow pale, with a general relaxation of the muscles of the face. He, thinking that he was dying, requested his children to be brought around him, in order that he might give them a parting farewell. I asked the assistance of the doctor, who took him by the feet and I under the arms and placed him on the floor, where, with little or no difficulty, the hernia was reduced.

I have given this hurried sketch of the case: 1st. As an evidence that relief may often be afforded by remedies at hand, without resorting to the knife and severer and more hazardous treatment. 2d. To show the beneficial effects of the use of tobacco, if we could only control or rather regulate its influence.

Chloroform and Tinct. Opii in Tetanus; Recovery. By JAS. L. ORD, M. D., of Santa Barbara, California.

In your number for October, 1860, there is reported a case of spasms which was cured by the use of chloroform, both externally and internally. The author leaves it to the future to determine whether the internal administration of chloroform is as important in the treatment of spasmodic contractions as analogy had led him to suppose.

The following case came under my care two years since, and was noted in my case-book, which goes to show that chloroform may be used with safety and success in spasmodic diseases of the worst type.

A. F. H., aged about 36, of a robust constitution, sanguine temperament, by occupation a merchant, had, by accident, the prong of a pitchfork thrust through the palm of his left hand about 5 P. M. He called at my office at 8 o'clock the same evening, suffering intense pain in the hand, extending up the arm to the shoulder; at the same time felt a kind of numbness, so much so that he could only move the arm by taking hold of it with the right hand. The jaws were quite stiff, and he could open his mouth only enough to drink. His articulation was also impeded, and he was labouring under considerable nervous excitement. From all the symptoms, he had traumatic tetanus, and there was no time to lose in treating it. I made a mixture of equal parts of tinct. opii and chloroform (3iv āā), and gave him a teaspoonful in half a tumbler of water, and told him to take the same quantity every hour until relieved; then applied a flannel cloth, moistened with the same mixture, to his arm and hand, covered it with

oiled silk to prevent the rapid evaporation, with directions to keep it wet until the pain left. Next morning early I visited him, and was glad to find that his jaws had relaxed to their normal condition, and all the pain in the hand and arm had left. He had taken four teaspoonfuls of the chloroform mixture before he obtained any relief, and did not get to sleep until about 2 o'clock A. M. next morning; had applied the mixture to his arm three times before the pain abated. He appeared almost well, and was but little under the influence of the medicine he had taken. For several days, there was a numbness of the hand and arm, but, by using the mixture at intervals externally, it gradually left him. He entirely recovered before the end of a week from the time he was injured.

Cherry Laurel Water in After-pains. By SAMUEL R. RITTENHOUSE, M. D., of Macungie, Lehigh Co., Pa.

I desire to call the attention of the medical profession to the use of cherry laurel water in those distressing pains to which many women are subject after parturition. In the great majority of cases, so far as my experience goes, this medicine, if given in doses varying from half to a fluidrachm, will produce almost immediate relief. I am in the habit of prescribing the above-named quantity at intervals of half an hour, until it produces its effects. In some instances, the dose requires no repetition. I think it much more prompt and certain in its effect in these cases than opium, and not so objectionable, being free of the constipating and stupefying properties of the latter. Its effects, of course, are to be ascribed to the hydrocyanic acid it contains, so that it is, perhaps, immaterial what form of the latter is used in such cases. The medicine was recommended in these cases by Hufeland.

December 24, 1860.

DOMESTIC SUMMARY.

Diphtheria.—Dr. FRANCIS MINOT gives (*Boston Med. and Surg. Journ.*, March 7, 1861) the following interesting account of the case of Dr. H. W. Adams, who died recently from an attack of diphtheria.

"On Tuesday, Feb. 5th, he [Dr. Adams] left Boston, in company with three other gentlemen, to pass a few days at Cotuit Port, a small village on the South Shore. He was suffering at the time from a heavy cold, and thought the change of air would be of benefit to him. The next day, one of the party, Mr. F. L. Gardner, 19 years old, a student in Harvard College, complained of a sore throat. His disease was considered and treated by Dr. Adams as one of acute tonsillitis, and appears to have been of moderate severity, as the patient was not confined to his bed. He had difficulty and pain in swallowing, swollen tonsils, and a whitish exudation on the fauces. On Sunday, Feb. 10th, Mr. Gardner was apparently better, and walked out, a short time, after breakfast. On returning to the house he expressed a wish to lie down, and Dr. Adams accompanied him to his chamber. He had hardly thrown himself upon the bed when he started up in a paroxysm of suffocation, and fell back dead. Dr. Adams immediately laid him on the floor, and tried to resuscitate him by clearing the throat with his fingers, and by endeavouring to inflate the lungs by blowing into his mouth. A large quantity of matter was removed from the patient's throat and mouth, and it was conjectured that an abscess in the tonsil had burst, and caused suffocation by the pus being inhaled. I may here remark, that this opinion was concurred in by Dr. J. Harpur, of Sandwich, who arrived after the patient's death. During their stay at Cotuit, the party experienced much vicissitude of weather,

the night between the 7th and 8th having been excessively cold, so that they all suffered, with the exception of Mr. Gardner, who was so well protected that he was not aware of its having been particularly cold.

The party returned to Boston on Monday, Feb. 11th, and the next evening Dr. Adams complained of some soreness of the throat, and passed an uncomfortable night, but on Wednesday, Feb. 13th, he was out, and attended the funeral of young Gardner. On his return home, however, he felt so unwell, that he sent for me, desiring me to bring some nitrate of silver and a sponge-probang. I saw him at about 2½ P. M. He was down stairs, walking about the room, rather excited, and the first thing he said was, "I have got young Gardner's disease." The voice was hoarse, the pulse at 108. The right side of the neck was a good deal swollen. Both tonsils were swollen, especially the right, and covered with a grayish-yellow, soft substance. The tongue was moist, and covered with a rather thick, dirty-white coat. The breath was rather offensive. There was no great difficulty in swallowing, and none in breathing. He had already taken some rhubarb, which had operated. He expressed a desire to go out, thinking that a walk would do him good. I persuaded him to go to his chamber, and in view of the circumstances of the case, proposed to ask Dr. Bowditch to see him with me, to which he readily assented. In the mean time, I directed him to take a grain of quinine every hour; strong soup every three hours, and brandy and water occasionally; and to gargle the throat with a solution of chlorate of potash, of the strength of half an ounce to a pint of water.

At 5 P. M., Dr. Bowditch saw him with me. He touched the fauces and epiglottis lightly with a sponge-probang dipped in solution of lunar caustic (thirty grains to the ounce), which was repeated in the evening, by his advice, with a solution of double the strength. He also advised a gargle of salt and vinegar, which, however, the patient found harsh, and did not continue. The treatment before adopted, in other respects, was continued. Dr. Adams was not at all prostrated, was cheerful, and said his throat was better.

Thursday, Feb. 14th.—Patient in bed. Dr. Bowditch saw him with me twice, and continued to do so daily, except on Saturday, 16th, when he saw him but once. To-day, the grayish appearance in the throat appeared extending, except towards the left side, and was invading the soft palate. The external swelling was increased; it was hard and lobulated, and but moderately tender. There was increased difficulty in swallowing. The uvula was large and œdematous, apparently filling up the narrow chink left between the tonsils. The pulse in the morning was at 96; towards evening it fell to 84. The voice generally whispering, but sometimes very hoarse. There was no difficulty of breathing; no nervousness; the patient was cheerful; the aspect and colour of the face were good. He was constantly employed in hawking and expectorating mucus, with a little serum, sometimes bloody. This symptom continued to the last—the favourite position of the patient being with his head over a basin at the side of his bed. During the day, he took a considerable quantity of beef-juice, milk and brandy, with quinine, but the pain in swallowing was evidently increased, and we were apprehensive that he would soon be unable to take nourishment by the mouth. Fearing that the greatly-enlarged uvula might accidentally obstruct the air-passage, by becoming engaged between the tonsils, it was amputated. It resembled a piece of umbilical cord, more than anything else.

Friday, Feb. 15th.—Dr. Adams was unable to swallow at all, during the night. He seemed unwilling to sleep, for fear he should be choked by the great amount of purulent mucus which accumulated in his throat. He however said he passed a comfortable night. Before morning he took some beef-juice and quinine in enema, which was not long retained. He afterwards had a free discharge from the bowels, after which he repeated the enema, with excellent effect, several times. He swallowed nothing during the day. The pulse was rather weaker, regular, and at about 84. There was less swelling outside the jaw on the right side. The right tonsil was much swollen, and with the soft palate adjoining, and the remains of the uvula, was covered with a grayish-white exudation. The breath was slightly offensive. The strength was good, and he was able to rise and walk about the room without effort.

Saturday, Feb. 16th.—The night was about the same as before. The patient

seemed to dread to sleep, and wished to be aroused should he do so. He dozed frequently, but had no continuous sleep. He succeeded in swallowing about half a pint of milk, mixed with bits of ice, during the night. The pulse in the morning was at 84; towards noon it rose to 96. The voice was fainter. There was at times some difficulty of breathing, but no actual dyspnoea. The patient was more restless than before. The throat looked more obstructed, the exudation extending up on the soft palate. At 4 P. M., he was suddenly seized with symptoms of strangling, and, after much effort, ejected a piece of firm, white membrane, five inches long, and one inch broad, which was tubular in two places. It was about the sixteenth of an inch thick, and looked like a piece of white kid. After this there was a coarse râle at times in the throat, but he could swallow with much more ease, and took freely champagne wine and beef-tea. At 7 P. M., he complained of great heat, and had the windows opened. The pulse at this time was at 108. He soon became more comfortable, but feeling anxious that he should have a medical man constantly with him, I requested Dr. Stearns to pass the night in his chamber, provided with instruments to open the trachea, in case symptoms of suffocation should come on. Dr. S. reported that he had a fair night. He raised two more pieces of membrane, smaller than the first, and without difficulty. The respiration was laboured at times, and he once rose and sat by the open window, feeling the want of air. He took about half a bottle of champagne, which he relished, and seemed to need. The pulse varied from 116 to 132. He arose once, and went to the water-closet without assistance. Dr. Stearns left him, apparently comfortable, at a quarter past 7, on the morning of Sunday, Feb. 17th. About twenty minutes afterwards, he suddenly had an attack of strangling, got out of bed, put his hands to his mouth, as if to remove some obstruction, and started for the door of an adjoining room. The nurse who was with him lifted him on the bed, and he immediately expired in the arms of his father, who entered the room at the moment. Dr. Stearns was summoned in haste, and opened the trachea, but although artificial respiration was kept up for some time, it was impossible to resuscitate him.

The following account of the *autopsy* was furnished by Dr. Ellis.

The neck was much swollen. The glottis and epiglottis were swollen and oedematous. A false membrane covered the tonsils and posterior part of the fauces, and extended through the glottis and trachea, to the secondary bronchi, where it terminated in a thin edge. It was somewhat ash-coloured, and perhaps less firm in the fauces than in the trachea. In the latter part it was quite firm, and about the sixteenth of an inch in thickness. It was perforated at the point of the operation, so that the air must have been admitted. Near the rima glottidis was a small shred, loose at one extremity, which might have caused obstruction, and suddenly terminated the life of the patient. The membrane was separated with ease from the subjacent surface, which was slightly reddened, and perhaps rough, but the change was not very marked. On microscopic examination of the false membrane, nothing unusual was observed. In the apex of the left lung were several groups of firm gray granulations. A few of the same were also seen at the right apex. Heart normal. Abdomen not exhibited."

Nitric Acid in Intermittent Fever.—Prof. WM. A. HAMMOND has published (*Maryland and Virginia Med. Journ.*, Feb. 1861) a table showing the results obtained in the treatment of forty-one cases of intermittent fever, at Fort Riley, Kansas Territory, in a period of six weeks in summer; of these forty-one cases, ten were quotidian and thirty-one tertian. Thirty-two cases were treated with the nitric acid and nine with the sulphate of quinine. Of the cases cured by nitric acid, three had previously used quinine without effect, and of those in which quinine had proved successful nitric acid had been employed without benefit in two, and in one other had to be omitted on account of causing nausea, heart-burn, &c.

The average period of treatment before the disease was permanently arrested was the same with each remedy—three days. The nitric acid was uniformly given in doses of ten drops (properly diluted with water) three times per day, the quinine in doses of eight grains three times per day.

Besides the fact that the nitric acid was equally successful with quinine in

arresting the disease, the difference in the cost of the two articles is so greatly in favour of the former substance, as to render it an object of importance to make its curative properties more widely known.

Nitric acid was first used as an anti-periodic by Dr. E. S. Baily, of Indiana. Its peculiar properties were brought to the notice of the profession by Dr. George Mendenhall, in the *Western Lancet* for August, 1854. A notice of the discovery is also contained in the *American Journal of the Medical Sciences* for October, 1854.

Dr. H. further states that he has since "very frequently employed nitric acid in the treatment of intermittent fever, and have rarely been disappointed in my expectations of its curative action. In fact, in simple uncomplicated intermitments, I seldom have occasion to use anything else.

"In cases of enlargement of the spleen, consequent upon frequent attacks of the ague, the remedy in question has, in my hands, proved very advantageous."

Ice-Water in the Treatment of Croup.—Dr. J. A. McFARLAND invites (*Columbus Review*, Dec. 1860) the attention of the profession to the use of ice-water in the treatment of croup. He states that "more than seventeen years since he first employed cold applications in the management of croup. During the treatment of a most violent attack, after the ordinary means had been tried without any apparent benefit, and when fairly puzzled what next to do for the relief of the little sufferer, it occurred to my mind, on placing my hand over the child's throat, that the leading indication was to subdue the burning heat, the result of local inflammatory action, and that nothing could be better for that purpose than ice-water, applied directly over the inflamed parts. This was immediately done; and in ten or fifteen minutes a most wonderful improvement was evident. Never have I witnessed, in my whole professional experience, a more rapid and delightful change, than in this my first trial of ice-water in croup." He has since employed this in at least two hundred cases.

In applying the remedy he uses folds of muslin or linen large enough to cover the whole throat and upper part of the sternum, wet just sufficient to prevent dripping, and which should be well covered with several thicknesses of dry flannel, and the whole secured with a small handkerchief. "When we wish," he says, "to have the applications very cold, it is best to have two wet cloths, using them alternately, as fast as they become warm. This course should be continued till the disease is thoroughly subdued. When treatment is commenced early, a few hours may suffice; in neglected cases, several days are sometimes required. In some cases, water fresh from the well or cistern will be sufficiently cold; but as a general rule, the preference should be given to ice-water."

Treatment of Rattlesnake Bite.—Dr. S. W. MITCHELL has published (*N. A. Med.-Chir. Rev.*, March, 1861) an elaborate paper on this subject. The following is the course of treatment advised. "The disease caused by the venom is," he says, "sometimes so prompt and terrible that it is impossible to rouse the system through the stomach, and this is doubly difficult when vomiting becomes one of the prominent symptoms of the general prostration. Under these circumstances, enemata of brandy may be used, and inhalations of hot alcohol or ether resorted to, in order to re-excite the flagging powers.

"When called to a patient who has been bitten by a rattlesnake, the physician should at once ligate the limb with a *broad* band, as tightly as may be needed to check the circulation, while, wherever it is possible, cups should be also used immediately over the wound. The question of immediate excision or ablation of the part will then be determined by considerations already before the reader (local treatment). Setting these means aside, the iodine treatment, as limiting the local disease, may then be resorted to; but if, as is usually the case, there is no instrument at hand to make possible this treatment, incisions and the actually cauteries are the final resort. Meanwhile, stimulus in some shape should be given, and when the excitement thus obtained is sufficient, the finger should be laid on the pulse and the band loosened. As the system becomes depressed, the ligature is once more to be drawn tighter, and, with continued use of stimulus,

the economy prepared for another dose of the venom, which is thus to be antagonized little by little. Finally, it will be requisite to shift the band higher up the limb, to avoid the too great constriction of the damaged member. The further management of the case, with regard to stimulus, must be left to the physician, who will remember that in most cases of severe poisoning, he has to deal finally with a blood which has lost a part or the whole of its power to coagulate. He may find in the mineral acids, tonics, as quinine, and the continued use of stimulus, the necessary means for carrying his patients through the later stages of the malady."

Creasote as a Local Application in Diphtheria.—A correspondent of the *American Med. Times* (March 9, 1861) calls attention to the efficacy of creasote as a local application in diphtheria. "One or two applications," he says, "remove the fœtor, and its continuance cures the throat." He employs ten or more drops of the creasote in a gill of warm water, as a gargle, or if the patient is too young for that, he applies it with a swab.

Recurrent Sensibility of the Anterior Roots of the Spinal Nerves.—Dr. AUSTIN FLINT, JR., relates (*New Orleans Med. Times*, March, 1861) two experiments performed by him which show that the anterior roots of the spinal nerves are not exclusively motor, but that they possess a certain degree of sensibility; that this sensibility is recurrent, or is derived from the posterior or sensitive roots, because, after the division of these roots, it is immediately lost. In this he has confirmed the experiments of Magendie, in 1822 and 1839, experiments which he failed to repeat with success since that date, which were repeated in 1846 by Bernard, and later still by Schiff.

Traumatic Tetanus; Inoculation with Corroval; Death.—Dr. E. T. MULHOLLAND records (*Maryland and Virginia Med. Journ.*, Jan. 1861) a case of traumatic tetanus in a coloured boy, in which various remedies having been administered without benefit, subcutaneous injection of corroval, at the suggestion of Dr. Hammond, was resorted to, but without producing the least impression on the tetanic spasms.

Prof. Hammond was of the opinion that no benefit is to be expected, in cases of this character, from the use of the corroval, and he supposed that death might have been indirectly hastened by the action of the substance in question upon the tissue of the heart. The mode, however, in which this event took place was sufficient to show that it was not due to the immediate effect of the corroval.

The case, he thought, was likewise instructive, as showing how radical a difference exists between traumatic tetanus and tetanus caused by over-doses of strychnia, and should prevent us forming conclusions as to treatment, applicable to both conditions.

Oxalate of Cerium in the Vomiting of Pregnancy.—Dr. W. H. JONES reports (*Chicago Medical Journ.*, Feb. 1861) five cases in which he employed the oxalate of cerium in the vomiting of pregnancy, and with results entirely in accordance with those obtained by Dr. C. Lee. See his paper in the October No. for 1860 of this Journal.

Tracheotomy in Croup.—Two cases of this, with successful results, are recorded in the *Boston Med. and Surg. Journ.*, Feb. 1861. The first is by Dr. JAS. AYER, of Boston. The subject of the case was a girl 5½ years of age, in whom the disease had run on for several days in spite of the usual remedies. Asphyxia being imminent, the child was etherized, and tracheotomy performed by Dr. Cabot, with instantaneous relief; and under very careful treatment the patient recovered.

The second case occurred in a boy two years and seven months old, and is related by Dr. SAMUEL CABOT, JR. Suffocation being imminent, Dr. C. opened the trachea, and by careful subsequent treatment the child recovered.

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GRADUATES OF JEFFERSON MEDICAL COLLEGE OF PHILADELPHIA,
MARCH, 1861.

At a Public Commencement, held on the 9th of March, 1861, the degree of DOCTOR OF MEDICINE was conferred on the following gentlemen by the Hon. EDWARD KING, LL. D., President of the Institution; after which a Valedictory Address to the Graduates was delivered by Prof. MITCHELL.

NAME.	STATE OR COUNTRY.	SUBJECT OF THESIS.
Abernethy, James M.	North Carolina.	Enteric Fever.
Alger, Luther W.	Massachusetts.	Catarrhal Fever.
Alter, David	Pennsylvania.	Angina Pharyngæ.
Appel, Charles H.	Pennsylvania.	Intermittent Fever.
Armfield, David A.	North Carolina.	Scarlatina.
Atkins, T. W.	Georgia.	Crural Hernia.
Baker, John A. P.	Virginia.	Jaundice.
Balsbaugh, George S.	Illinois.	Scarlet Fever.
Barclay, John	Pennsylvania.	Enteric Fever.
Beckham, Henry C.	Virginia.	Dysentery.
Bell, John B.	Virginia.	Fœtal Circulation.
Bickley, Lloyd Wharton	Pennsylvania.	Inguinal Hernia.
Blackburn, Cary B.	Mississippi.	Hydrocele.
Blalock, N. G.	North Carolina.	Influence of the Mind over the Body.
Bowdon, P. M.	Mississippi.	Tubercular Phthisis.
Bowers, E. D.	Ohio.	Cervical Triangles.
Brothers, Oscar C.	Mississippi.	Acute Pneumonia.
Brown, Ignatius C.	Tennessee.	Pneumonia.
Brown, M. A.	Missouri.	Pinguedo.
Brunner, Frank R.	Pennsylvania.	{ Aims and Duties of the Medical Student. Parturition. Enteric Fever. The Organs of Vision. Dropsy of the Knee.
Buford, Smith	Mississippi.	
Burges, Richard U.	Virginia.	
Bush, Dudley	Kentucky.	
Bushong, Israel	Pennsylvania.	
Chambers, William D.	Kentucky.	Scarlatina.
Clark, Patrick B.	Texas.	Digestion.
Cochran, John L.	Virginia.	{ Connection between Mind and the Cerebral Hemispheres.
Coe, Thomas Upham	Maine.	
Conkwright, Allen H. (M.D.)	Missouri.	Venesection.
Conner, Phineas S.	Ohio.	Semeiology of Phthisis.
Cook, George R.	Florida.	General Paralysis of the Insane.
Covington, Thomas H.	Kentucky.	Inflammation.
Crossley, George W.	Illinois.	Gonorrhœa.
Culver, Lucius Pitt	Ohio.	Professional Distinction.
		{ Therapeutic Properties of Tartar Emetic.
Daniel, Thomas W.	Virginia.	Anæsthetics.
Davies, William H.	Virginia.	Pneumonia.
Davis, Nicholas E.	Virginia.	Angina Pectoris.
Delaney, William T.	Tennessee.	Diagnosis.
Duff, Edmund	Pennsylvania.	Diarrhœa.
Duke, James E.	Virginia.	Circulation of the Blood.
Dupuy, Joseph T.	Virginia.	Hectic Fever.
Dye, Henry	Texas.	Typhoid Fever.
Earnest, J. Tate	Tennessee.	Typhoid Fever.
Eaves, Spencer	North Carolina.	Laryngo-tracheitis.
Elder, Samuel M.	Pennsylvania.	Inflammation.

NAME.	STATE OR COUNTRY.	SUBJECT OF THESIS.
Embree, John W. (M. D.)	Texas.	Scurvy.
Engelman, Joseph P.	Pennsylvania.	Cholera Infantum.
Eves, Abram E.	Delaware.	Hygiene of the Sick-room.
Ewing, W. D.	Virginia.	Scarlet Fever.
Farley, John C.	Alabama.	Physical Signs of Pleuritis.
Finney, Edward B.	Virginia.	Childbed Fever.
Flint, John Felix	Pennsylvania.	Chagres Fever.
Fox, Addison C.	Virginia.	Our Profession.
Frantz, John H.	Maryland.	{ Ether and Chloroform as Anæsthetic Agents in Surgery.
Fulkerson, Albert P.	Missouri.	Acute Gastritis.
Gamble, Hamilton M.	Virginia.	Ilio-femoral Luxations.
Garrett, Lewis T.	Pennsylvania.	Scarlatina.
Gidney, J. Chauncey	North Carolina.	Dysentery.
Gilman, Uriah	Arkansas.	Cathartics.
Goolrick, P., Jr.	Virginia.	Medical Science.
Grant, John (M. D.)	Canada West.	Pneumonia.
Greene, Lorenzo S. S.	Alabama.	{ Oxygen as a Chemico-physical Agent.
Gross, Chester L.	Pennsylvania.	Signs of Pregnancy.
Hammond, Alfred F.	North Carolina.	Bilious Fever.
Hanger, Cornelius	Missouri.	Malaria.
Hanly, John A. C.	Pennsylvania.	Typhoid Fever.
Harrill, Lawson	North Carolina.	Laryngismus Stridulus.
Haughton, Rich'd E. (M.D.)	Indiana.	Diphtheria.
Haupt, Frederick L.	Pennsylvania.	Typhoid Fever.
Hawkins, John W.	Missouri.	The Mind in Disease.
Henry, David H.	Indiana.	Mania-a-potu.
Herr, Ambrose J.	Pennsylvania.	Chorea.
Herrick, Geo. H. Webster	New Hampshire.	Death.
Hetzell, David G.	New Jersey.	{ Anatomy and Physiology of the Skin.
Higgins, Samuel J.	Georgia.	Scarlatina.
Hill, Lauriston H.	North Carolina.	Tobacco.
Hitch, William S.	Delaware.	Enteric Fever.
Horton, Samuel M.	Pennsylvania.	Germinal Spots.
Howard, Thomas Henry	Virginia.	Intermittent Fever.
Hugg, Joseph	New Jersey.	Dysentery.
Jones, E. W.	Alabama.	Influenza.
Jones, La Fayette J.	Virginia.	Menstruation.
Keely, Thomas J.	Pennsylvania.	Pseudo-membranous Angina.
Keene, Robert Wilson	Kentucky.	Marriages of Consanguinity.
King, William Coffield	Tennessee.	Enteric Fever.
Knott, Thomas M.	Kentucky.	Inflammation.
Kuykendall, William C.	Mississippi.	Dysentery.
Lane, Sidney W.	Maryland.	Diphthêrite.
Lea, John G.	North Carolina.	Diphtheria.
Lineaweaver, John K.	Pennsylvania.	Dysentery.
Logan, P. W.	Kentucky.	Clinical Examination of Patients.
Lowry, Squire M.	Kentucky.	The M. D.'s Mission.
Marley, H. B.	North Carolina.	Gunshot Wounds.
Marsh, Joseph W.	Delaware.	Typhoid Fever.
Martin, James	Ohio.	Pseudo-membranous Croup.
Massie, James W. (M. D.)	Kentucky.	Counterirritation.
Mathis, Aylesbury	Georgia.	Cyanosis Neonatorum.
McClarty, Hugh G.	Texas.	Gunshot Wounds.

NAME.	STATE OR COUNTRY.	SUBJECT OF THESIS.
McGee, J. P.	Tennessee.	Retrospect of Medicine.
Metheny, David	Pennsylvania.	The Physician.
Michler, William H. H.	Pennsylvania.	Tussis Convulsiva.
Miller, Victor Davis	Pennsylvania.	Veratrum Viride.
Milloy, John	Mississippi.	Enteric Fever.
Miner, James	Illinois.	Enteric Fever.
Montanye, Lester de la	Pennsylvania.	Tubercular Phthisis.
Morley, J. R.	Tennessee.	Typhoid Fever.
Morris, John	Ohio.	Gonozæmia.
Morrison, Joseph B.	Pennsylvania.	Dropsy.
Moses, Thomas Freeman	Maine.	Modes of Death.
Mosley, Benjamin J.	Georgia.	Sunstroke.
Munford, Samuel E.	Indiana.	{ Chemistry applied to Therapeutics and Forensic Medicine.
Nall, Burr F.	Kentucky.	Phthisis Pulmonalis.
Nicholson, John (M. D.)	Pennsylvania.	Phthisis Pulmonalis.
Noble, James D.	Pennsylvania.	Anæsthetics.
Norman, John P.	Pennsylvania.	Bronchitis.
Notson, William Morrow	Pennsylvania.	Diphtheritis.
Nottingham, Severn P.	Virginia.	Delirium Tremens.
O'Bryan, William R.	Kentucky.	Post-partum Hemorrhage.
Oldmixon, George Scott	Pennsylvania.	Diagnosis of Inguinal Hernia.
Parker, D. Reid	North Carolina.	Intermittent Fever.
Parker, William H. H.	Virginia.	Hydrophobia.
Pease, Loren H.	Connecticut.	Sumach.
Picot, Mitchell H.	Pennsylvania.	Typhoid Fever.
Pomerene, Peter P.	Ohio.	Diphtheria.
Powell, H. B.	Ohio.	Typhoid Fever.
Price, William Frederick	Virginia.	Typhoid Fever.
Pulliam, John D.	Virginia.	Digestion.
Pyatt, E. A.	Tennessee.	Scrofula.
Quarterman, Keith A.	Georgia.	Intermittent Fever.
Ragsdale, Joseph	Missouri.	Typhoid Fever.
Rhinehart, Alexander K.	Georgia.	Puerperal Convulsions.
Rhoads, Thomas J. B.	Pennsylvania.	Axillary Dislocations.
Rice, Albert R.	New York.	Retroversion of the Uterus.
Richey, James A.	Pennsylvania.	Diuretics.
Riggs, David W.	Pennsylvania.	Scarlet Fever.
Ritter, Nathaniel F.	Pennsylvania.	Typhoid Fever.
Robbins, Joseph	Illinois.	Periodicity in Disease.
Roberts, William H.	Indiana.	Pneumonia.
Robertson, A. T.	Arkansas.	Intermittent Fever.
Robertson, T. L.	Alabama.	Menstruation.
Roller, William C.	Pennsylvania.	Phenomena of Inflammation.
Ross, Elijah W.	Pennsylvania.	Chronic Cystitis.
Rugeley, Henry L.	Texas.	Abdominal Dropsy.
Sanders, William H.	Alabama.	Inguinal Hernia.
Satterfield, Benjamin F.	Missouri.	Syphilis.
Saunders, John B.	Kentucky.	{ Pathology and Treatment of Scarla- tina.
Savage, William E. F.	Virginia.	Laryngo-tracheitis.
Seargeant, Henry H.	Virginia.	The Signs of Pregnancy.
Senseny, William D.	Pennsylvania.	Diphtheria.
Shackelford, William	Kentucky.	Hydrocele.
Shackleford, James	Kentucky.	Syphilis.
Shankle, E. A.	Georgia.	Scarlatina.
Short, Wesley	Indiana.	Milk Sickness.

NAME.	STATE OR COUNTRY.	SUBJECT OF THESIS.
Simmons, Thomas W.	Maryland.	Diphtheria.
Simpson, Joseph Hawkins	North Carolina.	Typhoid Fever.
Slough, G. B.	Pennsylvania.	Physic, its true Nobility.
Smoot, John H.	Virginia.	Opium.
Spencer, William Canfield	New York.	Investigation in Diagnosis.
Stewart, Samuel Shaw	Pennsylvania.	Polygala Senega.
Stowe, Charles H.	Pennsylvania.	Aneurisms.
Thomas, Richard C.	Kentucky.	The Kidney.
Thompson, Thomas C.	Texas.	Pulmonitis.
Thompson, William B.	Pennsylvania.	{ Laws which govern the Strength of a Galvanic Current.
Thomson, James W.	Pennsylvania.	
Totten, John Baldwin	Ohio.	Variola.
Trego, Albert	Pennsylvania.	Fœtal Circulation.
Tyson, Andrew R.	Pennsylvania.	Diphtheria.
		Placenta Prævia.
Vaughan, Joseph H.	Virginia.	Chorea.
Wailes, Leonard A.	Mississippi.	Strabismus.
Wallis, Walter	Maryland.	Pneumonia.
Warren, Francis G. (M. D.)	Maine.	Enteric Fever.
Washington, James S.	Arkansas.	Auscultation and Percussion.
Way, William Henry	Georgia.	Bilious Remittent Fever.
Webb, Charles E.	Virginia.	Erysipelas.
Weist, J. R.	Ohio.	Duties of the Physician.
Welch, Stanton A.	Pennsylvania.	Retroversion of the Uterus.
White, Horace M.	Pennsylvania.	Scrotal Hernia.
Williams, Emmet	Mississippi.	Hydrophobia.
Williams, Junius S.	North Carolina.	Circulation of the Blood.
Willson, John	Pennsylvania.	Antimonial Powder.
Wilson, Lucien Strain	Georgia.	Diphtheria.
Wilson, W. Stockton	Maryland.	The Health of our Women.
Windle, Isaac	Iowa.	Inflammation of the Cervix Uteri.
Wood, J. Bestor	Alabama.	Menstruation.
Woolsey, William F.	Pennsylvania.	Diphtheria.
Wortham, John B.	Alabama.	Dysentery.
Wymond, Richmond	Indiana.	Diphtheria.

Of the above, there are from—

Pennsylvania	47	Illinois	4
Virginia	26	Arkansas	3
Kentucky	14	Delaware	3
North Carolina	13	Maine	3
Georgia	9	New York	2
Ohio	9	New Jersey	2
Mississippi	8	New Hampshire	1
Alabama	7	Florida	1
Tennessee	7	Connecticut	1
Missouri	7	Iowa	1
Texas	6	Massachusetts	1
Indiana	6	Canada West	1
Maryland	5		

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GRADUATES OF THE UNIVERSITY OF PENNSYLVANIA, 1860.

At a Public Commencement, held March 14th, 1861, in the Musical Fund Hall, the Degree of Doctor of Medicine was conferred by Rev. DANIEL R. GOODWIN, D.D., Provost, upon the following gentlemen; after which an Address was delivered by ROBERT E. ROGERS, M. D., Professor of Chemistry.

NAME.	TOWN OR P. O.	COUNTY.	STATE.	SUBJECT OF THESIS.
Allen, Harrison	Philadelphia,		Pa.	Entozoa Hominis.
Alexander, Octavius	Lexington,	Lafayette,	Mo.	Life of the Blood.
Amiss, Thos. B.	Slate Mills,	Rappahannock,	Va.	Typhoid Fever.
Armstrong, N. E.	Hallsville,	Duplin,	N. C.	Pleuritis.
Armstrong, James A.	Philadelphia,		Pa.	Children and their Diseases
Ashhurst, Samuel	Philadelphia,		Pa.	Periodical Dyspnœa.
Bacon, Charles P.	Roaring Spring,	Trigg,	Ky.	Causa Excitans Parturitiones.
Bacon, Thos. L. (M.D.)	Mayfield,	Graves,	Ky.	Milk Sickness.
Baldwin, C. (M.D.)	Winchester,	Frederick,	Va.	Signs of Pregnancy.
Bellows, Horace M.	Philadelphia,		Pa.	Wounds of the Abdomen.
Beshler, J. B.	Berrysburg,	Dauphin,	Pa.	Diphtheria.
Birkey, Henry W.	Philadelphia,		Pa.	Fractures.
Bonner, Robt. J.	Raymond,	Hinds,	Miss.	Yellow Fever.
Booth, E. G., Jr.	Wellville,	Nottoway,	Va.	Diagnosis and Treatment of Enteric Fever.
Bowen, Jno. B.	Bridgton,	Cumberland,	N. J.	Belladonna.
Brengle, Wm. D.	Frederick City,	Frederick,	Md.	Pseudarthrosis.
Breneman, E de W.	Lancaster,	Lancaster,	Pa.	Osteosis.
Bridges, John H.	Taylorstown,	Sunbury,	N. Brunswick,	Croup.
Brown, Jas. W. (M.D.)	Amherst C. H.,	Amherst,	Va.	Gleet.
Brown, A. M.	Cincinnati,	Hamilton,	Ohio.	Correlation of Forces.
Bunn, Wm. H.	Philadelphia,		Pa.	Acute Tetanus.
Cadwalader, Chas. E.	Philadelphia,		Pa.	Dyspepsia.
Cameron, Hugh	Antigonishe,	Sydney, Nova Scotia,		Pleurisy.
Carter, C. Shirley	Millwood,	Clark,	Va.	Pneumonia.
Casper, Thos. J.	Salem,	Salem,	N. J.	Hydrargyrum.
Chandler, J. T.	Oxford,	La Fayette,	Miss.	Yellow Fever.
Cheatwood, L. N.	Lynchburg,	Campbell,	Va.	Labour.
Cheek, B. A.	Warrenton,	Warren,	N. C.	The Pulse as a Diagnostic Symptom.
Church, Wm.	Meadville,	Crawford,	Pa.	Monomania.
Clark, Dougan	Westfield,	Hamilton,	Ind.	Parturition and its Management.
Clark, R. Woodhull	Philadelphia,		Pa.	Signs of Pregnancy.
Clemens, Henry S.	Springtown,	Bucks,	Pa.	Pneumonia.
Cobb, Wm. H. H.	Goldsboro',	Wayne,	N. C.	Lobelia Inflata.
Collins, Wm. A.	Vicksburg,	Warren,	Miss.	Phenomena of Inflammation.
Craighill, Edw. A.	Charlestown,	Jefferson,	Va.	Yellow Fever.
Crain, Wm. Baker	Warren,	Herkimer,	N. Y.	Croup.
Cruice, John J.	Philadelphia,		Pa.	Puerperal Convulsions.
Darrach, Wm., Jr.	Philadelphia,		Pa.	Diphtheria.
Davis, John I.	Buckeystown,	Frederick,	Md.	Delirium Tremens.
Davis, J. D.	Prairieville,	Pike,	Mo.	Jaundice.
Detwiler, S. B.	West Earl,	Lancaster,	Pa.	The Character and Duty of a Physician.
Dodson, M. Emmet	Greensborough,	Guilford,	N. C.	Puerperal Fever.

NAME.	TOWN OR P. O.	COUNTY.	STATE.	SUBJECT OF THESIS.
Dungan, D. H.	Thompson,	Williamson,	Tenn.	Topography & Diseases of vicinity of Thompson, with remarks on Infantile Tetanus and Synchooid Fever.
Dunlap, F. S.	Delaware City,	New Castle,	Del.	Experiments on the effects of penetrating Wounds of the Chest.
Ellis, Geo. W.	Philadelphia,		Pa.	Odontalgia.
Ellis, Chas. M.	Elkton,	Cecil,	Md.	Diphtheria.
Evans, James	Pine Bluffs,	Jefferson,	Ark.	Dyspepsia.
Eversfield, W. Octavus	Bladensburg,	Pr. George,	Md.	Malaria.
Ford, Chas. M.	Troy,	Rensselaer,	N. Y.	Scarlatina.
Forwood, Wm. H.	Chester,	Delaware,	Pa.	Differential Diagnosis of the Exanthemata.
Fowler, Geo. H.	Mobile,	Mobile,	Ala.	Parturition.
Frick, A. P.	Neffsville,	Lancaster,	Pa.	The Twofold Nature of Man.
Fuller, Junius T.	Berea,	Granville,	N. C.	Enteric Fever.
Fulton, J. Harvey	Elkton,	Cecil,	Md.	Labiata and Pycnanthemum Linifolium.
Gardner, T. Sobieski	Hollidaysburg,	Blair,	Pa.	The Venereal Disease.
Garsia, W. Marston	Valparaiso,	South America,	Medico-Chemical Analysis of the Urine.	
Ginkinger, W. H. H.	Allentown,	Lehigh,	Pa.	Enteric Fever.
Gleason, M. K.	Summit,	Cambria,	Pa.	Sulphate of Quinia.
Gotwald, J. H.	Cincinnati,	Hamilton,	Ohio,	Mucous Membrane Anatomically, Physiologically, and Pathologically Considered.
Gregory, F. R.	Sassafras Fork,	Granville,	N. C.	Semeiology of Pregnancy.
Grier, J. H.	Elizabeth,	Union,	N. J.	Remittent Fever.
Haney, J. Reigel	Doylestown,	Bucks,	Pa.	Acute Pneumonitis.
Hardaway, Daniel H.	Nottaway C. H.,	Nottaway,	Va.	Hydrophobia.
Harris, J. O.	Washington,		D. C.	Delirium Tremens.
Harris, R. B. P.	Milldale,	Warren,	Miss.	Acute Dysentery.
Hawkins, A. W. H.	Clarksville,	Washington,	Pa.	Dyspepsia.
Hay, Thomas	Philadelphia,		Pa.	Acute Pleurisy.
Henderson, N. P.	Franktown,	Northampton,	Va.	Croup.
Herron, Jas S.	Washington,	Escambia,	Fla.	Typhus Icterodes.
Hersom, Nahum A.	North Lebanon,	York,	Maine,	Hereditary Transmission.
Hicks, J. H.	Faisons,	Duplin,	N. C.	Acute Pneumonia.
Higgins, J. Mitchell	Lambertsville,	Hunterdon,	N. J.	Duties of the Accoucheur in Natural Labour.
Holt, W. A.	Graham,	Alamance,	N. C.	Cholera Infantum.
Hooks, J. F.	Boston,	Bowie,	Texas,	Sleep.
Horn, Geo. H.	Philadelphia,		Pa.	Sprains.
Hunter, H. T.	Kempersville,	Pr. Anne,	Va.	Typhoid Fever.
Hurst, M. W.	Goodville,	Lancaster,	Pa.	Typhoid Fever.
Illig, A. F.	Stouchburg,	Berks,	Pa.	Strabismus.
Jamar, J. H.	Elkton,	Cecil,	Md.	Gelsenium Sempervirens.
James, Ed. C.	Courtland,	Lawrence,	Ala.	Pneumonia.
Johnson, J. T.	Happy Home,	Burke,	N. C.	Duty of a Medical Student.
Joyce, E.	Baltimore,		Md.	Dyspepsia or Indigestion.
Knight, Jas. S.	Camden,	Kent,	Del.	Sunlight.
Knight, M. D.	Montg Square,	Montgomery,	Pa.	Hæmatemesis.
Kohler, John P.	White Hall Station,	Lehigh,	Pa.	Scarlatina.

NAME.	TOWN OR P. O.	COUNTY.	STATE.	SUBJECT OF THESIS.
Landis, H., Jr.	Lancaster,		Pa.	Hereditary transmission of Disease.
Leete, J. M.	Calhoun Depot,	Madison,	Miss.	Parenchymatitis Renalis.
Lesesne, Chas.	Elizabethtown,	Bladen,	N. C.	Pneumonia.
Levan, Jeremiah R.	Kutztown,	Berks,	Pa.	Pneumonia.
Love, W. W.	Quincy,	Gadsden,	Fla.	Physical, Chemical, and Structural Characters of the Blood.
Marcy, Alex.	Cape Island,	Cape May,	N. J.	The Effects of Strychnia locally upon Inflammation.
Marriott, Henry	West River,	A. Arundel,	Md.	Diabetes.
Mays, George	Schafferstown,	Lebanon,	Pa.	The Blood.
McCausland, R. K.	Pt. Pleasant,	Mason,	Va.	Diphtheritis.
McCloud, H. C.	Robson's Landing,	Coahoma,	Miss.	Erysipelas.
McGill, John	Clio,	Marlboro',	S. C.	Typhoid, or Enteric Fever.
McGill, Geo. M.	Princeton,	Mercer,	N. J.	The Liver.
McPherson, Samuel M.	Lewisburg,	Greenbrier,	Va.	Diphtheria.
Meade, H. B.	Chula,	Amelia,	Va.	Man and Woman.
Medlin, P. P.	Eagle Rock,	Wake,	N. C.	Remittent Fever.
Mentzer, Wm.	Carlisle,	Cumberland,	Pa.	Veratrum Viride.
Meredith, J. Q. A.	Pughtown,	Chester,	Pa.	Wounds.
Millar, Robt.	Providence,	Providence,	R. I.	Diphtheria.
Miller, Jacob A.	Lancaster,	Lancaster,	Pa.	Bloodletting
Miller, Jesse	Smithfield Station,	Mahoning,	Ohio.	Pleuritis.
Mitchell, Geo. H.	Philadelphia,		Pa.	Phenomena of Psychological Changes.
Mitchell, Goodrich	Warrenton,	Fauquier,	Va.	Mercury.
Morrison, T. S.	Coatesville,	Chester,	Pa.	Scarlatina.
Morrison, W. Clingan	Philadelphia,		Pa.	Enteric Fever.
Morrison, J. A. (M. D.)	Philadelphia,		Pa.	
Murphy, Robt. R.	Unionville,	Union,	S. C.	Intermittent Fever.
Nassau, R. Hamill	Lawrenceville,	Mercer,	N. J.	De Officiis Adipis.
Noble, P. H. C.	Morven,	Amelia,	Va.	Typhoid Fever.
Norris, Wm. F.	Philadelphia,		Pa.	Generation and Development.
Nowland, Jas. A.	New Castle,		Del.	Hysteria.
Nuttall, J. H.	Memphis,	Shelby,	Tenn.	Diphtheria.
Oakley, Thos. A.	Leasburg,	Caswell,	N. C.	Abortion.
O'Neill, James P.	Philadelphia,		Pa.	Cathartica.
Opie, Thomas	Staunton,	Augusta,	Va.	Opium.
Page, R. P. (M. D.)		Clarke,	Va.	Hearing.
Parry, H. Chester	Pottsville,	Schuylkill,	Pa.	Opium.
Paullin, Geo. M.	West Philada.,		Pa.	Inflammation.
Perkins, Edw. W.	Malonesville,	Dinwiddie,	Va.	Intermittent Fever.
Pulliam, A. B.	La Grange,	Fayette,	Tenn.	Gonorrhœa.
Ramsey, J. Sylvester	Bloomsburg,	Columbia,	Pa.	Diabetes Mellitus.
Reeder, Silas Andrews	Doylestown,	Bucks,	Pa.	Intermittent Fever.
Reid, Thos. J.	Macon,	Fayette,	Tenn.	Anæsthesia in Parturition.
Rewalt, Luther L.	Middletown,	Dauphin,	Pa.	Yellow Fever.
Robbins, Harry A.	Washington,		D. C.	The Uterus.
Robertson, J. B.	Creachville,	Johnston,	N. C.	Enteric Fever.
Robertson, S. D.	Brownsville,	Hinds,	Miss.	Pneumonia.
Robinson, Henry	Laurens C. H.	Laurens Dist.,	S. C.	Etiological and Therapeutical Effects of Emotions and Passions.

NAME.	TOWN OR P. O.	COUNTY.	STATE.	SUBJECT OF THESIS.
Roe, Jos. B.	Philadelphia,		Pa.	Puerperal Peritonitis.
Rogers, M. L.	Knoxville,	Knox,	Tenn.	Erysipelas.
Rupp, Harry B.	Mechanicsb'g,	Cumberland,	Pa.	Licentiousness—Its moral and Physical Effects.
Russ, Simpson	White Creek,	Bladen,	N. C.	Acute Pneumonia.
Savery, Wm.	Philadelphia,		Pa.	Abortion.
Scull, W. J.	Marianna,	Jackson,	Fla.	Pernicious Fever.
Seawell, L. H.	Marion,	Perry,	Ala.	Dysentery.
Shank, John H.	Lebanon,	Lebanon,	Pa.	Rheumatic Pericarditis.
Shepherd, Cornelius, Jr.	Doylestown,	Bucks,	Pa.	Bile and its Uses.
Shimer, Jacob C.	Easton,	Northampton,	Pa.	The Physical Education of the Female.
Sloan, Chas. J.	Brownsville,	Fayette,	Pa.	Delirium Tremens.
Smith, Wm. F.	Chanceford,	York,	Pa.	Gastronomy.
Sowerby, J. John	Holmesburg,	Philadelphia,	Pa.	Phthisis.
Spear, John C.	Middletown,	New Castle,	Del.	The Blood.
Stedman, H. C.	Milton,	Northumber'd,	Pa.	Signs of Pregnancy.
Stephens, W. G.	Leasburg,	Caswell,	N. C.	Scarlatina.
Stephenson, J. M.	Parkersburg,	Wood,	Va.	Chancere.
Stovall, J. B. (M. D.)	Sassafras Fo'k,	Granville,	N. C.	Scarlatina.
Strickler, M. B.	Carlisle,	Cumberland,	Pa.	Chorea.
Stroud, P. Van Buren	Parkesburg,	Chester,	Pa.	Typhoid, or Enteric Fever.
Swasey, Chas. E.	Milton Mills,	Strafford,	N. H.	Major Exanthemata.
Talbot, John W.	Boston,	Bowie,	Texas,	Puerperal Fever.
Timms, C. W.	Buffalo,	Putnam,	Va.	Pleurisy.
Trau, Adam	Philadelphia,		Pa.	Nephrolithiasis.
Tryon, J. Rufus	New York,		N. Y.	Excretion.
Vallarino, B.	Panama,	Panama,	S. Am.	Dyspepsia.
Voorhies, Charles A.	Easton,	Northampton,	Pa.	The Duties of a Young Physician as essential to his Progress in the Profession.
Wager, P.	Mont. Square,	Montgomery,		Scorbutus.
Wayt, Newton	Staunton,	Augusta,	Va.	Pneumonia.
Wales, Philip S. (M. D.)	U. S. N.			Dysentery.
Ware, John J.	Brownsville,	Haywood,	Tenn.	Remittent Fever.
Watford, Wm. B.	Pitch Landing,	Hertford,	N. C.	Portal Circulation.
Watson, Wm.	Bedford,	Bedford,	Pa.	Enteric Fever.
Weaver, John D.	Danville,	Montour,	Pa.	Diseases of Pregnancy.
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Yarrow, H. C.	Philadelphia,		Pa.	Urine and Urinary Deposits.
Young, John	Philadelphia,		Pa.	Variola.

The Lectures of the ensuing Session of 1861-62 will begin on the second Monday of October and close on the first of March.

HARVARD UNIVERSITY.

SUMMER SESSION OF THE MEDICAL DEPARTMENT.

THE Annual Course of Summer Instruction in the Medical Department of Harvard University will commence at the Massachusetts Medical College, in North Grove Street, Boston, on Monday, March 11, 1861, and continue till November.

Clinical Medical and Surgical Instruction will be given at the Massachusetts General Hospital, adjoining the College.

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DAVID W. CHEEVER, M. D., Demonstrator and Assistant in Surgery.

WM. E. COALE, M. D.,	Assistant in Theory and Practice.
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CALVIN ELLIS, M. D.,	Assistant in Morbid Anatomy.
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E. OLIVER FITCH, M. D.,	Assistant in Materia Medica.

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D. HUMPHREYS STORER, Dean of the Faculty,

Jan. 1, 1861.—2t

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FACULTY.

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WILLIAM E. A. AIKIN, M. D.,	Professor of Chemistry and Pharmacy.
SAMUEL CHEW, M. D.,	Professor of Principles and Practice of Medicine, and of Clinical Medicine.
G. W. MILTENBERGER, M. D.,	Professor of Obstetrics and the Diseases of Women and Children.
WILLIAM A. HAMMOND, M. D.,	Professor of Anatomy and Physiology.
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UNIVERSITY OF MARYLAND—CONTINUED.

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